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Effort-based decision-making and cognitive fatigue

Edited by
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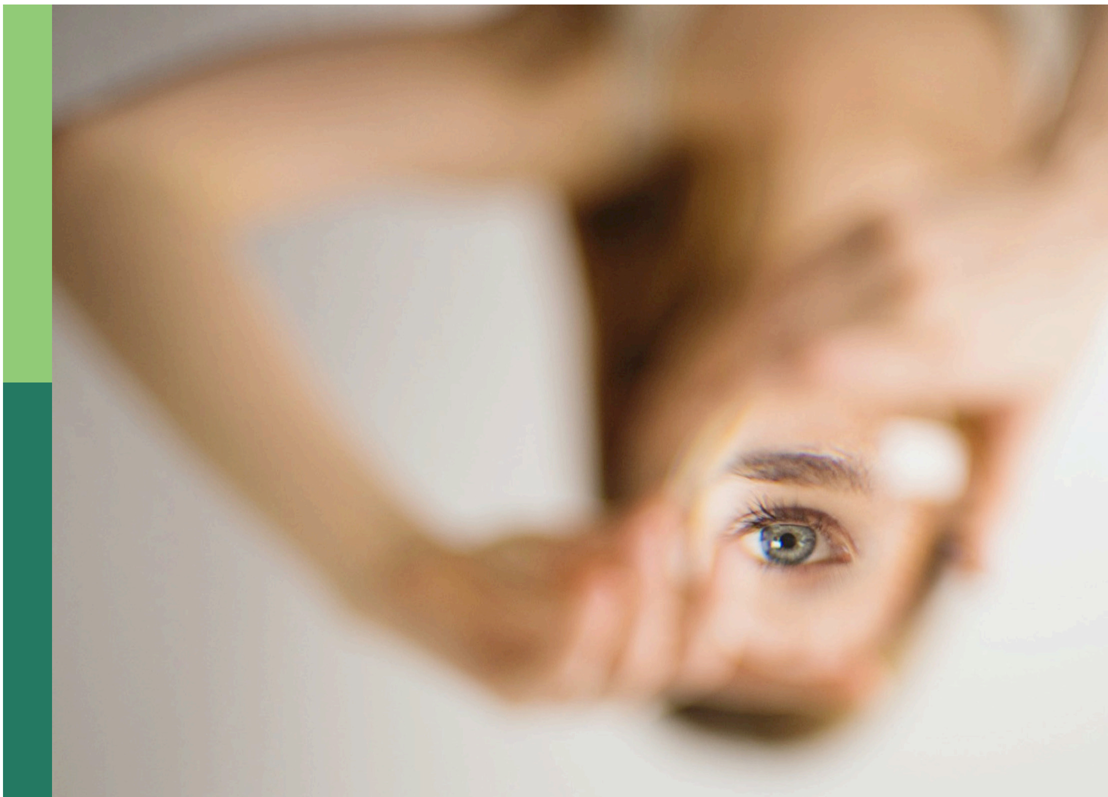

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Effort-based decision-making and cognitive fatigue

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Editorial: Effort-based decision-making and cognitive fatigue

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Editorial on the Research Topic
Effort-based decision-making and cognitive fatigue

Introduction

During their daily life, humans and animals must frequently make effort-based decisions about choosing among several more or less effortful activities, stopping or maintaining an ongoing effortful activity. In the field of effort-based decision-making, several questions remain to better understand how individuals engage or persevere in cognitive or physical tasks requiring effortful control. This Research Topic aimed to address several questions, such as: How long does it take to recover from cognitive fatigue? How can self-control and boredom interact with exercise behavior? Which brain structures support effortful control? Do perceived exertion scales effectively monitor effort commitment? Is it possible to train willpower?

In this perspective, researchers with interests in psychology, neuroscience and movement science submitted new reflections, data and modeling allowing significant advancements in the understanding of effort-based decision-making and cognitive fatigue in symptomatic and asymptomatic populations. Twenty articles were included in the Research Topic and contributed to answering the abovementioned questions.

The Research Topic is organized into five parts. The first part focuses on the acute effect of cognitive fatigue on motor control and executive control. The second part debates on self-control depletion. The third part is related to effortful control deployment in humans and rats. The fourth part is dedicated to the perception of effort. The fifth part focuses on training programs aiming to improve the capacity to exert effortful control.

Part 1. Cognitive fatigue

Cognitive fatigue, also known as mental fatigue, is generally observed during or after prolonged engagement in effortful cognitive tasks (Van Cutsem et al., 2017; Brown et al., 2020). Skau et al., the first article of this Research Topic, analyzes different definitions of fatigue and makes a clear distinction between the objective and the subjective manifestations of fatigue. Then, four experimental studies consistently observed a negative impact of cognitive fatigue on performance.

First, Salomone et al. induced cognitive fatigue with a 24-min time load dual back (TLDB) task followed by a 45-min Simon task. Cognitive fatigue was measured through performance indexes as a function of time-on-task during the Simon task. The results showed that time-on-task impaired online control by disrupting the capacity to suppress the incorrect response.

Second, Jacquet et al. demonstrated that cognitive fatigue induced by a 32-min TLDB task negatively impacts the performance in a subsequent arm-pointing task. Concerning the persistence of cognitive fatigue after the TLDB, task performance in the pointing task and theta and alpha power density of brain oscillations recorded during rest periods suggested that participants remained mentally fatigued until 20 min after the end of the cognitive task.

Third, Walker et al. examined the effect of cognitive fatigue in healthy controls and a sample of multiple sclerosis (MS) patients characterized by high fatigability. Cognitive fatigue was induced with different versions of the paced auditory serial addition test (PASAT). Their results showed that the 3-s intertrial interval version of the PASAT is the most effective version to detect impaired performance and cognitive fatigue in MS patients.

Fourth, Wylie et al. used a *n*-back task to induce cognitive fatigue. They measured cognitive fatigue with visual analog scale, indexes from signal detection theory and functional magnetic resonance imaging. They showed that signal detection theory indexes and level of activation in the caudate nucleus significantly correlated with subjective fatigue.

Globally, the four experimental studies of this part showed an impaired performance during or after an effortful task tapping executive functions. Interestingly, two studies (Jacquet et al.; Wylie et al.) support that an increase in activation of the insula and prefrontal theta density could be two biomarkers of cognitive fatigue (Borghini et al., 2014; André et al., 2019).

Part 2. Self-control failure

Social psychologists used the sequential task protocol to obtain self-control failure (Figure 1; Lee et al., 2016). This phenomenon, also called ego depletion, is observed after an initial act of self-control and leads to impaired performance in a subsequent self-control task (Dang, 2018).

Over the past decade, the existence of the ego-depletion effect has been challenged by meta-analytic studies (Carter et al., 2015) and faces a replication crisis (Hagger et al., 2016; Vohs et al., 2021). First, Englert and Bertrams present a short history of this replication crisis and criticize the scientific approach used by the replication studies. More importantly, the authors ask for the necessity to clearly operationalize the central constructs of the theoretical model that hypothesize the ego depletion effect to test its validity.

Another problem related to the use of the sequential task protocol, is that the depleting task and the control task can induce boredom and interact with self-control (e.g., Mangin et al., 2021). In this perspective, Wolff et al. suggested that boredom might have contributed to the inconsistencies observed in replication studies by acting as a confound of self-control effects on performance.

As mentioned above, the duration of the depleting task (see Figure 1) seems to be an important parameter in the occurrence of the ego depletion effect. In the third article, Boat et al. manipulated the duration of the depleting task (4, 8, and 16 min). They showed that the performance of the subsequent wall-sit task was more negatively impacted when participants spent longer on the initial self-control task.

In contrast, the fourth and fifth articles (Englert, Dziuba, Giboin et al.; Englert, Dziuba, Schweizer et al.) failed to find an ego depletion effect.

In the sixth article, Alquist et al. presented three experiments using the sequential task protocol and showed that uncertainty introduced in the depleting condition impaired a subsequent task involving executive control. They suggest uncertainty is a cue for conserving effort.

Overall, this section shows that there are still strong debates about the conditions of occurrence of the ego depletion effect.

Part 3. Effort-based decision making

The third part is dedicated to the decision-making process allowing the deployment of effortful control according to the costs and benefits associated with the achievement of the goal of the task. Cost-benefit models (e.g., Shenhav et al., 2017) are presently the most popular models of effort-based decision-making in economics, neuroscience and psychology. These models consider that an individual consent to deploy effort when benefits outweigh costs. Numerous neurophysiological studies suggest that the anterior cingulate cortex (ACC) plays a crucial role in this decision-making process (André et al., 2019; Müller and Apps, 2019). This part includes five articles that contribute to a better understanding of this process.

The first study conducted by Jiang et al. aimed to examine the deployment of effortful control with event-related potential (ERP) in schizophrenia, a mental disorder often associated with deficits of effort mobilization (McCarthy et al., 2016). Their results suggest that schizophrenia patients experienced an increased mental workload and slowed processing speed due to effortful information processing deficits.

In the second article, Lacroix et al. introduced a neuropsychological model based on Kahneman's capacity model of attention (Egeth and Kahneman, 1975). Their model partially explains the variability of results observed in vestibular-damaged patients and contributes to the understanding of the vestibular compensation process.

In the third article, Silva et al. investigated whether electrical stimulations of the ACC or anterior insula, change the rat's persistence in an effortful weightlifting task. Their results confirm

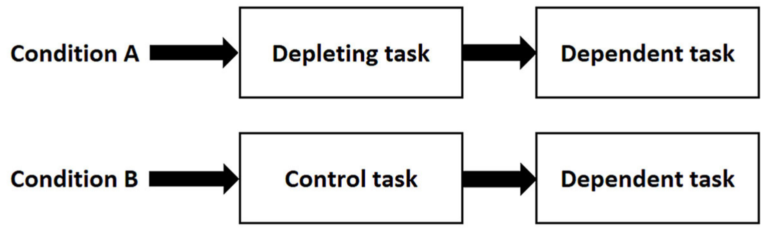

FIGURE 1
Time course of the sequential task protocol. In condition A, participants perform a depleting task requiring self-control followed by a dependent task also requiring self-control. In condition B, participants perform a control task that does not require self-control followed by the same dependent task than in condition A. The two conditions can be arranged in a within-subjects design (same participants performing two counterbalanced sessions) or in a between-subjects design (one group per condition).

the crucial role of the ACC in the deployment of effortful control during a cognitive or physical task.

In the fourth study, van As et al. examined whether individuals weigh physical effort-costs more strongly when they are cognitively or physically fatigued. For that purpose, they induced cognitive fatigue with a 45-min 2-back task or physical fatigue with a 45-min intermittent submaximal handgrip task. In the subsequent effort-based decision-making task, participants had repeatedly to accept or reject offers with varying levels of rewards and physical effort. The results of this study suggest that individuals ascribe more weight to physical effort-costs than cognitive effort-costs.

In the last study of this part, Feng et al. aimed to validate a model of procrastination with mathematical simulations. Their model predicts that procrastination can be mitigated by explicitly informing an individual about the remaining future cost associated with procrastination and the possible decrement of this cost if the individual chooses to perform the procrastination-related behavior.

Part 4. Perception of effort

Previous research in sports sciences showed that the perception of effort in a physical task, also called perceived exertion, is increased when it is preceded by a long cognitive effortful task (Marcora et al., 2009; Pageaux and Lepers, 2016). The two experimental studies included in this part aimed to validate the use of the perception of effort to prescribe and/or monitor exercise in healthy young adults.

In the first article, Payen de la Garanderie et al. conducted two experiments to manipulate the physical demand and alter the difficulty of the task. They monitored the perception of effort with the Borg's CR100 scale while controlling for performance in two upper limb motor tasks: the box and block test and a pointing task. The authors showed that perception of effort is a valid tool to prescribe and monitor exercise during upper-limb motor tasks.

In the second article, Armes et al. examined the validity of repetitions in reserve (RIR) scales that are used to assess and/or control effort by participants estimating how many repetitions they can perform before reaching momentary task failure during resistance exercises. They conducted two experiments to test the validity of the RIR scales in resistance exercises with submaximal intensities. They showed that participants with at least 1 year of resistance training experience are likely not adequately accurate at gauging effort in submaximal conditions. These results suggest that the RIR scale during resistance training exercise may not be as accurate as needed to estimate accurately the actual effort.

These two studies emphasize the importance for researchers to carefully check in the literature the validity of the psychophysical scales they plan to use for assessing effort allocation, as not all scales seem adequate. Indeed, while one was able to monitor quite accurately the effort deployed in fine motor tasks, the second one was inadequate to predict task failure in submaximal resistance exercises.

Part 5. Training the capacity to deploy effortful control

This part includes two articles dedicated to the hypothesis of improving the capacity to exert effortful control in difficult conditions through training programs. In this perspective, two recent studies tested the efficacy of brain endurance training programs and received relative success (Dallaway et al., 2021, 2023).

First, Audiffren et al. report an umbrella review that examined the efficacy of different training programs in improving executive functions and self-control. The results of 63 meta-analyses on this topic were analyzed. More than 79% of these reviews showed that training programs are effective in improving performance in tasks tapping executive functions and/or self-control with a small to large effect size. Training programs including physical exercises or mindfulness exercises seem to be the most promising in terms of far-transfer effects. In the second part of the article, the authors propose a theoretical neuroscience framework explaining these gains in willpower.

In the second article, Holmqvist et al. showed that patients with mild to moderate stroke or traumatic brain injury can benefit from a 6-week intensive cognitive training. The training program targeted five attention modalities: focused, sustained, selective, divided, and flexible attention. Their results suggest that patients with high levels of cognitive fatigability benefit most from attention training.

The two articles of this part suggest that training the capacity to maintain effortful control despite internal or external constraints is a promising way to increase the resistance to cognitive fatigue. The development of effective training programs applied to specific domains (e.g., sport, education, labor) is a very exciting perspective.

Conclusion and perspectives

The investigation of effort-based decision-making and cognitive fatigue embraces several scientific disciplines, such as economics, psychology, neuroscience, and exercise physiology. A better understanding of these two scientific objects requires at least a multidisciplinary, and more optimally an interdisciplinary approach. This Research Topic constitutes an additional step in this direction, and we hope that the reading of this selection of articles has significantly contributed to the advancement of this scientific field.

Author contributions

MA wrote the manuscript. RC, JS, NS, SR, and BP revised and edited the manuscript. All authors contributed to the article and approved the submitted version.

Conflict of interest

The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

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What You Don't Know Can Hurt You: Uncertainty Impairs Executive Function

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Three studies demonstrated that situational uncertainty impairs executive function on subsequent unrelated tasks. Participants were randomly assigned to either uncertain situations (not knowing whether they would have to give a speech later, Studies 1-2; uncertain about how to complete a task, Study 3) or control conditions. Uncertainty caused poor performance on tasks requiring executive function that were unrelated to the uncertainty manipulation. Uncertainty impaired performance even more than certainty of negative outcomes (might vs. definitely will have to make a speech). A meta-analysis of the experimental studies in this package found that the effect is small and reliable. One potential explanation for this effect of uncertainty on executive function is that uncertainty is a cue for conserving effort.

Keywords: uncertainty, executive function, self-regulation, self-control, ego depletion

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INTRODUCTION

Uncertainty is a common experience for decision-makers in many contexts, including health care (e.g., Babrow et al., 1998; Han et al., 2011), business (Erdem and Keane, 1996; Bloom et al., 2018), military conflict (Posen, 2016), environmental protection (Ascough et al., 2008; Polasky et al., 2011), government economic policy (Stockhammer and Grafl, 2010), real estate (Thanh et al., 2018), and sports (Knobloch-Westerwick et al., 2009). Elucidating the effects of uncertainty could therefore have practical value as well as build scientific theory. If uncertainty itself causes cognitive fatigue, that could impair effortful decision-making — quite possibly in ways of which the decision maker would be unaware. The present research is designed to test the hypothesis that uncertainty impairs executive function.

Executive Function and Ego Depletion

Executive functions are the top-down processes required to change or override automatic responses (Diamond, 2013). Executive function is required for processes such as decision-making, self-control, and initiative (Baumeister, 2002). The hypothesis that executive function can be impaired because of low energy, akin to the folk notion of willpower, was proposed in the 1990s (e.g., Baumeister et al., 1998; Muraven et al., 1998). The state of impaired performance was dubbed ego depletion. Over the past two decades, hundreds of studies were published showing various kinds of ego depletion effects (for reviews, see Hagger et al., 2010; Baumeister and Vohs, 2016). Recently, there has been a lively debate about the existence, effect size, and mechanism of the ego depletion effect (Beedie and Lane, 2012; Kurzban et al., 2013; Inzlicht et al., 2014, 2015; Xiao et al., 2014; Carter et al., 2015; Inzlicht and Berkman, 2015; Cunningham and Baumeister, 2016; Dang, 2016, 2017;

Hagger et al., 2016; Lurquin et al., 2016; Dang et al., 2017, 2020; Garrison et al., 2019). Ego depletion can be replicated in many contexts and laboratories but also does not occur invariably, and so extending the theory to include moderating factors and parallel processes is a high priority.

Multiple studies have extended ego depletion to decision-making. Vohs et al. (2014) showed that making effortful decisions led to subsequent impairments in executive function. Pocheptsova et al. (2009) showed, conversely, that ego depletion stemming from effortful tasks impaired subsequent decision making, effectively shifting people toward low-effort responses to decision dilemmas. They found that depleted participants maximized on a single dimension rather than integratively compromising to maximize across multiple dimensions, they postponed decisions, and they failed to think carefully so as to prevent logically irrelevant information from biasing their choices. Making decisions impairs executive function, and previous acts of executive function impair decision-making.

The initial theorizing about ego depletion assumed that executive function was impaired because the person's energy had been expended and was too low to permit further exertions. That view is no longer considered tenable. A variety of moderators have been shown to reduce or eliminate the effects of depletion. If participants performed poorly on a second executive function task because they were truly unable to control themselves, factors such as motivation on the second task (Muraven and Slessareva, 2003; Park et al., 2008; Vohs et al., 2012), self-affirmation (Schmeichel and Vohs, 2009), and positive affect induced between tasks (Tice et al., 2007) would be unlikely to eliminate ego depletion effects (Inzlicht and Schmeichel, 2012). Participants' likelihood of failing at self-control is also increased by believing self-control is limited (Job et al., 2010; but cf. Vohs et al., 2012) and by believing one has expended energy on a previous task (Clarkson et al., 2010). Beliefs about the nature of self-control or the task one has completed would be unlikely to moderate the effect of a first task on a second task if the resource was "used up" and unavailable for further exertion.

Some recent theories of ego depletion suggest that the effect of a first task on a second is due to cost-benefit calculations, or a combination of such calculations and limited resources (Beedie and Lane, 2012; Kurzban et al., 2013; Inzlicht et al., 2014; Shenhav et al., 2017; André et al., 2019). There is evidence that ego depletion effects observed in the lab are due to conservation, not a thoroughgoing exhaustion of a resource. The self may have expended some energy and though it is far from being entirely out of fuel it seeks to conserve what remains. Previous research on conservation has shown that when participants have already expended effort and anticipate additional tasks requiring self-control, they perform worse on the current task and better on the anticipated task than participants who are surprised with an additional task (Muraven et al., 2006). One reason people fail at sequential executive function tasks may be that once they have expended some effort, they begin to conserve their remaining energy for future tasks that may have high priority. In this way, mental energy is similar to physical energy, in which muscles feel tired and athletes begin conserving their energy long before their muscles even approach true exhaustion (Evans et al., 2015).

Uncertainty

There are hundreds of published laboratory studies on ego depletion, but most of them have induced the state by requiring participants first to engage in a task requiring self-control. We sought to broaden the potential focus of this area by showing that encountering uncertainty can cause impairments in executive function similar to those caused by a task specifically designed to be effortful. Determining other ways of producing depletion-like effects may provide additional information about the mechanism of the effect (Milyavskaya et al., 2019).

Uncertainty involves an individual lacking important information (Bar-Anan et al., 2009). One may lack information about whether, when, or where something will happen, or what will happen. One may have multiple pieces of conflicting information and lack information about which is true or should be weighted most heavily. One may know the details of the situation but be uncertain about how best to respond effectively.

Uncertainty may cue conservation via The Behavioral Inhibition System. The Behavioral Inhibition System is a motivational system that becomes activated in response to situations that are conflicted or uncertain and pauses progress on uncertain or conflicted goals (Gray and McNaughton, 2000; Corr et al., 2013; Hirsh and Kang, 2016). Halting progress when circumstances are uncertain can protect an organism from encountering harm (trying to get a piece of food a predator is guarding), and it can preserve energy for the yet-undetermined demands of the situation. Previous research has shown that thinking about an issue about which one was uncertain impairs task performance through activation of the Behavioral Inhibition System (Alquist et al., 2018).

A recent review of animal research by Anselme and Güntürkün (2018) showed that in environments marked by uncertainty about food, animals shifted toward conservation strategies, including caching and hoarding food, eating more, and gaining weight. Thus, uncertainty causes conservation of energy resources even in quite simple animals. Such animals are presumably unable to engage in complex projections of multiple possible futures (or, indeed, cost-benefit calculations amid multiple alternatives). Indeed, a recent experiment showed that even humankind's closest and presumably highly intelligent ape relatives were unable to learn to understand the future as containing multiple alternative possibilities — unlike human children, who quickly grasped the multiplicity of alternatives (Redshaw and Suddendorf, 2016). Responding to uncertainty therefore does not require complex understanding, and the impulse to conserve resources in response to uncertainty may be unconscious and automatic.

Energy conservation would likely be an adaptive response to uncertainty. Presumably people (like other animals) evolved to conserve energy because one could not be sure of always having enough resources. Inadequate energy exposed one to multiple risks, including impaired immune function and death. The more uncertain the future, the more adaptive it would be to conserve energy generally so as to be able to cope with unknown developments. If ego depletion typically occurs because the human body is reluctant to expend energy that it might need later on, then uncertainty should heighten this tendency because it heightens the possibility of future demands. By definition, uncertainty means not knowing what to expect — and so it is impossible to know how much energy will be required. Therefore, the adaptive response to uncertainty would be to conserve.

Present Research

Our studies manipulated initial exposure to uncertainty and then measured self-regulatory performance. In Studies 1 and 2, we randomly assigned participants to be either certain or uncertain about what they would be doing later in the study and measured executive function using a skill-based game, namely Operation (Study 1), and solvable anagrams (Study 2). In Study 3, we randomly assigned participants to be uncertain or certain about how to respond to task prompts by making the instructions mismatched to the response situation, and then measured their subsequent executive function by measuring persistence on unsolvable puzzles. We also report a meta-analysis testing whether the effect is reliable across all measures. These manipulations are a departure from much of the previous research on depletion, because we are not directly manipulating how effortful the initial task is.

Social psychology has recently shifted toward new methodological criteria, including pre-registration of methods and hypotheses, and larger samples. This research was conducted prior to those changes, back when the best practices emphasized convergence across multiple methods in different studies. Publication was delayed because we sought to establish the mediating process based on the initial theory that uncertainty would evoke extra mental work and emotion regulation to account for multiple alternative possibilities. We were unable to find evidence of that mechanism. A complete list of the mediators and moderators tested in these studies is presented in the **Supplementary Materials**. The revised theory, that uncertainty serves as a cue to stimulate conservation, has emerged as a more plausible alternative, particularly in light of the recent review by Anselme and Güntürkün (2018).

STUDY 1: OPERATING UNDER UNCERTAINTY

Study 1 experimentally manipulated uncertainty in order to test whether uncertainty impaired executive function. The type of uncertainty being tested in this study involved uncertainty in which the participant was waiting for important information. Specifically, participants in the uncertain condition were left uncertain about whether they would have to give a speech later in the study (Core et al., 2018). Participants in the certain conditions were either told that they would soon have to give a speech or were told that they would not have to give a speech.

Expecting to give a speech is a highly aversive and stressful circumstance for many people. Previous research has shown that participants assigned to anticipate and then give a speech had an increased heart rate and cortisol as compared to baseline (Kirschbaum et al., 1993).Expecting a speech has been shown to affect participant's performance on tasks requiring executive function. Participants assigned to anticipate giving a speech learn more slowly on the Iowa Gambling task (Preston et al., 2007) and score lower on decision-making tasks (Starcke et al., 2008). A simple prediction would be that the aversiveness of the experience, and therefore the degree of impairment, would be felt in direct proportion to the anticipated likelihood of the aversive (speech) outcome. Thus, definitely having to give a speech would be the worst, definitely not having to speak would be the best, and uncertainty would fall in between. In order to show that uncertainty *per se* was depleting, we predicted that uncertainty would be at least as detrimental to subsequent executive function as the certain expectation of having to speak.

Executive function was measured using the board game Operation, which has been used in previous studies (e.g., DeWall et al., 2008; Englert and Bertrams, 2013). The game requires participants to remove pieces from a board as quickly and with as few errors as possible. Inhibition is required for participants to stay focused on the task and carefully avoid making errors. Balancing the need to finish things quickly and the desire to do them well is relevant in everything from meeting deadlines at work to performing non-board-game surgery to getting a manuscript submitted to a scientific journal. We predicted that participants who were uncertain about whether they would be giving a speech would make more errors and take more time to complete the task than participants in the no speech condition — and would also be equal to or worse than participants in the definite speech condition.

Method

Participants

Fifty participants (22 women; 28 men) participated in this study in exchange for course credit. Four participants were excluded from the final sample: two participants who reported knowing there were no other participants in the experiment; one participant who came into the lab very sick; and one participant who arrived too late to complete the study. The final sample had an average age of 19.87 (SD = 4.87). 10.9% identified as Latino or Hispanic Latino. Participants' races were 4.3% Asian,13.6% Black or African American, 78.3% white, 2.2% more than one race and 2.2% unknown or not reported.

Procedure

Uncertainty manipulation

All participants were told that some participants would be giving speeches while other participants rated those speeches (Core et al., 2018). They were told that they would be completing the communication task later in the study, but to save time, they would be assigned their condition now. In the speech and no speech conditions, participants were told, "You are participant number ___, and it says here that you are in the speech (no speech) condition. Let's start on the intelligence task, and when you're done, we'll move to another room for the communication task." In the uncertain condition, the experimenter acted flustered and said, "Hmm. You are supposed to be participant number ___, but I don't see your number on here anywhere. I have a master sheet in the other room with all the numbers on it. I'm going to start you on the next task, and I'll go get the sheet while you are working." This left the participants in the uncertain condition uncertain about whether they would be giving a speech later in the study.

Executive function

Operation. Executive function was measured using the board game Operation (DeWall et al., 2008). Participants were told they were doing the Operation task as a measure of hand-eye coordination. Each participant was asked to try removing one piece for practice before the task began. Participants were asked to remove all the pieces from the board as quickly as possible. The experimenter recorded the time the participant spent working and the number of times the participant sounded the buzzer by hitting the sides of a piece's space on the game board. For consistency across all studies, we report each measure (e.g., time and errors; number attempted and solved) separately rather than computing composite scores (DeWall et al., 2008, 2011).

Competence

We worried that participants may have withheld effort in the uncertain condition because they viewed the experimenter as incompetent (given that the experimenter did not know the condition). In order to test this possibility, participants were asked to respond to the question, "How competent was the researcher who administered your study today?" on a scale of 1 (not at all) to 5 (extremely). Responses were made on the computer to reduce participants' concern that the experimenter would see their responses.

Additional measures

In addition to the measures reported here, exploratory mediators and moderators that were included in this and the following studies are reported in the **Supplementary Materials**.

Data Analyses

We conducted ANOVAs comparing the means in the three conditions on errors, time, and perceived experimenter competence. We also conducted planned comparisons between individual conditions.

Results

Operation Performance

As predicted, ANOVA revealed a significant difference among conditions on the number of errors participants made (i.e., the number of times they sounded the buzzer), $F(2, 43) = 4.14$, $p = 0.02$, $\eta^2 = 0.16$, 90% CI[0.01, 0.30], See **Table 1**. Planned comparisons revealed that participants in the uncertain condition ($M = 21.92$, $SD = 5.84$) made significantly more errors than participants in the no speech condition ($M = 15.92$, $SD = 7.50$), $t(43) = 2.15$, $p = 0.04$, $d = 0.86$, 95% CI[0.05, 1.66]. Planned comparisons also indicated that participants in the uncertain condition made significantly more errors than participants in the speech condition ($M = 15.10$, $SD = 7.29$), $t(43) = 2.77$, $p = 0.01$, $d = 0.98$, 95% CI[0.25, 1.69].

There was no significant difference among conditions on the amount of time participants took to complete the Operation task, $F(2, 43) = 0.29$, $p = 0.75$, $\eta^2 = 0.01$, 90% CI[0.00, 0.08]. There was also no significant relationship between the time participants took and the number of errors they made, $r = 0.09$, $p = 0.56$, 95% CI[-0.21, 0.37]. Thus, it appears that only the error measure was sensitive to the manipulation, and how long it took participants to finish it was relatively unaffected by it.

TABLE 1 | Study 1: Means and standard deviations across uncertainty conditions on errors and time during the operation task.

	Uncertain	Speech	No Speech
	M (SD)	*M (SD)*	*M (SD)*
Number of errors	21.92_a (5.84)	15.10b (7.29)	15.92_b (7.50)
Time in seconds	229.31 (37.51)	214.08 (75.05)	214.71 (61.20)

Means with different subscripts are significantly different at $p < 0.05$.

Competence

There were no differences among conditions in participants' perceptions of the experimenter's competence, $F(2, 43) = 0.86$, $p = 0.43$, $\eta^2 = 0.04$, 95% CI[0.00, 0.16]. There was also no correlation between perceived experimenter competence and the time participants spent or the number of errors they made on the Operation task, all p's > 0.54.

Discussion

Participants who were uncertain about whether they would have to give a speech made significantly more errors on the Operation task than both participants who knew they would not have to give a speech and participants who knew they would have to give a speech. There were no differences between conditions in time spent on the task. We acknowledge that multiple values resulting from the dependent variable increase the risk of Type 1 error. In order to address this concern, we include all values (for example, in this study, number of errors and time spent) for dependent variables in the meta-analysis of studies on page 17.

Participants who were uncertain about whether they would have to give a speech showed poorer executive function than participants who knew for sure that they would have to give a speech. This suggests that, as far as executive function is concerned, it is actually better to be sure of a negative outcome than to know a negative outcome is possible.

A possible alternative explanation for the predicted results would be that participants in the uncertain condition inferred that the experimenter was incompetent, based on the experimenter in that condition not knowing what treatment had been assigned to them. This alternative was not supported by the ratings of the experimenter competence, which showed no difference by condition.

STUDY 2: UNCERTAINTY AND SOLVABLE ANAGRAMS

Study 1 found evidence that waiting to find out if one was giving a speech impaired executive function more than knowing one would have to give a speech. Study 2 was designed to extend this effect to another measure of executive function, as a conceptual replication.

Participants' executive function was measured using a series of solvable anagrams. Working through a daunting task under the pressure of a deadline demands that people use executive function to avoid distractions and to focus their attention on the task at hand. Solving anagrams also requires working memory because it involves trying letter combinations in different orders. Maintaining focus and persevering despite failure also requires inhibition for successful anagram performance. Anagram attempts have been used as a measure of executive function in previous research (Muraven et al., 1998), and we specifically used solvable anagrams because we wanted to include a measure for which success was possible.

Method

Participants

Ninety-two participants (70 women; 22 men) participated in this study in exchange for course credit. Three participants were excluded from the final sample because they reported knowing that they would not have to give a speech. The final sample was 22.4% Hispanic or Latino. Participants' race representation was: 1% American Indian/Alaska Native, 2% Asian, 3.1% Black, 69.4% White, 6% More than one race, 18.5% Unknown or not reported. Participants' mean age was 18.35 (SD = 0.80).

Measures and Procedure

Uncertainty manipulation

Uncertainty was manipulated using the same procedure as in Study 1.

Executive function

In order to measure participants' executive function, participants were given a set of fifty solvable five-letter anagrams and were asked to solve as many as possible in ten minutes. There was a blank line next to each anagram where participants were asked to put their solution. Any line on which the participants wrote an attempted solution was coded as an attempted anagram, and each anagram solved correctly was considered a completed anagram.

Self-reported uncertainty

After working for ten minutes on the anagrams, participants were asked to answer some questions before they began the speech task. Participants were asked to respond to the statement, "Earlier in the study, I was uncertain about whether or not I'd be giving a speech" on a scale of 1 (strongly disagree) to 9 (strongly agree).

Data Analyses

We ran ANOVAs comparing the means in the three conditions on anagrams attempted, anagrams solved, and self-reported uncertainty. We also ran planned comparisons between individual conditions.

Results

Anagram Performance

ANOVA revealed a significant effect of condition on the number of anagrams participants attempted, $F(2, 86) = 3.59$, $p = 0.03$, $\eta^2 = 0.08$, 90% CI[0.004, 0.17], See **Figure 1**. Participants in the uncertain condition (M = 11.37, SD = 5.38) attempted significantly fewer anagrams than participants in the no speech condition (M = 15.07, SD = 8.21), $t(86) = 2.18$, $p = 0.03$, $d = 0.54$, 95% CI[0.05, 1.03]. Participants in the uncertain condition also attempted significantly fewer anagrams than participants in the speech condition (M = 15.57, SD = 7.48), $t(86) = 2.26$, $p = 0.03$, $d = 0.61$, 95% CI[0.07, 1.14]. This replicates the finding from Study 1 that executive function suffered more when participants knew a bad outcome was possible than when they knew the same outcome was definite.

The effect of condition on the number of anagrams solved was not significant, $F(2, 86) = 2.49$, $p = 0.09$, $\eta^2 = 0.05$, 90% CI[0.00, 0.13]. Planned comparisons showed that participants in the uncertain condition (M = 9.70, SD = 5.52) solved significantly fewer anagrams than participants in the no speech condition (M = 13.54, SD = 7.99), $t(86) = 2.17$, $p = 0.03$, $d = 0.53$, 95% CI[0.04, 1.02]. Although participants in the uncertain condition solved fewer anagrams than those in the definite speech condition, (M = 12.19, SD = 8.69), the results were not significant, $t(86) = 1.29$, $p = 0.20$, $d = 0.35$, 95% CI[-0.18, 0.88]. The difference between the speech and no speech conditions was also not significant, $t(86) = 0.89$, $p = 0.38$, $d = 0.26$, 95% CI[-0.31, 0.82].

Thus, participants who were uncertain about whether they would have to give a speech attempted fewer anagrams than participants in the speech and no speech conditions and solved fewer anagrams than participants in the no speech condition.

Self-Reported Uncertainty

ANOVA revealed a significant degree of variation among conditions in how uncertain participants reported feeling about their role in the communication task, $F(2, 86) = 4.26$, $p = 0.02$, $\eta^2 = 0.09$, 90% CI[0.01.18]. Planned contrasts revealed that participants in the uncertain condition (M = 7.17, SD = 2.01) reported feeling more uncertain than participants in the no speech condition (M = 5.61, SD = 2.64), $t(86) = 2.87$, $p = 0.005$, $d = 0.77$, 95% CI[0.23, 1.31]. Although the means were in the predicted direction, participants in the uncertain condition did not report feeling significantly more uncertain than participants in the speech condition (M = 6.81, SD = 1.94), $t(86) = 0.61$ $p = 0.54$, $d = 0.15$, 95% CI[-0.33, 0.63].

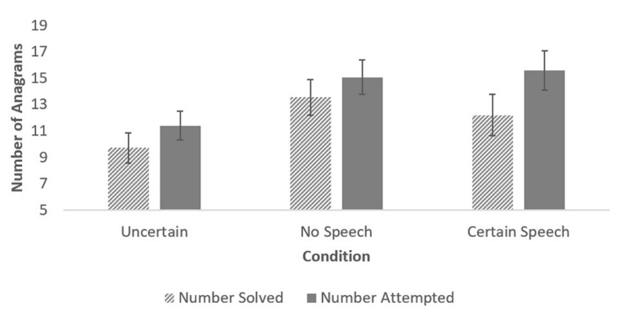

FIGURE 1 | Anagrams solved and attempted by condition in study 2. Error bars show standard errors.

Discussion

Participants who were uncertain about whether they would have to give a speech made significantly fewer attempts to solve the anagrams than both participants who knew they would not have to give a speech and participants who knew they would have to give a speech. Thus, once again, uncertainty in the form of possibly bad news produced worse performance than definite bad news. Uncertain participants also solved fewer anagrams than those in the no speech condition and those in the definite speech condition, though the last difference was not significant. These results provide evidence that uncertainty about what one will be required to do impairs people's performance on subsequent measures of executive function.

Participants in the uncertain condition reported being significantly more uncertain than participants in the no speech condition. Those in the definite speech condition reported levels of uncertainty intermediate between the two (and not significantly different from either). Participants in the speech condition may have reported uncertainty about their potential performance on the speech task rather than uncertainty about which task they would be completing.

STUDY 3: RESPONSE UNCERTAINTY

Study 3 manipulated uncertainty by giving participants a task where it was either clear how they should respond (control condition) or unclear how they should respond (uncertain condition). Participants were briefly shown a colored square on the computer. They were then asked to complete the math problem associated with the color they just saw. For example, participants saw a yellow square for one second and then the instructions on the computer read "Please complete the equation associated with the color you just saw: Blue: 2 × 6; Green: 12 × 3; Yellow: 10 × 7; Red: 9 × 9." For participants in the control condition, all twenty trials showed colored squares that fit clearly into the four categories provided (blue, green, yellow and red). However, for participants in the uncertain condition, twelve of the twenty trials included colors that did not fit the colors provided (e.g., orange, blue-green, purple). We predicted that participants who performed the unclear task would feel significantly more uncertain than participants who were given the clear task — and this uncertainty would carry over to cause impairments in performance on a subsequent, unrelated task.

Executive function was measured using persistence on puzzles that (unbeknownst to participants) were unsolvable. Persistence on a difficult (in this case, impossible) task requires inhibition because individuals have to override the impulse to quit (Baumeister et al., 1998). Executive function includes the effortful overriding of one's responses, particularly with the goal of changing them according to some standard. Persistence requires overriding any desire to quit in order to force oneself to keep striving despite discouragement and failure. Because the tasks were unsolvable, discouragement and failure would continue unabated as long as the person persisted. We predicted that participants who were given the unclear version of the task would subsequently spend significantly less time persisting on the puzzles and make fewer attempts to solve the puzzle than participants who were given the clear version of the task. We also predicted that this relationship would be mediated by participants' self-reported uncertainty from the first task.

Method

Participants

Fifty-one participants (15 men, 36 women) participated in this study in exchange for course credit. One participant was excluded from analyses for recognizing that the puzzle was unsolvable. The final sample had an average age of 18.65 (SD = 1.41).

Procedure

Uncertainty manipulation

The uncertainty manipulation was programed using MediaLab research software (Jarvis, 2006). The experimenter told participants that the purpose of the study was to understand how people reason through different kinds of puzzles. Participants were shown a square of color on the computer screen for one second. They were then shown the names of four colors next to four math problems and were asked to complete the math problem associated with the color of the square they had seen previously. Participants in the control condition saw colors that clearly matched the colors listed for all twenty trials. Participants in the uncertain condition were shown colors that did not clearly fit the colors listed (e.g., blue-green) for twelve of the twenty trials.

To prevent participants from stopping to ask about the ambiguous colors, participants were told that they would be timed and should work as quickly as possible. To increase participants' motivation to do well, all participants were told they would earn twenty-five cents each time they answered correctly, and they could earn up to five dollars on the task. At the end of the study, all participants were given $5.

Executive function

Persistence. After completing the colored square task, participants were given an unsolvable tracing puzzle as a measure of executive function (Baumeister et al., 1998). In order to convince participants that the puzzle was solvable, the experimenter completed a solvable tracing puzzle in front of the participant as an example. Participants were given the instructions from Baumeister et al. (1998), and were told that if they wished to stop before they finished, they should ring the bell on the table. Participants were provided with a stack of paper containing many copies of the same unsolvable puzzle and a highlighter. The experimenter left the room, and began timing the amount of time the participant persisted before ringing the bell. Any participant still working after 30 minutes was interrupted and asked to continue with the rest of the study (3 participants worked until the limit: 1 in the uncertain condition, 2 in the control condition). Each copy of the puzzle that was marked with the highlighter was coded as one attempt to solve the puzzle.

Manipulation check

After the unsolvable puzzle, participants were asked to respond to the question, "When you were completing the task with the colored squares and the math problems, how uncertain did you feel?" on a scale of 1 (not at all uncertain) to 5 (very uncertain). Last, participants were probed for suspicion, debriefed about the purpose of the study, paid, and dismissed.

Data Analysis

We ran t-tests comparing the uncertain and control conditions on puzzle attempts, time, and self-reported uncertainty. We also tested whether the effects of condition on attempts and time were mediated by self-reported uncertainty.

Results

Executive Function

Persistence

There was a significant difference between the uncertain and control conditions on the number of attempts made at solving the unsolvable tracing puzzle, $t(48) = 2.28$, $p = 0.03$, $d = 0.63$, 90% CI[1.05, 16.86], See **Table 2**. As predicted, participants who had been given the ambiguous task subsequently made fewer attempts ($M = 18.00$, $SD = 11.41$) than participants in the control condition ($M = 26.95$, $SD = 16.38$). A parallel effect was found for the measure of time spent working on the puzzles, but it was not significant, $t(48) = -1.73$, $p = 0.09$, $d = 0.49$, 95% CI[-8.46,0.63]. Participants in the uncertain condition ($M = 10.61$ minutes, $SD = 7.35$) spent less time on the unsolvable tracing puzzle than participants in the control condition ($M = 14.53$ minutes, $SD = 8.63$).

Manipulation Check

There was a significant difference between the uncertain and control conditions in how uncertain participants felt about the first task, $t(48) = 3.42$, $p = 0.001$, $d = 0.97$, 95% CI[0.36, 1.37]. Participants in the ambiguous color condition ($M = 2.68$, $SD = 0.86$) reported being significantly more uncertain about the task than participants in the control condition ($M = 1.82$, $SD = 0.91$). Thus, the manipulation had the intended effect.

Mediation

Self-reported uncertainty was negatively correlated with both the number of attempts, $r = -0.28$, $p = 0.05$, and the amount of time people spent on the unsolvable puzzle, $r = -0.42$, $p < 0.01$. We tested the mediating effect of condition on the dependent variables through self-reported uncertainty using the method recommended by Preacher and Hayes (2004). The indirect effect of condition on time persisting was estimated to be 164.67, 95% CI [58.72, 403.53]. Because the confidence interval does not include zero, this suggests that the indirect effect of condition on time persisting through self-reported uncertainty was significant. The indirect effect of condition on number of puzzle attempts through self-reported uncertainty was estimated at 2.19, 95% CI [-1.55, 6.65]. Because the confidence interval contains zero, this indicates that the indirect effect of condition through self-reported uncertainty on puzzle attempts was not significant.

The amount of uncertainty participants felt about the task mediated the relationship between their assigned condition and how long they persisted on the subsequent unsolvable puzzles, but not on how many attempts they made to solve the puzzle. This suggests that condition decreased time persisting by increasing uncertainty.

TABLE 2 | Study 3: Means and standard deviations across conditions on attempts and time on unsolvable puzzles and on self-reported uncertainty.

	Uncertain	Control
	M (SD)	*M (SD)*
Attempts	18.00* (11.41)	26.95* (16.38)
Time on puzzles	10.61 (7.35)	14.53 (8.63)
Self-reported uncertainty	2.68* (0.86)	1.82* (0.91)

*$p < 0.05$.

Discussion

Study 3's results converged with those of the first two studies, despite changes in both manipulation and dependent measure. We found once again that uncertainty impaired subsequent executive function. Participants who performed one task hampered by unclear, ambiguous instructions later quit more quickly on a separate, unrelated task, as compared to people for whom the initial instructions could be clearly and easily followed.

It is possible that other differences between the uncertain and certain condition (such as task difficulty) could have contributed to the poorer performance on the second task. However, the effect of condition on executive function was mediated by how uncertain participants reported feeling, which suggests that uncertainty is at least part of the reason for impaired performance on the subsequent task. The manipulation check indicated that the manipulation increased uncertainty, though not to extreme levels (2.68 out of maximum 5). This suggests that even a moderate amount of uncertainty is enough to impair executive function.

META-ANALYSIS

We conducted a meta-analysis to test the effect of uncertainty on executive function across studies. When two outcomes were measured (e.g., time persisting and number of attempts), both were included in the analyses for significance, and the effect sizes were combined following the guidelines for combining dependent effects (Rosenthal and Rubin, 1986). For studies with multiple contrasts, the comparison between the uncertain and speech conditions (the more conservative test) was used. We found that the effect of uncertainty on executive function was reliable, $Z = 4.67$, $p < 0.001$, and the effect size was small, $r = 0.101$.

GENERAL DISCUSSION

Three studies provided evidence that uncertainty impaired performance on subsequent self-control tasks, even though those tasks had no logical relationship to the previous experience of uncertainty. Participants who were left uncertain about whether they would have to give a speech showed impaired performance on the game Operation (Study 1) and on an anagram completion task (Study 2), as compared to participants in no speech or speech control conditions. In Study 3, participants who were given an unclear task gave up faster on a second unrelated task than participants who were given a clear first task.

The effect of uncertainty on self-control was robust across different experiences of uncertainty. We tested manipulations of uncertainty involving an unclear task and uncertainty in the form of waiting to find out whether one will have to perform an anxiety-producing task. The convergence across these different experiences of uncertainty increases confidence in the general conclusion that being uncertain leads to impairments in self-regulatory performance, even in domains unrelated to the uncertainty. We also found that feelings of uncertainty mediated the effects of uncertainty manipulations on subsequent self-control (Study 3).

Studies 1 and 2 indicated that the effects of uncertainty go beyond merely raising the possibility of a bad outcome. They showed that the uncertain possibility of a bad outcome caused more impairment than certainty that the bad outcome would occur. Specifically, participants who thought they might have to make a speech performed significantly worse than participants who faced the worst possible outcome, namely a definite assignment that they would have to give a speech. Uncertainty in the form of anticipating the *possibility* of a negative outcome thus impaired self-control more severely than certain anticipation of the same negative outcome. Previous research has found that people are willing to pay *less* for a chance at one of two outcomes (e.g., you will receive a $50 or $100 gift certificate) than for the worse outcome guaranteed (e.g., you will receive a $50 gift certificate; Gneezy et al., 2006; Simonsohn, 2009). Although choosing a less-positive certain outcome over an uncertain outcome may seem irrational, it may sometimes be worth avoiding the psychological costs of experiencing uncertainty, namely impairments to executive function.

Alternative Explanations

Our findings cannot establish whether uncertainty actually causes cognitive fatigue or merely mimics it. In practice, the difference to the decision-maker may be trivial. In either case, the person may automatically shift toward less effortful modes of deciding (Pocheptsova et al., 2009; Pohl et al., 2013). These conserve energy but reduce the role of rational input into the decision process.

One might argue that our results were obtained because uncertainty distracted participants in the moment, rather than necessarily impairing self-control on a subsequent task. Study 3 provides some evidence against this. Although in studies 1 and 2, participants were uncertain while completing the dependent measure of self-control, in Study 3, the uncertainty manipulation and subsequent measure of self-control were distinct tasks. Participants first completed the task on which they were made to feel uncertain. Self-regulatory deficits were found on a subsequent, separate, and unrelated task, and it is unlikely that while participants were working to solve the figure tracing puzzles they were still ruminating about whether the color squares they had seen earlier had been red or blue. Also, as noted above, Study 1's measures also spoke against the alternative interpretation that the uncertainty condition caused people to think the experimenter was incompetent.

Implications

The idea that uncertainty can impair self-control has diverse potential for advancing ego-depletion theory. We assume that participants in our studies did not deliberately, knowingly lower their performance based on exposure to unrelated uncertainty. Unconscious processes presumably mediated the link between experiencing uncertainty in one context and seeking to conserve volitional resources in another. Hence decisions about whether to exert effort may be influenced by multiple factors, only some of which are conscious.

The analogy of executive function fatigue to a muscle was creatively extended by Evans et al. (2015). They noted that feelings of muscular tiredness are only loosely linked to the physical condition of the muscle. Some brain processes presumably keep track of exertion and create a feeling signal of tiredness to promote energy conservation. Our findings fit well with the suggestion that ego depletion also may be only distantly related to the actual availability of energy resources. Instead, various cues associated with past and future demands may prompt the individual to curtail self-regulatory effort. Our findings suggest that uncertainty may be one such cue. To be sure, conserving resources may often be a highly adaptive response to uncertainty — even though in our experimental situation, it brought no benefits.

We cited evidence that people in uncertain conditions suffer problems of mental and physical health (e.g., Wiggins et al., 1992; Burgard et al., 2009). Impaired self-control may prove to be a mediating factor, if people struggling with uncertainty cease to control their eating and alcohol consumption, curtail their health behaviors, mistreat relationship partners, or fail to regulate their emotions. The present research suggests that delivering clear news quickly to patients (when possible) may make it easier for them to make important choices or follow demanding treatment regimens than if information is delayed or unclear.

The importance of self-control to smooth societal functioning suggests that large-scale uncertainty could have a variety of troublesome effects. Uncertainty may disproportionately be present for certain social classes (such as low-income groups) or may periodically affect society as a whole (such as in times of economic downturns, political turmoil, or public health crises). Crime, addiction, intimate partner violence, and general impulsivity might all increase. These would compound the problems facing society that gave rise to the original uncertainty.

Limitations and Future Directions

There are a few reasons to interpret the presented results with caution. As mentioned in the introduction, these studies were conducted before new standards for pre-registration and large sample sizes were established. Research conducted in the future on this topic should follow the current standards in the field. It is also worth noting that the internal meta-analysis is only based on a small number of studies. Although the hypothesis was always that uncertainty would impair performance, we tested a number of exploratory hypotheses across studies about potential mediators and moderators. None of these predominantly non-significant results advanced the theory, and all are reported in the **Supplementary Materials**. We also note that the effect of uncertainty was replicated, so the impairment of executive function caused by uncertainty is robust, even though we were unable to find evidence of a specific mechanism in these studies.

Although uncertainty was our primary independent variable, we cannot claim to have studied all forms of uncertainty. Undoubtedly there are some differences among the varieties of uncertainty (Kahneman and Tversky, 1982), even in our studies. However, for both manipulations, participants were made aware that they lacked highly relevant information. It is this lack of knowledge that ultimately results in poorer subsequent self-control, regardless of the exact kind of knowledge that is lacking. We deliberately broadened our investigation to encompass multiple forms of uncertainty (rather than operationalizing it in the same way in all studies) to increase generalizability and ensure that our results were not due to one particular method or one kind of uncertainty. Research using other measures of uncertainty (e.g., a scratched vs. unscratched lottery ticket) have shown that people are more likely to choose "wants" over "shoulds" when uncertain, which provides additional evidence that uncertainty has a negative effect on self-control (Milkman, 2012). The convergence of results across these different uncertainties increases our confidence that the pattern is indeed a relatively general one.

Future work may test whether there are situations in which some kinds of uncertainty would not impair self-control. For example, uncertainty about a definite positive outcome (e.g., uncertainty about which online interaction partner said which positive thing about the participant) has been shown to increase the duration of positive affect (Wilson et al., 2005; Kurtz et al., 2007). Because positive mood has been shown to eliminate the effects of one act requiring self-control on subsequent self-control (Tice et al., 2007), it is possible that the net effect of a purely positive uncertainty may be neutral or even restorative. However, when a negative possibility exists, the present evidence suggests that people will be less likely to perform well at executive function if they have recently been or are currently uncertain.

CONCLUSION

Uncertainty increases the difficulty of decision-making (Shafir, 1994). When all relevant facts are known, decision processes can be a fairly straightforward product of logic, preference, and goals or values. Often, however, decisions must be made when key facts are lacking (Orasanu and Connolly, 1993). Uncertainty hampers the decision maker directly, because it makes it difficult to calculate which option will yield best results. Our findings suggest a second way in which uncertainty impairs decision makers: It makes them act as if they had cognitive fatigue.

DATA AVAILABILITY STATEMENT

The raw data supporting the conclusions of this article will be made available by the authors, without undue reservation.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by Florida State University Institutional Review Board; Texas Tech Review Board. The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

DT, JA, and RB discussed and developed the idea. JA designed Studies 1 and 3 with input from RB and DT and designed Study 2 with input from all other co-authors. DT designed the manipulation from Study 3. JA and RB wrote the introduction and discussion, with input from the other co-authors. JA executed Studies 1–3, and analyzed and wrote the methods and results for Studies 1 and 3. TC analyzed and wrote the methods and results for Study 2.

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SUPPLEMENTARY MATERIAL

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fpsyg.2020.576001/full#supplementary-material

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ORIGINAL RESEARCH
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Manipulation of the Duration of the Initial Self-Control Task Within the Sequential-Task Paradigm: Effect on Exercise Performance

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Self-control exertion on an initial task has been associated with impaired performance on subsequent physical tasks also requiring self-control; an effect suggested to be mediated by changes in perceptions of pain and motivation. However, the effects of spending longer on the initial self-control task are unknown. This study, therefore, explored the potential for the duration of the initial self-control task to influence subsequent physical performance, perceptions of pain, and perceived motivation; particularly during the early stages of the physical task. In a within-subject design, 29 participants (11 male, 18 female) completed a wall-sit task until volitional exhaustion, on four separate occasions. Prior to each wall-sit, participants completed either a non-self-control task (congruent Stroop task) for 4 min, or a self-control task (incongruent Stroop task) for 4 (short duration), 8 (medium duration), or 16 (long duration) min. Participant's perceptions of pain and motivation were recorded every 30 s during the wall-sit. Wall-sit performance time was analyzed using one-way ANOVA and perceptions of pain and motivation analyzed using multi-level modeling. Wall-sit performance time was significantly longer on the non-self-control exertion trial compared to all other trials (all $p < 0.01$), as well as longer on both the short duration and medium duration self-control exertion trials compared to the long duration self-control exertion trial (both $p < 0.001$). Perceptions of initial (at 30 s) pain and motivation were different between the trials (main effect of trial: pain, $p = 0.001$; motivation, $p < 0.001$); whereby longer durations of self-control exertion increased perceptions of pain and decreased motivation. The decrease in motivation during the wall-sit task was greater on the long duration self-control exertion trial compared to all other trials (trial*time interactions, all $p < 0.05$). The present study provides novel evidence that spending longer on the initial self-control task led to greater detrimental effects on subsequent wall-sit performance time. Furthermore, longer duration self-control exertion tasks led to increased perceptions of pain and decreased motivation within the first 30 s of the wall-sit task, as well as a greater decrease in motivation across the wall-sit task. These attentional and motivational shifts may explain performance decrements following the exertion of self-control.

Keywords: ego depletion, pain, motivation, Stroop task, physical performance

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INTRODUCTION

Self-control is defined as the ability to volitionally regulate dominant impulses or urges to bring them in line with more desirable, long-term goals (Baumeister et al., 1998). Self-control helps individuals to exhibit appropriate behavior by helping to regulate urges, juggle competing goals, and to maintain focus on the desired goal (Baumeister et al., 2007). High levels of self-control have been linked with numerous adaptive behaviors from a variety of contexts; including enhanced psychological well-being, higher levels of achievement and performance, and improved interpersonal relationships (e.g., Tangney et al., 2004; Baumeister et al., 2007; De Ridder et al., 2012). In addition, self-control has been shown to affect athletic performance (Englert, 2016), whereby it is essential for athletes to control their cognitive, emotional, and motor processes, in addition to their behavioral tendencies (Englert and Bertrams, 2012; Wagstaff, 2014).

The capacity to exert self-control can differ both between individuals (i.e., trait self-control), as well as across situations within the same individual (i.e., state self-control; Tangney et al., 2004). Concerning state self-control, recent meta-analytic evidence has emphasized that the initial exertion of self-control on one task, impairs performance on a subsequent, ostensibly unrelated task also requiring self-control (Hagger et al., 2010; Dang, 2017; Giboin and Wolff, 2019; Brown et al., 2020). However, a Registered Replication Report did not find support for this depletion effect (Hagger and Chatzisarantis, 2016); with some researchers suggesting that publication bias may have led to an overestimation of the size of the effect (Carter et al., 2015; Wolff et al., 2018). However, many recent commentaries, analyses, and debates have implied that although the size of the depletion effect is likely smaller than previously suggested, it is too early to reject the effect altogether (e.g., Baumeister and Vohs, 2016; Sripada et al., 2016; Blázquez et al., 2017).

Within the literature to date, the completion of various self-control tasks (e.g., completing an incongruent Stroop task, transcribing a neutral text while omitting the letters "e" and "n," suppressing emotions during an upsetting movie) have impaired performance on subsequent physical tasks including a wall-sit task (Boat et al., 2018), cycling performance (Wagstaff, 2014; Englert and Wolff, 2015; Boat et al., 2017), press-up and sit-up tasks (Dorris et al., 2012), as well as skill-based tasks (Englert and Bertrams, 2012; McEwan et al., 2013). While it is important to note that there is some contrasting research (Hagger and Chatzisarantis, 2016), overall the evidence base suggests that self-control exertion seems to have a negative effect on subsequent physical performance (Giboin and Wolff, 2019; Brown et al., 2020).

The shifting priorities model (Inzlicht et al., 2014; Inzlicht and Schmeichel, 2016) has recently been applied to explain self-control failures in a multitude of performance contexts, including sport and exercise settings. The core assumption of this model is that following the exertion of self-control, individuals experience shifts in motivation and attention that undermines performance on subsequent tasks that also require self-control (Inzlicht et al., 2014; Inzlicht and Schmeichel, 2016). A number of physical tasks that have been employed in previous self-control research are unpleasant and induce elevated levels of discomfort and pain (e.g., Dorris et al., 2012; Englert and Wolff, 2015). An essential function of pain is to disturb and stimulate attention (Eccleston and Crombez, 1999). Thus, perceptions of pain during physically effortful tasks can be utilized as a measure of attentional shifts within the shifting priorities perspective (Boat and Taylor, 2017). For instance, following prior self-control exertion, recreationally active participants described higher perceptions of pain and decreased motivation during the initial stages of a wall-sit task, which resulted in reduced performance on the wall-sit task; relative to when they did not initially exert self-control (Boat and Taylor, 2017; Boat et al., 2018). Although initial evidence appears to support the shifting priorities model, further research is required to test the mechanisms of this model (Englert, 2019). For instance, examining changes in perceptions of pain and motivation to perform subsequent task goals, throughout a physical performance task, have not been examined to date, and would provide a novel insight into the mechanisms underpinning the shifting priorities model and how this affects subsequent performance.

Recent literature relating to the shifting priorities model of self-control is consistent with reward-based models of self-control, whereby individuals weigh the benefits of pursuing a specific task against its costs (Kurzban et al., 2013; Wolff and Martarelli, 2020). In other words, during an endurance task, individuals repeatedly appraise the pros and cons of decreasing or sustaining effort to perform optimally. For example, the accumulating sensations of pain and discomfort during a prolonged, high-intensity endurance task can encourage an individual to gradually focus on relieving the pain, and eventually the cons (i.e., pain) outweigh the pros (i.e., optimal performance) of continuing the endurance task and participants choose to quit (Taylor et al., 2018).

Support for these models comes from a substantial evidence base suggesting that performance on subsequent physical tasks is reduced following self-control exertion (e.g., Dorris et al., 2012; Wagstaff, 2014; Englert and Wolff, 2015). Typically, experimental protocols have consisted of two unrelated tasks requiring self-control, commonly referred to as the sequential-task paradigm (Baumeister et al., 2007). Within the sequential-task paradigm, the experimental (self-control) group/condition requires participants to exert self-control on both tasks. Conversely, in the control (non-self-control) group/condition, the initial task does not require any, or very little, self-control (Baumeister et al., 1998). Typically, the self-control tasks utilized require the alteration or modification of an instinctive, well-learned response, similar to resisting an impulse or temptation (Baumeister et al., 2007). Research suggests that when the initial task requires self-control, performance on the second self-control task will be impaired, relative to when the first task does not require self-control (Baumeister et al., 1998).

Within the sequential task paradigm, the duration of the initial self-control task appears inconsistent throughout the literature (Brown and Bray, 2017); however, the majority of the primary self-control tasks are relatively brief in duration (typically 4–15 min; Giboin and Wolff, 2019). In contrast, mental fatigue research utilizes initial tasks that are 30 min or longer, and typically ∼90 min in duration (e.g., Van Cutsem et al., 2017). Therefore, it has been argued that typical self-control depletion tasks are not long enough to lead to subjective feelings of mental fatigue (Pageaux et al., 2013). In addition, regarding self-control, all studies to date have only examined one duration of initial self-control exertion; research has not manipulated the initial task duration within the sequential-task paradigm, or considered the effect on physical performance during the second self-control task (Hagger et al., 2010; Lee et al., 2016; Giboin and Wolff, 2019). While recent research has demonstrated that different durations of the initial self-control task did not affect subsequent cognitive performance (Wolff et al., 2019), it is currently unknown whether longer durations of self-control exertion could have a greater detrimental effect on subsequent physical performance. Spending longer on the initial self-control task may lead to greater shifts in motivation and attention (Inzlicht and Schmeichel, 2016), exacerbating the performance decrements on a subsequent physical task, also requiring self-control.

Building on the literature discussed above, the aims of the current research were to explore: (a) the potential for the initial self-control task duration to moderate any decrements in performance on a subsequent physical task and (b) whether exerting self-control increases perceptions of pain and reduces perceptions of motivation during a subsequent physical task. Based on the broad self-control literature (e.g., Dorris et al., 2012; Inzlicht and Schmeichel, 2016; Boat and Taylor, 2017), it was hypothesized that spending longer on the initial self-control task would result in an increased deleterious effect on subsequent wall-sit task performance (hypothesis 1). In addition, it is hypothesized that self-control exertion will lead to increased perceptions of pain, and reduced perceptions of motivation, during the wall-sit task (hypothesis 2).

MATERIALS AND METHODS

Participants

The sample consisted of 29 participants (11 male, 18 female) aged 18–22 years old (M age = 20.7 years, SD = 0.8 years). On average, the participants exercised on 3 days (SD = 2 days) per week. All participants were healthy, as determined by a University approved general health questionnaire. A power calculation (G*Power version 3.1; Faul et al., 2007) with power = 0.95 and $\alpha = 0.05$ (ANOVA repeated measures, within factors), specified a minimum sample size of $N = 23$ would be satisfactory to detect a medium effect size (0.40), which is representative of previous self-control studies (Giboin and Wolff, 2019; Brown et al., 2020).

Procedures

Following ethical approval, the study was explained in full to participants (including that their participation was anonymous and voluntary). Participants then signed an informed consent form. In addition, participants were asked to refrain from strenuous physical activity and alcohol consumption for 24 h before the start of each trial. Participants took part in four experimental sessions in total (separated by at least 48 h).

Experimental Protocol

On arrival in the laboratory, participants first completed questionnaires to control for the influence of daily stress (see section "Measures"), given the potential for stress to influence the effects of self-control exertion on subsequent performance (Tangney et al., 2004; Englert and Rummel, 2016). Participants were then familiarized with the wall-sit procedure. Individuals were instructed to lean with their back against a wall, hips and knees bent at 90°, feet shoulder width apart, with their hands resting against the wall (Boat et al., 2018). This task requires self-control as the procedure becomes increasingly painful and requires individuals to persist at the task, rather than quit the wall-sit, to relieve the associated pain (Boat and Taylor, 2017; Boat et al., 2018). The physical task instructions were scripted so that they remained the same for all trials. Individuals practiced the wall-sit task once to ensure that they were familiar with it and understood the task requirements. This procedure has been used successfully in similar self-control research (e.g., Boat and Taylor, 2017; Boat et al., 2018).

Participants were then required to complete either a non-self-control task (congruent Stroop task) for 4 min, or a self-control task (incongruent Stroop task) for 4 (short duration), 8 (medium duration), or 16 min (long duration). Self-control manipulation took place via a modified Stroop task (Stroop, 1935), which is well established and commonly used in the self-control literature (e.g., McEwan et al., 2013; Englert and Wolff, 2015; Boat et al., 2017). Furthermore, these durations of the Stroop task were utilized as previous research has employed this task for the same length of time (i.e., 4 min; Boat and Taylor, 2017). Also, 8 and 16 min reflect a 200 and 400% increase in duration, respectively, thus reflecting a suitable variance for differences to be observed and is in line with previous research (e.g., Wolff et al., 2019).

In the Stroop task, a word (always a color) was displayed in the center of a computer screen, and participants were required to select the correct response using a response pad. In the congruent version of the Stroop task (non-self-control exertion), the word and the print color were congruent (e.g., the word "green" was printed in green ink). In the incongruent version of the Stroop task (self-control exertion), the word itself and the print color were incongruent. For instance, if the word "green" was printed in blue ink, the correct keypad response would be the blue button. The incongruent Stroop task requires self-control because participants have to inhibit their natural response to name the word rather than the ink color (e.g., McEwan et al., 2013; Englert and Wolff, 2015; Boat et al., 2018). Stimuli were presented on the screen one at a time, and remained until a response was registered. The Stroop task was completed in a quiet room and participants were asked to respond as quickly and as accurately as possible. Prior to the actual test, participants completed a brief (30 s) practice session to re-familiarize themselves with the requirements of the Stroop task. Immediately following the Stroop task, participants completed a manipulation check (CR-10 Scale; Borg, 1998), which assessed their perceived mental effort during the cognitive task (see section "Measures").

Immediately following the completion of the CR-10 scale, participants performed the wall-sit. Participants were instructed to hold the position for as long as possible, until volitional exhaustion (i.e., the point at which participants chose to give up on the task, as they could no longer hold the correct wall-sit positioning). The time started as soon as participants were in the correct wall-sit position. The time was stopped when participant's knees, extended above or flexed below, the required 90° angle they were asked to hold throughout the wall-sit. Overall, participants performed four wall-sits under four experimental conditions: non-self-control task (congruent Stroop task) for 4 min, or a self-control task (incongruent Stroop task) for 4 (short duration), 8 (medium duration), or 16 min (long duration). The order of the sessions was counterbalanced to eliminate order effects. Throughout the wall-sit task, participants' perceptions of pain and motivation were recorded every 30 s (see section "Measures").

Measures

Daily Stress

The Daily Inventory of Stressful Events Questionnaire (Almeida et al., 2002) was utilized to measure participants' daily stress. Participants were instructed to indicate whether or not a number of stressful events had occurred on the day (e.g., "Anything at work or university that most people would consider stressful"). This questionnaire has been shown to have high internal consistency and predictive validity (Almeida et al., 2002).

Mental Exertion

Borg's single-item CR-10 scale (Borg, 1998) was completed to measure mental exertion following the Stroop task (0 = extremely weak; 10 = absolute maximum). This questionnaire has been used extensively in previous self-control research (e.g., McEwan et al., 2013; Boat et al., 2018).

Perceptions of Pain and Motivation

A Visual Analog Scale (VAS), adapted from the short-form McGill pain questionnaire (Melzack, 1987), was used to measure participant's perceptions of pain, and motivation to continue the wall-sit task, every 30 s during the wall-sit. Both VAS scales consisted of a 10 cm line ("no pain" to "worst possible pain"; "zero motivation to continue" to "full motivation to continue") with participants' responding according to their perceived pain and motivation at that point in time. The VAS has demonstrated acceptable predictive validity and reliability (Wright et al., 2001) and has been successfully utilized in previous self-control research (e.g., Boat and Taylor, 2017; Boat et al., 2018).

Task Performance

Performance was measured using the time (in seconds) participants quit the wall-sit task. Quitting the wall-sit task was considered as the moment when participant's knees, extended above or flexed below, the required 90° angle they were asked to hold the wall-sit.

Statistical Analysis

Data were analyzed using SPSS (version 25; SPSS Inc., Chicago, IL, United States). To check for baseline differences between the trials, stress, fatigue, and mental exertion were analyzed using one-way repeated measures analysis of variance (ANOVA), with Bonferroni-corrected paired samples *t*-tests used as *post hoc* testing where significant differences existed. Wall-sit performance time was also analyzed using one-way repeated measures ANOVA (with Bonferroni-corrected paired samples *t*-tests as *post hoc* testing, with effect sizes calculated as Cohen's *d*).

Due to the different number of data points between participants and experimental trials for perceptions of pain and motivation (given these were measured every 30 s), multi-level modeling was used to analyze these data. These analyses were conducted in the open-source software R (version 3.5.1[1]). First, data were transformed to ensure a normal distribution (due to the left-hand skew and right-hand skew of pain and motivation data, respectively). All parameter estimates were "untransformed" prior to reporting, for ease of interpretation. Subsequently, linear mixed effect models were applied using the *lme* function (which yields "*t*" statistics), utilizing a trial * time approach, with a random effect (intercept) for each participant included in all models. To gain a greater insight, trial was converted to a factor, to allow comparisons between each of the experimental trials. Further separate linear mixed effect models were conducted for initial (i.e., at 30 s into the wall-sit task) perceptions of pain and motivation, due to the aforementioned evidence suggesting that shifts in pain and motivation may occur early in the wall-sit task (Boat and Taylor, 2017; Boat et al., 2018). Furthermore, to examine how initial pain and initial motivation affected wall-sit performance time, linear mixed effect models were conducted. For these models, the dependent variable was wall-sit performance time and the independent variables were trial, initial pain, and initial motivation. To compare model fit, Akaike information criteria (AIC) and Bayesian information criteria (BIC) were used, with smaller AIC and BIC values indicating that the independent variables explain a greater amount of the variance in the dependent variable. For all analyses, statistical significance was accepted as $p < 0.05$.

RESULTS

Pre-trial Manipulation Checks

There was no difference at baseline between the trials for stress ($p = 0.734$) or fatigue ($p = 0.388$). However, the manipulation of self-control did affect mental exertion [main effect of trial, $F_{(3,84)} = 77.1$, $p < 0.001$]. Upon further inspection, pairwise comparisons revealed mental exertion was significantly different between all trials (non-self-control exertion: 0.8 ± 0.1; short duration self-control exertion: 2.5 ± 0.2; medium duration self-control exertion: 3.9 ± 0.3; long duration self-control

[1]www.r-project.org

exertion: 5.5 ± 0.4; all pairwise comparisons, $p < 0.001$). These findings confirm the manipulation of self-control.

Wall-Sit Performance Time

Overall, wall-sit performance time was significantly different between the trials [main effect of trial, $F_{(3,84)} = 22.7$, $p < 0.001$; **Figure 1**]. Upon further inspection, wall-sit performance time was significantly longer on the non-self-control exertion trial (166 ± 9 s, range 98–305 s), compared to all other trials [short duration self-control exertion: 148 ± 9 s, range 74–263 s, $t_{(28)} = 2.8$, $p = 0.008$, $d = 0.38$; medium duration self-control exertion: 140 ± 9 s, range 71–295 s, $t_{(28)} = 3.9$, $p = 0.001$, $d = 0.53$; long duration self-control exertion: 116 ± 8 s, range 70–234 s, $t_{(28)} = 9.4$, $p < 0.001$, $d = 1.13$]. Wall-sit performance time was also significantly longer on both the short duration self-control exertion [$t_{(28)} = 5.1$, $p < 0.001$, $d = 0.71$] and medium duration self-control exertion [$t_{(28)} = 4.6$, $p < 0.001$, $d = 0.53$] trials, compared to the long duration self-control exertion trial. However, there was no difference in wall-sit performance time between the short duration and medium duration self-control exertion trials ($p = 0.270$, $d = 0.16$).

Perceptions of Pain

Overall, there was a difference in perceptions of pain between the trials [main effect of trial, $t_{(474)} = 3.2$, $p = 0.001$; **Table 1**]. Upon further inspection, perceived pain was significantly greater on the medium duration self-control exertion [$t_{(474)} = 2.2$, $p = 0.031$] and long duration self-control exertion [$t_{(470)} = 2.6$, $p = 0.011$] trials, compared to the non-self-control exertion trial. There was no overall difference in perceived pain between the other trials (all $p > 0.05$). All models demonstrated that perceived pain increased across time on all trials (main effect of time, all $p < 0.001$). However, the pattern of change in perceived pain across time was similar between all trials (trial * time interactions, all $p > 0.05$; **Table 1**).

Initial Perceptions of Pain

When considering initial (30 s) perceived pain, there was a significant difference between the trials [main effect of trial, $t_{(86)} = 3.3$, $p = 0.001$; **Figure 2**]. Specifically, perceived pain was greater on the long duration self-control exertion trial (4.8 ± 0.3) compared to the non-self-control exertion trial [3.6 ± 0.3; $t_{(84)} = 3.1$, $p = 0.003$] and short duration self-control exertion trial [4.0 ± 0.3; $t_{(84)} = 2.1$, $p = 0.042$]; and was also greater on the medium duration self-control exertion trial (4.4 ± 0.3) compared to the non-self-control exertion trial [$t_{(84)} = 2.0$, $p = 0.049$]. All other pairwise comparisons for initial perceptions of pain revealed no differences between the trials (all $p > 0.05$).

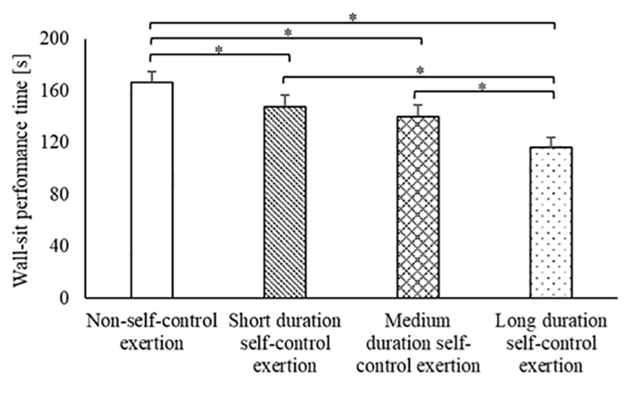

FIGURE 1 | Wall-sit performance time on all trials. Data are mean ± SEM (main effect of trial, $p < 0.001$; * indicates difference between trials, $p < 0.01$).

Motivation

Overall, there was a difference in motivation between the trials [main effect of trial, $t_{(474)} = -2.8$, $p = 0.005$; **Table 2**]. Upon further inspection, motivation was significantly greater on the non-self-control exertion trial compared to all other trials [main effects of trial: short duration self-control exertion, $t_{(470)} = -2.7$, $p = 0.007$; medium duration self-control exertion, $t_{(470)} = -2.1$, $p = 0.037$; long duration self-control exertion, $t_{(470)} = -2.7$, $p = 0.008$]. There was no overall difference in motivation between the self-control exertion trials (all $p > 0.05$). All models demonstrated that motivation decreased across time on all trials (main effect of time, all $p < 0.001$). The decrease in motivation across the wall-sit was greater on the long duration self-control exertion trial, compared to all other trials (trial * time interactions: non-self-control exertion, $t_{(470)} = -2.3$, $p = 0.022$; short duration self-control exertion, $t_{(470)} = -2.3$, $p = 0.023$; medium duration self-control exertion, $t_{(470)} = -2.1$, $p = 0.039$; **Table 2**). The pattern of change in motivation across time was similar between the other trials (trial * time interactions, all $p > 0.05$; **Table 2**).

Initial Perceptions of Motivation

When considering initial (30 s) motivation, there was a significant difference between the trials [main effect of trial, $t_{(86)} = -4.7$, $p < 0.001$; **Figure 3**]. Specifically, motivation was greater on the non-self-control exertion trial (6.5 ± 0.3) compared to all other trials [main effect of trial: short duration self-control exertion, 5.0 ± 0.3, $t_{(84)} = -3.3$, $p = 0.001$; medium duration self-control exertion, 5.0 ± 0.4, $t_{(84)} = -3.3$, $p = 0.001$; long duration self-control exertion, 4.2 ± 0.4, $t_{(84)} = -5.0$, $p < 0.001$]. All other pairwise comparisons for initial motivation revealed no differences between the trials (all $p > 0.05$).

Factors Affecting Wall-Sit Performance Time

Table 3 presents the models examining how initial pain and initial motivation affected wall-sit performance time. The addition of initial pain and initial motivation separately to models 2 (AIC = 1113.6; BIC = 1127.2) and 3 (AIC = 1131.7; BIC = 1145.3), respectively, reduced the AIC and BIC compared to model 1 (AIC = 1134.4; BIC = 1145.4), indicating that both variables explain some of the variance in wall-sit performance time. Furthermore, the addition of initial pain and initial motivation to the same model (model 4) reduced the AIC and BIC further (AIC = 1111.6; BIC = 1127.9), suggesting that both of these variables contribute to explaining the variance in wall-sit performance time.

TABLE 1 | Results of the multilevel models conducted for perceptions of pain.

	Baseline trial	Comparison trial	Intercept	Parameter estimate	95% CI	*t*	*p*
Main effect of trial	Non-self-control exertion	Short duration self-control exertion	1.76	5.68	4.79, 6.53	3.17	0.002
		Medium duration self-control exertion		5.98	5.10, 6.81	16.40	< 0.001
		Long duration self-control exertion		6.25	5.30, 7.12	0.89	0.376
	Short duration self-control exertion	Medium duration self-control exertion	2.35	5.31	4.39, 6.21	0.66	0.507
		Long duration self-control exertion		5.59	4.59, 6.55	1.16	0.246
	Medium duration self-control exertion	Long duration self-control exertion	2.58	5.28	4.39, 6.16	0.56	0.579
Trial * time interaction	Non-self-control exertion	Short duration self-control exertion	1.76	5.02	4.77, 5.27	0.14	0.892
		Medium duration self-control exertion		4.97	4.71, 5.22	−0.25	0.802
		Long duration self-control exertion		5.29	4.96, 5.61	1.75	0.082
	Short duration self-control exertion	Medium duration self-control exertion	2.35	4.95	4.68, 5.22	−0.36	0.717
		Long duration self-control exertion		5.27	4.94, 5.60	1.58	0.115
	Medium duration self-control exertion	Long duration self-control exertion	2.58	5.32	4.98, 5.65	1.85	0.065
Initial pain	Non-self-control exertion	Short duration self-control exertion	3.59	0.40	−0.35, 1.15	1.04	0.303
		Medium duration self-control exertion		0.77	0.02, 1.52	2.00	0.049
		Long duration self-control exertion		1.19	0.44, 1.94	3.10	0.003
	Short duration self-control exertion	Medium duration self-control exertion	3.99	0.37	−0.38, 1.12	0.96	0.338
		Long duration self-control exertion		0.79	0.04, 1.54	2.06	0.042
	Medium duration self-control exertion	Long duration self-control exertion	4.36	0.42	-0.33, 1.17	1.10	0.275

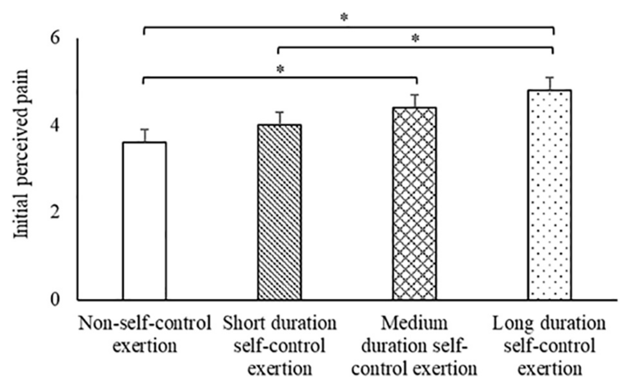

FIGURE 2 | Initial (30 s) perceptions of pain across the trials. Data are mean ± SEM (main effect of trial, $p = 0.001$; * indicates difference between trials, $p < 0.05$).

DISCUSSION

The present study examined the potential for the initial self-control task duration to moderate any decrements in performance on a subsequent physical task, and whether exerting self-control increased perceptions of pain and reduced motivation during a subsequent physical task. The findings provide novel evidence that spending longer on the initial self-control task led to greater detrimental effects on subsequent wall-sit performance time. Furthermore, a longer duration self-control exertion task led to increased perceptions of pain and decreased motivation within the first 30 s of, as well as a greater decrease in motivation across, the wall-sit task. Perceptions of pain and motivation may explain decrements in physical performance following the exertion of self-control.

A key finding of the present study was that a relatively brief (4 min) self-control exertion task led to impaired performance on a subsequent physical (wall-sit) task. Participants gave up quicker following a difficult cognitive task (requiring self-control), compared to when they completed a cognitively simple task (requiring no self-control). This is supported by previous research also demonstrating that a relatively brief self-control exertion task (i.e., 4–6 min) affects subsequent physical performance (e.g., Englert and Wolff, 2015; Boat and Taylor, 2017; Brown and Bray, 2017; Boat et al., 2018). Moreover, the findings significantly extend the extant literature by providing novel evidence that spending longer on the initial self-control task led to greater detrimental effects on subsequent wall-sit performance time. Participants persisted at the wall-sit task 32 s longer on average, when they exerted self-control for a short duration (i.e., 4 min) relative to when they exerted self-control for a long duration (i.e., 16 min); equivalent to a 28% improvement in performance. This is interesting given that recent research has suggested that the initial task duration is not associated with the magnitude of performance impairment for physical (Giboin and Wolff, 2019) or cognitive (Wolff et al., 2019) performance. However, it is important to highlight that prior cognitive exertion appears to have a greater negative influence on performance during subsequent isolation tasks (e.g., wall-sit task), compared to whole-body endurance tasks (e.g., cycling) (Giboin and Wolff, 2019). As such, varying physiological and psychological task demands may well contribute to this debate. Future studies could also examine the effects on "real world" sporting performance by employing ecologically valid physical endurance tasks that require self-control (e.g., cycling). This study provides initial evidence that longer durations of self-control exertion have a greater negative impact on subsequent physical performance. It is possible that differences in the size of the depletion effect across previous studies may well be a result of the variations in the duration of the initial self-control task (Lee et al., 2016).

Another key finding of the present study was that the exertion of self-control led to elevated perceptions of pain and reduced motivation during the first 30 s of the wall-sit task. These findings are in accordance with previous research (e.g., Boat and Taylor, 2017; Boat et al., 2018) and align well with the shifting priorities model of self-control (Inzlicht et al., 2014; Inzlicht and Schmeichel, 2016), whereby self-control exertion led to a state of elevated distress in the early stages of the wall-sit task (Elkins-Brown et al., 2017). This aversive state has been proposed to not only encourage individuals to attend to the presence of task goal conflict (i.e., quitting to relieve the pain versus persisting on the wall-sit task) (Baumeister and Bargh, 2014), but also encourage participants to prepare for actions to reduce this distressing state (Inzlicht and Legault, 2014). Accordingly, motivational priorities shifted toward an increased focus on the proximal tempting goal (i.e., quitting or reducing effort on the wall-sit task to relieve the pain), relative to the distal goal (i.e., persisting on the wall-sit task to optimize performance), resulting in reductions in performance following self-control exertion, in line with the shifting priorities (Inzlicht and Schmeichel, 2016; Milyavskaya and Inzlicht, 2018) and reward-based (Kurzban et al., 2013; Wolff and Martarelli, 2020) models of self-control. Of note, the findings of the present study suggest that both initial pain and initial motivation contribute to explaining the variance in wall-sit performance time following the depletion of self-control.

Previous research has only examined the effects of self-control exertion on perceptions of pain and motivation at the very early and final stages of the subsequent physical performance task (e.g., Boat and Taylor, 2017; Boat et al., 2018). The present study extends these findings by examining perceptions of pain and motivation throughout the wall-sit task, with the findings suggesting that participant's motivation decreased more rapidly during the wall-sit task on the long duration self-control exertion trial (i.e., 16 min). However, there were no differences in the pattern of change in perceptions of pain throughout the wall-sit task across the experimental trials. These findings imply that perceptions of pain and motivation in the early stages of the wall-sit task are a potential mechanism to explain the performance decrements following prior self-control exertion. The findings of the present study also suggest that long durations of self-control exertion influence motivation throughout the subsequent physical performance task. This novel finding has implications for the design of future interventions aimed at attenuating the effects of self-control exertion on subsequent physical performance. Intervention strategies that

TABLE 2 | Results of the multilevel models conducted for motivation.

	Baseline trial	Comparison trial	Intercept	Parameter estimate	95% CI	*t*	*p*
Main effect of trial	Non-self-control exertion	Short duration self-control exertion	7.52	−3.43	−2.47, − 4.54	−2.73	0.007
		Medium duration self-control exertion		−3.78	−2.76, − 4.92	−2.09	0.037
		Long duration self-control exertion		−3.34	−2.32, − 4.56	−2.65	0.008
	Short duration self-control exertion	Medium duration self-control exertion	6.13	5.38	4.18,6.53	0.62	0.537
		Long duration self-control exertion		−4.90	−3.63, − 6.19	−0.15	0.884
	Medium duration self-control exertion	Long duration self-control exertion	6.48	−4.53	−3.29, − 5.83	−0.71	0.476
Trial * time interaction	Non-self-control exertion	Short duration self-control exertion	7.52	5.02	4.69,5.34	0.11	0.916
		Medium duration self-control exertion		−4.98	−4.64, − 5.31	−0.15	0.885
		Long duration self-control exertion		−4.51	−4.10, − 4.93	−2.29	0.022
	Short duration self-control exertion	Medium duration self-control exertion	6.13	−4.96	−4.61, − 5.31	−0.24	0.815
		Long duration self-control exertion		−4.94	−4.51, − 5.38	−2.28	0.023
	Medium duration self-control exertion	Long duration self-control exertion	6.48	−4.54	−4.10, − 4.98	−2.08	0.039
Initial pain	Non-self-control exertion	Short duration self-control exertion	6.49	−1.53	−0.63, − 2.44	−3.32	0.001
		Medium duration self-control exertion		−1.52	−0.62, − 2.43	−3.29	0.001
		Long duration self-control exertion		−2.33	−1.43, − 3.24	−5.04	<0.001
	Short duration self-control exertion	Medium duration self-control exertion	4.95	0.01	−0.90,0.92	0.02	0.982
		Long duration self-control exertion		−0.80	−1.71,0.11	−1.73	0.088
	Medium duration self-control exertion	Long duration self-control exertion	4.96	−0.81	−1.72,0.10	−1.75	0.084

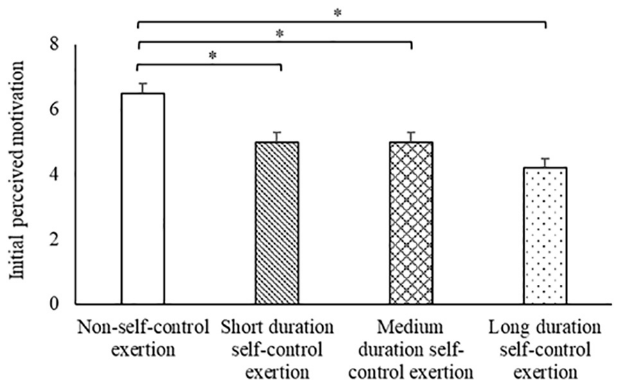

FIGURE 3 | Initial (30 s) perceptions of motivation across the trials. Data are mean ± SEM (main effect of trial, $p < 0.001$; * indicates difference between trials, $p < 0.01$).

target motivation throughout subsequent physical tasks, by reinforcing the value of distal goals (e.g., persisting on a physical task to optimize performance), or decreasing the worth of indulging in competing proximal goals (e.g., quitting or reducing effort on the physical task to relieve the pain) may help to reduce the rapid decline in motivation following self-control exertion (Taylor et al., 2018). Specifically, the findings of the present study suggest that future interventions should target initial perceptions of pain and motivation, as well as motivation throughout the subsequent physical task, to target the tenants of the shifting priorities model that were affected in the present study and ultimately enhance physical performance.

Limitations and Future Research Directions

Although yielding important findings, some limitations must be addressed. For example, performance on the initial self-control task (i.e., the Stroop task) was not examined. It is possible that individuals may have exerted differing amounts of self-control according to the extent to which they were motivated during the initial self-control task (Lee et al., 2016). While the CR-10 questionnaire confirmed the manipulation of self-control in the present study, monitoring performance on the Stroop task could provide an informative measure of participants' engagement and motivation during the initial self-control task (Lee et al., 2016). However, recent evidence has indicated that performance does not vary across different durations of the Stroop task (Wolff et al., 2019). In addition, although the participants in the current study were recreationally active (three times per week), we did not assess details of participants habitual physical activities. Future research could explore how habitual exercise habits may mediate the effects of self-control depletion on subsequent physical performance.

It is important to highlight that in the current study we utilized a 4-min control task (i.e., congruent Stroop task) as the reference performance for all self-control depleting conditions. Future research could compare self-control depleting tasks with the same duration (i.e., 8-min congruent Stroop task vs. 8-min incongruent Stroop task) to provide further insight into the potential for the duration of the initial self-control task to influence subsequent physical performance, perceptions of pain, and perceived motivation.

Furthermore, our findings are in line with the tenants of the shifting priorities model of self-control from a motivational and attentional viewpoint (Inzlicht et al., 2014; Inzlicht and Schmeichel, 2016). However, the use of objective measures of perceived pain and motivation may yield valuable insights into these underpinning mechanisms of the shifting priorities model. For example, electroencephalogram (EEG) and fNIRS activity of the prefrontal cortex could be utilized to examine the underlying motivational processes (Schmeichel et al., 2016). In addition, electromyography (EMG) of the facial muscles could be used to objectively measure perceptions of effort and pain (Huang et al., 2014), as well as eye-tracking to explore attentional focus (Kredel et al., 2017). Consequently, such methods would enable the objective exploration of shifts in motivational and attentional processes, following self-control exertion, while completing physically demanding tasks.

Finally, researchers should investigate additional mechanisms that may explain performance reductions following self-control exertion. For instance, recent research has suggested that within the sequential task paradigm, the initial self-control task is likely to induce forms of boredom, thus altering behavior and influencing performance on subsequent tasks that require self-control (Milyavskaya et al., 2019; Wolff and Martarelli, 2020). As such, task-induced boredom could be further investigated as

TABLE 3 | Model characteristics examining the factors affecting wall-sit performance time.

Model	Variable	p	AIC	BIC
1: Trial	Trial	< 0.001	1134.4	1145.4
2: Trial + initial pain	Trial	< 0.001	1113.6	1127.2
	Initial pain	< 0.001		
3: Trial + initial motivation	Trial	< 0.001	1131.7	1145.3
	Initial motivation	0.139		
4: Trial + initial pain + initial motivation	Trial	< 0.001	1111.6	1127.9
	Initial pain	< 0.001		
	Initial motivation	0.256		

AIC = Akaike information criteria; BIC = Bayesian information criteria.

a psychological factor that may explain performance reductions following self-control exertion.

CONCLUSION

The present study provides novel evidence that spending longer on the initial self-control task leads to greater detrimental effects on subsequent wall-sit performance time. Furthermore, the present study suggests that a longer duration self-control exertion task leads to increased perceptions of pain and decreased perceptions of motivation within the first 30 s of the wall-sit task, as well as a greater decrease in motivation across the wall-sit task. These attentional and motivational shifts may explain performance decrements following the exertion of self-control.

DATA AVAILABILITY STATEMENT

The raw data supporting the conclusions of this article will be made available by the authors, without undue reservation.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by the Nottingham Trent University Ethics Committee. The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

RB and SC designed the study and analyzed and interpreted the data. RB, RH, EW, AD, and ET collected the data. RB, RH, and SC drafted and revised the manuscript. All authors approved the final version of the manuscript.

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"Just One More Rep!" – Ability to Predict Proximity to Task Failure in Resistance Trained Persons

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In resistance training, the use of predicting proximity to momentary task failure (MF, i.e., maximum effort), and repetitions in reserve scales specifically, is a growing approach to monitoring and controlling effort. However, its validity is reliant upon accuracy in the ability to predict MF which may be affected by congruence of the perception of effort compared with the actual effort required. The present study examined participants with at least 1 year of resistance training experience predicting their proximity to MF in two different experiments using a deception design. Within each experiment participants performed four trials of knee extensions with single sets (i.e., bouts of repetitions) to their self-determined repetition maximum (sdRM; when they predicted they could not complete the next repetition if attempted and thus would reach MF if they did) and MF (i.e., where despite attempting to do so they could not complete the current repetition). For the first experiment ($n = 14$) participants used loads equal to 70% of a one repetition maximum (1RM; i.e., the heaviest load that could be lifted for a single repetition) performed in a separate baseline session. Aiming to minimize participants between day variability in repetition performances, in the second separate experiment ($n = 24$) they used loads equal to 70% of their daily isometric maximum voluntary contraction (MVC). Results suggested that participants typically under predicted the number of repetitions they could perform to MF with a meta-analytic estimate across experiments of 2.0 [95%CIs 0.0 to 4.0]. Participants with at least 1 year of resistance training experience are likely not adequately accurate at gauging effort in submaximal conditions. This suggests that perceptions of effort during resistance training task performance may not be congruent with the actual effort required. This has implications for controlling, programming, and manipulating the actual effort in resistance training and potentially on the magnitude of desired adaptations such as improvements in muscular hypertrophy and strength.

Keywords: perception, effort, physical, strength, exercise, performance

INTRODUCTION

Prolonged performance of physical tasks with fixed absolute demands results in a reduction in the capacity to meet their demands (i.e., fatigue), and thus a requirement for greater effort to maintain performance. As a result of this, the perception of that effort also increases (Horstman et al., 1979; Noakes, 2004). This appears to be the case over varying exercise modalities including both endurance and resistance training (Horstman et al., 1979; Pincivero et al., 2004; Marcora and Staiano, 2010). Though rating of perceived effort (RPE) scales are widely employed in physical tasks, scales have been developed that are aimed at utilizing the feedback from increasing perceptions of fatigue and effort in order to predict proximity to task failure (Coquart et al., 2012; Helms et al., 2016). The application of predictions of proximity to task failure has been a particularly popular approach within resistance training in recent years to manipulate and control the intensity of effort employed in a given bout (Hackett et al., 2012, 2016; Helms et al., 2016; Zourdos et al., 2016).

Within physical tasks such as resistance exercise the intensity of effort employed has been defined as the task demands (i.e., the load) relative to the current ability to meet those demands (i.e., a person's strength; Steele, 2014, 2020; Steele et al., 2017b, 2019). Considering this, *maximal* effort is anchored at the set endpoint where the participant reaches momentary task failure (MF, i.e., where despite attempting to do so the trainee cannot complete the current repetition; Steele, 2014; Steele et al., 2017b). MF has also been argued to be the most appropriate way to control for effort intra- and inter-individually (Dankel et al., 2016). However, to better understand applications of *submaximal* intensities of effort (i.e., set end-points that occur at different proximities to MF) 'repetitions in reserve' (RIR) scales have been developed and employed (Hackett et al., 2012, 2016; Helms et al., 2016; Zourdos et al., 2016). RIR scales assess or control effort by participants estimating how many repetitions they can perform before reaching MF. These scales have been argued to be a more valid method of representing effort during resistance training when compared to traditional RPE scales or the use of relative demands from a prior test of strength (i.e., % of one repetition maximum [1RM]; Hackett et al., 2012; Helms et al., 2016; Steele et al., 2017a). Indeed, traditional RPE scales often result in submaximal ratings even at MF (Steele et al., 2017c). Further, the numbers of possible repetitions prior to MF at the same relative loads (%1RM) vary between exercises and individuals (Steele, 2014; Steele et al., 2017a,b). Thus, RIR scales might provide a more accurate way of controlling for effort during resistance training. Further, predictive ability offers a behavioral test of the congruence of perception of effort and actual effort in resistance exercise tasks.

An assumption inherent in use of RIR scales to provide valid control of intensity of effort is that participants can accurately predict their number of repetitions until MF. Several recent studies have examined this predictive ability under a variety of conditions, including *a priori* to beginning the exercise (Steele et al., 2017a; Emanuel et al., 2020), and at varying proximities to MF during the exercise (Hackett et al., 2012, 2016; Altoé Lemos et al., 2017; Zourdos et al., 2019; Hughes et al., 2020; Mansfield et al., 2020). Most have shown that people are inaccurate in their predictions suggesting that, when using an RIR based prescription, they may be training at a lower actual effort than intended. This may have implications for training outcomes from interventions. A recent meta-analysis reported little difference between training to MF, or not (Grgic et al., 2020). However, some studies comparing groups training to MF and those who stopped at a self-determined repetition maximum (sdRM, i.e., when a person predicts they could not complete the next repetition if attempted and thus would reach MF if they did; Steele et al., 2017b) have shown greater responses when training to MF (Giessing et al., 2016a,b). This may be due to participants stopping further from MF than intended due to their poor ability to predict actual proximity to MF.

Throughout a bout of resistance exercise, the combined perceptions associated with that gestalt experience (i.e., perceived fatigue, effort, and discomfort) typically intensify with closer proximity to MF. Thus, we might expect the accuracy of prediction should increase the closer to MF a person is when they make it. Indeed, prediction has been shown to be more accurate when using heavier loads (i.e., where fewer repetitions are possible such that any given repetition is closer to MF; Altoé Lemos et al., 2017; Steele et al., 2017a). Further, accuracy increases with subsequent sets possibly due to practice, or lingering fatigue (Hackett et al., 2012; Emanuel et al., 2020; Mansfield et al., 2020). However, only one study has examined varying proximities to failure (Zourdos et al., 2019). Zourdos et al. (2019) examined the validity of predictions of 5RIR, 3RIR, and 1RIR (i.e., 5, 3, and 1 repetition in reserve). They found that accuracy improved with proximity to MF, but participants were still inaccurate even for 1RIR. Further, these were previously trained individuals. Indeed, it has been argued that RIR might be best applied in trained persons (Helms et al., 2016). Although, there is some contrasting evidence regarding the effect of prior experience on accuracy of prediction (Hackett et al., 2016; Steele et al., 2017a). Considering previous findings and the interest in quantifying effort through RIR scales, there is a need to examine this further. Indeed, given the increasing predictive accuracy with increasing proximity to MF, we might expect predictive ability to be at its greatest when participants are attempting to get as close to, but not reach, MF. The use of RIR implies *complete* repetitions that a person predicts they can perform. As such, 1RIR would mean that a person estimates they could perform one more complete repetition. Contrastingly, a 0RIR would mean they estimate that they would reach MF on the subsequent repetition (Helms, Personal Communication). No prior research has examined predictive ability for a 0RIR, or what Steele et al. (2017b) have referred to as the sdRM. Therefore, the aim of this study was to examine ability to predict proximity to MF at the sdRM/0RIR. In two separate experiments using a deception design, participants experienced in resistance training (>1 year) were tested over four trials whilst performing one set of knee extensions to either MF or sdRM.

MATERIALS AND METHODS

Experimental Approach

The study was approved by the Health, Exercise, and Sport Science ethics committee at Solent University (ID: standish-hunt2018). There were two separate experiments conducted in this study for which separate samples of participants were recruited. Testing procedures involved performing knee extensions on a knee extension dynamometer (MedX, Ocala, FL, United States; Experiment 1 and 2) or a knee extension resistance machine (Cybex, Medway, MA, United States; Experiment 1). In both experiments, participants underwent four resistance exercise trials involving single sets (i.e., bouts of repetitions) of knee extensions with at least 48 h in between to determine their ability to accurately identify their sdRM (i.e., 0RIR). Two of the resistance exercise trials were comprised of one set until their sdRM and the other two trials of one set until MF in a randomized order. To reduce demand characteristics (where participants' expectations of the experiments purpose might influence their performance) from invalidating the results, a deception was used blinding the participants to the actual goal of the study. Participants were informed that this was a reliability study examining similarities within the repeated identical condition trials (i.e., the reliability of sdRM or MF repetition performance between days). However, the study actually investigated the agreement between the different conditions. This was aimed at addressing participants consciously or unconsciously adapting their behavior, such that their apparent predictive ability was influenced (i.e., adjusting the number of repetitions performed in either condition to make it appear as though predictive accuracy was greater). In debrief after completion of the experiments, participants were asked whether they knew what the purpose of the study was to which all confirmed that they thought it was a reliability study as they were informed. Thus, it was confirmed that no participants had determined the true purpose of the study suggesting the deception had been successful.

Participants

Originally 11 participants were recruited for Experiment 1. From the initial data collected in Experiment 1 we produced an exploratory linear mixed model using the 'lme4' package (Bates et al., 2015) in R (version 3.6.1; R Core Team, 2020) to examine the fixed effect of condition adjusted for the fixed effect of day and allowing random intercepts by participant. Then, using the 'simr' package (Green and MacLeod, 2015), this model was extended to 100 participants and a simulation (1000 resamples) conducted to allow power curve analysis to be performed (see Supplementary Materials). Simulation showed that, for >80% power, ∼30 participants would be required at an alpha level of 0.05 and ∼25 participants at an alpha level of 0.1. As such, we aimed to recruit ∼30 for Experiment 2 to be able to exclude a zero effect. However, we were unable to achieve the intended 30 participants due to cessation of data collection as a result of 'lockdown' measures because of COVID-19. Hence, the final sample for Experiment 2 was 24 participants. An opportunity to collect additional data for Experiment 1 in another location and using a knee extension resistance machine (Cybex, Medway, MA, United States)[1] resulted in a final sample of 14 participants, but was also cut short due to the same reasons. Thus, the results of either experiment should be treated with caution individually. To somewhat overcome the sample issues, we conducted an internal meta-analysis (see below).

The final samples were $n = 14$ (11 males aged 22 ± 2 years and 3 females aged 20 ± 1 years) for Experiment 1, and $n = 24$ (20 male aged 27 ± 6 years and 4 females aged 24 ± 2 years) for Experiment 2. None of the participants took part in both experiments. Participants were required to have a resistance training experience of at least 1 year and to have abstained from any strenuous physical activity for 72-h prior to testing. All participants were provided with a participant information sheet including the deceptive purpose of the study and gave written informed consent. The participants had to complete a physical activity readiness questionnaire which covered any areas whereby there may be contraindications to the exercise (e.g., injury etc.). Participants were given the opportunity to withdraw from the study at any time and were debriefed after completion of the study.

Experiment 1: Resistance Exercise Trials Based on Baseline 70%1RM

The testing procedure of Experiment 1 involved one baseline 1RM test and four resistance exercise trials (2x sdRM; 2x MF) where one set of knee extension resistance exercise for each condition was performed. All conditions were performed in a randomized order and separated by at least 48 h. Within the baseline session, participants' range of motion (ROM) was determined by measuring their maximum knee extension and flexion angles. Following a warm-up using 50% of their estimated 1RM load, their 1RM was determined within a maximum of five attempts with 4-min rest between attempts. For some participants it was possible for the maximum resistance on the weight stack to be lifted for multiple repetitions and so 1RM was predicted using the Brzycki (1993) equation [predicted 1RM = load lifted/(1.0278 − (0.0278 × number of repetitions)] which has been shown to have a very high correlation to actual 1RM ($r = 0.99$; Nascimento et al., 2007). The load for the following four trials was calculated as 70% of their baseline 1RM. Subsequently, two sessions of submaximal sets to sdRM and two sessions of maximal sets to MF were performed.

Each session started with a warm-up involving one set of knee extensions at 50% of the calculated condition load with 8–10 repetitions, followed by a rest of 5 min after which the condition was performed. The previously determined ROM was set such that a 'beep' sound was provided by the dynamometer when at full extension/flexion to ensure that a full ROM was used for each repetition. Participants were instructed as follows. For the sdRM conditions they were instructed to, immediately upon completing a given repetition, consider whether they felt

[1]One of the researchers had moved during the study to a separate location and had access to a knee extension resistance machine. Thus, to contribute further data that might improve meta-analytic estimates (see the section "Statistical Analysis"), the researcher was able to recruit some additional participants and test them.

they could complete the next if attempted; if they did not think they could complete another if attempted they were to stop there and inform the investigator. For the MF conditions they were instructed to, immediately upon completing a given repetition, always attempt the next repetition; this was to continue until they reached a point where despite their maximal effort they could not complete the concentric portion of a repetition. The total number of completed repetitions were examined for each condition (i.e., the repetition chosen to stop on during sdRM, and the last complete repetition prior to MF). Participants were encouraged to think carefully about their sdRM prediction during that condition and push as close to, but not actually reach MF, and to perform with maximal effort for the MF condition.

Experiment 2: Resistance Exercise Trials Based on Daily 70%MVC

The testing procedure of Experiment 2 was the same as that used for Experiment 1 with one difference. We found that participants' repetition performances between the trials but within conditions were highly variable in Experiment 1, potentially attributed to individual day-to-day variabilities in preparedness (e.g., fatigue, mental state, stress, prior sleep, muscle glycogen concentrations etc.). Hence in Experiment 2, we opted to perform a daily maximal voluntary contraction (MVC) to examine participants' 'daily max performance' and allow us to normalize loads to each participants strength on the day of each resistance exercise trial. We chose MVCs as opposed to daily 1RMs, due to their brief nature and the minimal impact of fatigue that might affect the subsequent trial (Kennedy et al., 2015).

At the beginning of each session, following a warm-up and a practice isometric trial, participants performed an isometric MVC at 78° of flexion (previous testing in our lab suggests that most participants reach a peak torque at this angle) to determine their maximum voluntary torque in N·m. The load for each condition was thus calculated by 70% of their MVC in N·m for that day. The process of measuring MVCs was repeated before each session. Loads on the weight stack for the MedX Knee Extension are expressed in N·m and so we were able to normalize load against the MVC expressed in the same units. After a warm-up of 8–10 repetitions at 50% of their condition load followed by a rest of 5 min, the condition for that day was performed (i.e., sdRM or MF).

Statistical Analysis

The dependent variable was the number of complete repetitions performed and the independent variable was the condition (sdRM and MF). Linear mixed modeling using Restricted Maximum Likelihood Estimation was used for analysis. Condition was modeled as a fixed factor with random intercepts by participants included. As each condition was performed across two sessions (days), each participant had two pairs of sdRM:MF repetitions. Thus, day was also adjusted for in the model as a fixed factor. Estimated marginal means with 95% confidence intervals (CI) were produced using the "emmeans" package. Contrasts were produced using both 95% and 90% CIs to support inferences regarding equivalence. Equivalence bands were determined based upon the between day reliability of repetitions performed to MF within each study based upon the half-width of the minimal detectable change (MDC), sometimes referred to as the minimal difference, as typically suggested for examination of equivalence (Lesaffre, 2008). The MDC was calculated for the two repeated MF trials as:

$$MDC = SEM\ x\ 1.96\ x\ \sqrt{2}$$

Where,

$$SEM = SDd/\sqrt{2}$$

And the SDd is the standard deviation of the difference scores between the two trials (Weir, 2005).

Lastly, we combined the results from the two Experiments using an internal meta-analysis to obtain an overall effect estimate (Goh et al., 2016). The 'metafor' (Viechtbauer, 2010) package was used to perform a random effects meta-analysis weighted by sample size to produce effect estimates using both 95% and 90% CIs.

Inferences were drawn primarily regarding the magnitude and uncertainty of each outcome, whether it be close to zero or the equivalence bands. We opted to avoid dichotomizing the existence of an effect and therefore did not employ traditional null hypothesis significance testing, which has been extensively discussed (Amrhein et al., 2019; McShane et al., 2019). Instead, we consider the implications of all results compatible with these data, from the lower limit to the upper limit of the CIs, with the greatest interpretive emphasis placed on the point estimate. All effect estimates are reported in their raw units (number of repetitions) to facilitate practical interpretation.

RESULTS

Experiment 1: Resistance Exercise Trials Based on Baseline 70%1RM

The point estimate for the number of repetitions performed during the sdRM condition was 13.3 with the 95%CIs suggesting compatibility with a range of 11.6 to 15.0 repetitions. For the MF condition the point estimate was 14.1 repetitions with the 95%CIs suggesting compatibility with a range of 12.4 to 15.8 repetitions. The paired contrast showed that the number of repetitions performed during the MF condition was 0.8 greater than during the sdRM condition. The 95%CIs ranged −0.26 to 1.8 and thus did not exclude a possible effect estimate of zero, though included possible estimates of as high as 1.8 repetitions. The 90%CIs ranged from −0.1 to 1.6. Notably, considering the MDC for Experiment 1 (3.2 repetitions), neither the point estimate nor 95% or 90% estimate intervals excluded its upper bound thus suggesting equivalence within the range of the MDC between the repetitions performed in both conditions. **Figure 1** shows the individual paired comparisons (Session:Participant) across the conditions in addition to the paired contrast with both 95%CIs (gray band) and 90%CIs (black error bars) with the equivalence bands (dashed red line).

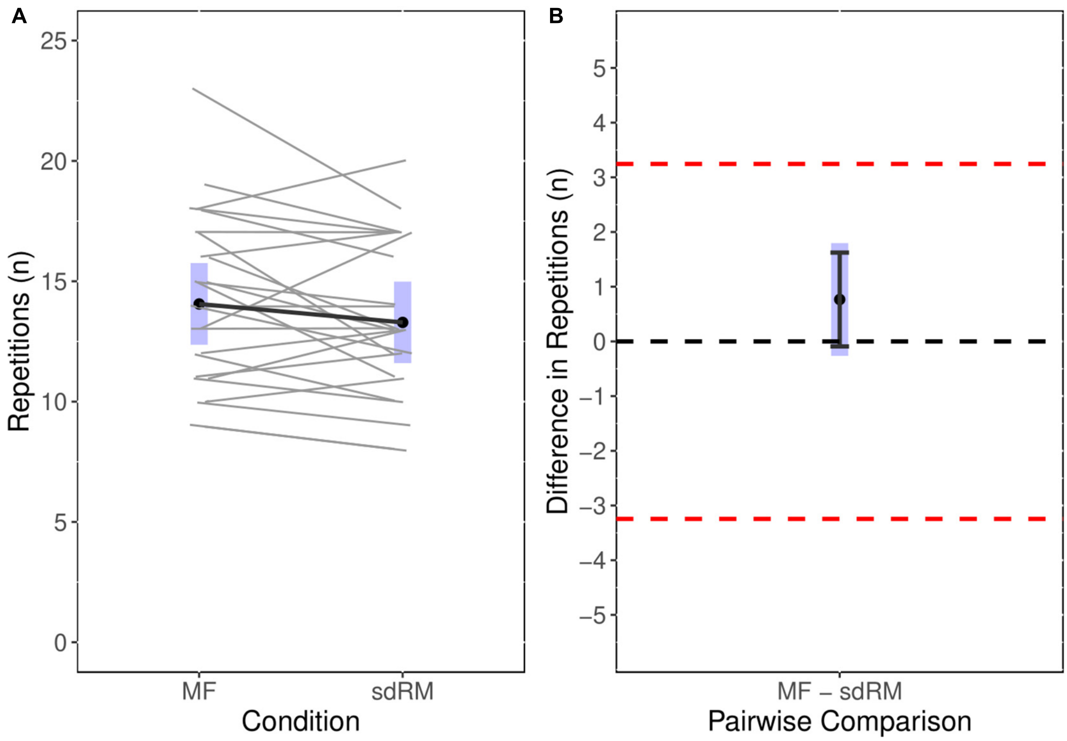

FIGURE 1 | Experiment 1: **(A)** Estimated marginal means with individual paired data for number of repetitions performed in MF and sdRM; **(B)** estimated marginal mean for the pairwise comparison between MF and sdRM with both 95%CIs (*gray* band) and 90%CIs (black error bars) with the equivalence bands (dashed red line). Individual data are presented as paired observations within days (i.e., sdRM day 1 was paired with MF day 1) as this was adjusted for within the model. MF, momentary failure; sdRM, self-determined repetition maximum.

Experiment 2: Resistance Exercise Trials Based on Daily 70%MVC

The point estimate for the number of repetitions performed during the sdRM condition was 11.6 with the 95%CIs suggesting compatibility with a range of 9.1 to 14.0 repetitions. For the MF condition the point estimate was 14.3 repetitions with the 95%CIs suggesting compatibility with a range of 11.9 to 16.8 repetitions. The paired contrast showed that the number of repetitions performed during the MF condition was 2.8 greater than during the sdRM condition. The 95%CIs ranged 1.5 to 4.0 and thus excluded a possible effect estimate of zero. The 90%CIs ranged from 1.7 to 3.8. Notably, considering the MDC for Experiment 1 (2.0 repetitions), the point estimate exceeded this; however, neither the 95% or 90% estimate intervals excluded its upper bound thus equivalence within the range of the MDC remains a possible compatible effect between the repetitions performed in both conditions. **Figure 2** shows the individual paired comparisons (Session:Participant) across the conditions in addition to the paired contrast with both 95%CIs (gray band) and 90%CIs (black error bars) with the equivalence bands (dashed red line).

Internal Meta-Analysis

The paired contrast estimate from the random effects meta-analysis showed that the number of repetitions performed during the MF condition was 2.0 greater than during the sdRM condition. The 95%CIs ranged 0.0 to 4.0 and thus just included a possible effect estimate of zero. The 90%CIs ranged from 0.3 to 3.7. **Figure 3** presents the forest plot with 95%CIs and **Figure 4** presents the forest plot with 90%CIs in addition to the upper equivalence bands from both Experiment 1 (dashed red line) and Experiment 2 (dashed blue line).

DISCUSSION

The results of the present study suggest on average participants under predicted the number of repetitions they could perform to MF. Compared to the actual number of complete repetitions in sets to MF, the number of complete repetitions in the sdRM condition were typically lower. However, in Experiment 1 this did not exceed the MDC. Thus, based upon the between day variability in repetition performance, the repetition numbers were inferred to be equivalent between conditions. For Experiment 2, as expected, there was a reduction in the between day variability as seen by the reduced MDC; indeed the intraclass correlation coefficient [3,1] for Experiment 1 was 0.5 (95%CI 0.03 to 0.8), and for Experiment 2 was 0.96 (95%CI 0.92 to 0.98). Results from Experiment 2 suggested more strongly that participants under predicted the number of repetitions they could perform to MF; though could still not wholly exclude an

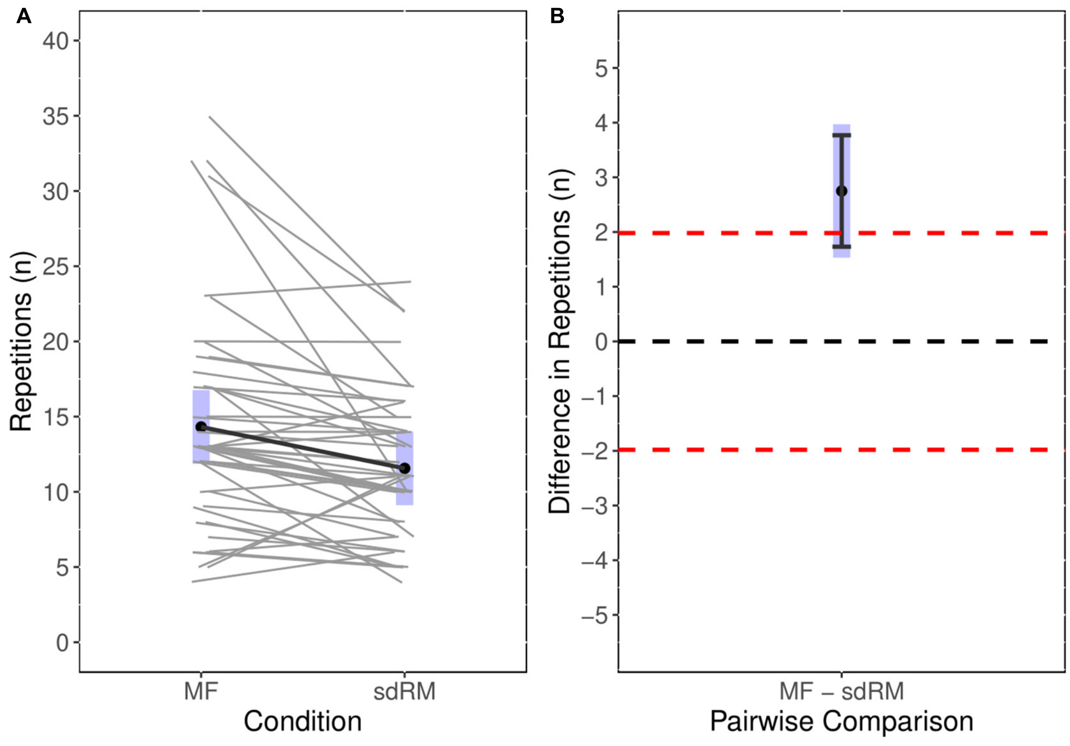

FIGURE 2 | Experiment 2: **(A)** Estimated marginal means with individual paired data for number of repetitions performed in MF and sdRM; **(B)** estimated marginal mean for the pairwise comparison between MF and sdRM with both 95%CIs (*gray* band) and 90%CIs (black error bars) with the equivalence bands (dashed red line). Individual data are presented as paired observations within days (i.e., sdRM day 1 was paired with MF day 1) as this was adjusted for within the model. MF, momentary failure; sdRM, self-determined repetition maximum.

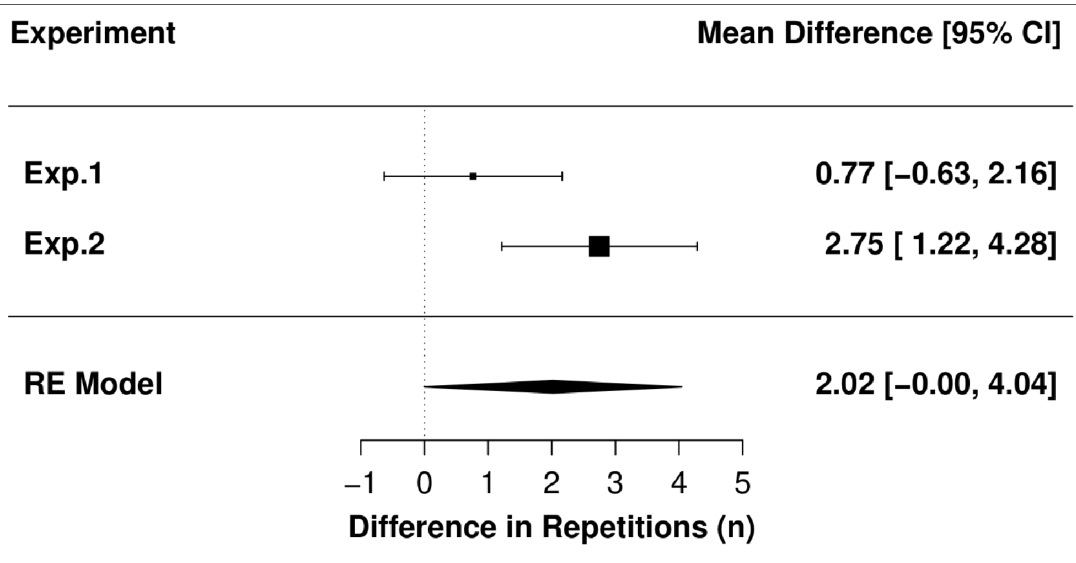

FIGURE 3 | Forest plot of both experiments with 95%CIs; RE, random-effects.

effect within the range of the MDC. The internal meta-analysis echoed the results of Experiment 2 supporting that participants under predicted. These results are mostly in line with previous findings (Hackett et al., 2012, 2016; Giessing et al., 2016a,b; Altoé Lemos et al., 2017; Steele et al., 2017a; Zourdos et al., 2019; Emanuel et al., 2020; Hughes et al., 2020; Mansfield et al., 2020). However, in contrast with prior research this study is the first to examine predictive ability at the sdRM/0RIR. Further, it is the first to use a deception design thus reducing potential demand characteristics from influencing results. This study also offers

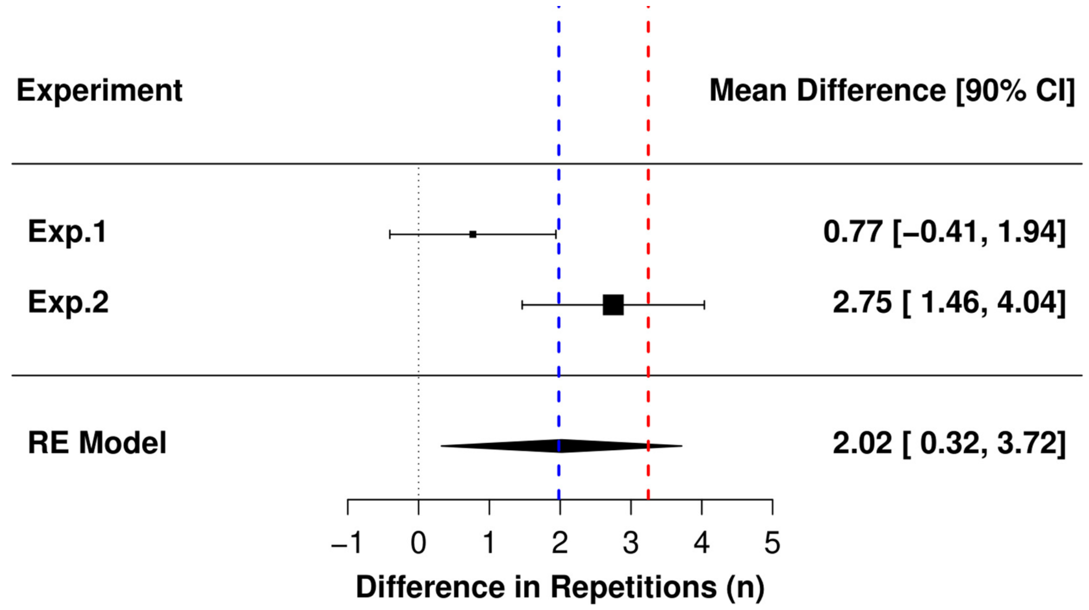

FIGURE 4 | Forest plot of both experiments with 90%CIs in addition to the upper equivalence bands from both Experiment 1 (dashed red line) and Experiment 2 (dashed blue line); RE, random-effects.

a behavioral test of the congruence of perception of effort and actual effort in resistance exercise tasks.

Many authors have examined the accuracy of participants' ability to predict proximity to MF across different exercises using both single and multiple sets, varying relative loads, and predictions both *a priori* and during sets at varying proximities to MF (Hackett et al., 2012, 2016; Altoé Lemos et al., 2017; Steele et al., 2017a; Emanuel et al., 2020; Hughes et al., 2020; Mansfield et al., 2020). The overall results of these studies suggest participants generally under predict the number of repetitions they can perform to MF whether predictions are made *a priori* to initiation of exercise, or at varying degrees of proximity to actual MF. Improved accuracy, which has been shown with subsequent sets (Hackett et al., 2012; Emanuel et al., 2020; Mansfield et al., 2020) or heavier loads (Altoé Lemos et al., 2017; Emanuel et al., 2020; Hughes et al., 2020), would suggest proximity to MF may play a role, though accuracy may still be imperfect. Indeed, Zourdos et al. (2019) found that, despite improved accuracy of predictions with closer proximity to MF, participants still under predicted when they thought they were 5, 3, and 1 repetition away from MF (difference between predicted and actual of 5.15 ± 2.92, 3.65 ± 2.46, and 2.05 ± 1.73 for 5RIR, 3RIR, and 1RIR, respectively). In the current study, participants were instructed to perform a single set to either sdRM (i.e., 0RIR) or MF. Prior studies have not examined this context though it has been speculated that predictive ability would be improved with greater proximity to MF (Mansfield et al., 2020). Furthermore, experienced (>1 year) participants were chosen following prior suggestions that participants predictive ability may improve with training experience (Helms et al., 2016; Steele et al., 2017a). However, our results suggest that even during the gestalt experiences of attempting to get as close as possible, but not reach MF, resistance training experienced participants (>1 year) are still not adequately accurate in their predictions. This is in accordance with other findings in trained participants (Hackett et al., 2012, 2016; Steele et al., 2017a; Zourdos et al., 2019).

Congruence of the perception of effort compared with the actual effort required may play an essential role in individuals' ability to predict proximity to MF. The actual effort required to complete a task can be defined as a function of the absolute demands of the task and the current ability to meet those demands (Steele, 2020). As such, in resistance training for example, the load can affect the actual effort required (higher loads will require greater actual effort to lift them), as can fatigue (reduced capacity) insidious to continued performance (as a set of repetitions progresses each repetition will require greater and greater effort). Both load and fatigue therefore are related to the actual effort required to complete a resistance exercise task. Indeed, the perception of load (i.e., task demands) as well as fatigue (i.e., capacity) and thus perception of effort (Steele, 2020) might determine the accuracy of predictions of proximity to MF. However, though related, the perception of these three (load, fatigue, and effort) can be differentiated (e.g., Buckingham et al., 2014; Micklewright et al., 2017). Despite this, studies suggest trainees may anchor their perceptions of effort upon other salient perceptions; for example, discomfort (see Steele et al., 2017a). This has been argued to be a potential factor influencing predictive accuracy (Steele et al., 2017c). Although the combined perceptions associated with the gestalt experience of performing a resistance exercise bout (i.e., perceived fatigue, effort, and discomfort) typically intensify with closer proximity to MF, the salience of discomfort may overwhelm and influence prediction. In the current study as well as in previous studies (Hackett et al., 2012, 2016; Giessing et al., 2016a,b; Altoé Lemos et al., 2017; Steele et al., 2017a; Zourdos et al., 2019; Emanuel et al., 2020; Hughes et al., 2020; Mansfield et al., 2020), it might have been the case that participants anchored their perception of effort upon their perceptions of discomfort, leading to an overestimation of effort and thus under prediction of how close they were to MF. As outlined by Steele et al. (2017c), without clear instructions, anchoring of effort based on other perceptions such as discomfort seems to happen during resistance exercise.

Poor predictive ability may have implications for managing resistance training through predictions of proximity to failure; this includes both application of sdRM and RIR scales more generally. It may be the case that an initial period of familiarization with the scale (including with training to MF so as to provide an experiential top anchor under supervised conditions) is required to improve predictive accuracy and the RIR scales utility (Helms et al., 2016). Indeed, where it has been recently applied with strength athletes such as powerlifters, an initial familiarization period has been included (Androulakis-Korakakis et al., 2018). Trainees and coaches should be aware that programming resistance training using RIR might result in systematically training with a lower than intended effort if accuracy in predicting proximity to MF is poor. This may have potential to impact upon their adaptations to resistance training (Giessing et al., 2016a,b). However, a limitation of this study should be acknowledged. We did not ask the participants regarding the specifics of their prior training history and thus the extent to which they trained specifically with the knee extension exercise and to MF are unclear. It is indeed possible that, though participants were 'trained,' they may have been relatively inexperienced in the procedures performed in the present experiments (i.e., training to MF). Thus, the generalizability of our findings to 'trained' persons should be treated with the appropriate caution.

CONCLUSION

In conclusion, our results seem to suggest that trained participants with a minimum of 1-year training experience are not adequately accurate at predicting proximity to MF during the gestalt experience of resistance exercise. Further research should look to identify the information that persons utilize to form their predictions during resistance exercise and other physical tasks (i.e., discomfort, fatigue, effort). The inaccuracy of prediction for even trained persons has implications for the control of effort (i.e., proximity to MF) during resistance training. Whether or not predictive ability is sufficient is still yet to be determined as some research suggests effort is an important variable for determining adaptations to resistance training. However, these results suggest this is something to be aware of and will be an issue for controlling submaximal effort. In fact, it is suspected that people on average are inaccurate at gauging effort during submaximal conditions.

DATA AVAILABILITY STATEMENT

The datasets presented in this study can be found in online repositories. The names of the repository/repositories and accession number(s) can be found below: https://osf.io/s9yqk/.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by Health, Exercise, and Sport Science ethics committee at Solent University (ID: standish-hunt2018). The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

JS and HS-H conceived of the study. JS, CA, HS-H, and JF designed the study. JS, CA, HS-H, PA-K, NM, TG, and AH conducted the experiments and collected the data. JS analyzed the data. JS, CA, HS-H, PA-K, NM, TG, AH, JF, PG, and JG all contributed to the interpretation of findings and manuscript preparation. All authors contributed to the article and approved the submitted version.

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Persistence of Mental Fatigue on Motor Control

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The effects of mental fatigue on both cognitive and physical performance are well described in the literature, but the recovery aspects of mental fatigue have been less investigated. The present study aimed to fill this gap by examining the persistence of mental fatigue on behavior and electrophysiological mechanisms. Fifteen participants performed an arm-pointing task consisting of reaching a target as fast as possible, before carrying out a 32-min cognitively demanding task [Time Load Dual Back (TLDB) task], and immediately, 10 and 20 min after completion of the TLDB task. During the experiment, electroencephalography was continuously recorded. The significant increase in mental fatigue feeling after the TLDB task was followed by a decrease during the 20 min of recovery without returning to premeasurement values. Brain oscillations recorded at rest during the recovery period showed an increase in both theta and alpha power over time, suggesting a persistence of mental fatigue. Arm-pointing movement duration increased gradually over time during the recovery period, indicating that behavioral performance remained impaired 20 min after the end of the cognitively demanding task. To conclude, subjective measurements indicated a partial recovery of mental fatigue following a cognitively demanding task, whereas electrophysiological and behavioral markers suggested that the effects of mental fatigue persisted for at least 20 min. While the subjective evaluation of mental fatigue is a very practical way to attest the presence of mental fatigue, electrophysiological and behavioral measures seem more relevant to evaluate the time course of mental fatigue effects.

Keywords: cognitive fatigue, recovery effect, electroencephalography (EEG), brain oscillations, event-related potentials, Fitts' law, arm-pointing task

INTRODUCTION

Mental fatigue, which is defined as a psychobiological state caused by prolonged and/or intense periods of demanding cognitive activity and characterized by subjective feelings of "tiredness" and "lack of energy" (Boksem and Tops, 2008; Rozand and Lepers, 2016), is a common problem in everyday life. It can affect patients with cancer, Alzheimer's or Parkinson's disease (Chaudhuri and Behan, 2000), as well as healthy individuals, in whom it can lead to a decrease in productivity, an increase in road accidents (Dinges, 1995), or even be involved in burnout or depression (Lavidor et al., 2002).

Mental fatigue is a very complex phenomenon, and its neural mechanisms are still poorly known. They likely involve changes in brain activity involving the anterior cingulate cortex (ACC), a brain area at the interface between cognition, emotion, and motor control (Müller and Apps, 2019).

Several different theories have been proposed to explain the mental fatigue-induced performance decrements; these include underload theories (Manly et al., 1999), resource theories (Sanders, 1997), motivational control theories (Kurzban et al., 2013), and dual regulation system (Ishii et al., 2014). The present study falls within the framework of resource theories, and is close to ego-depletion theory (Baumeister et al., 1998; Hagger et al., 2010). In this background, performing a cognitively fatiguing task reduces, or even under certain circumstances, depletes cerebral and cognitive resources that cannot be replenished immediately and that cannot be fully available to perform a following task (cognitive or physical). This could result in performance decrement (although some compensatory mechanisms might maintain performance, Hockey, 2013; Wang et al., 2016) and cerebral brain changes.

Since the middle of the 19th century (Davy, 1845), researchers have investigated mental fatigue in both laboratory and field conditions to better understand its effects and mechanisms. Under laboratory conditions, mental fatigue is generally induced by specific cognitive tasks. For example, the Stroop task (e.g., Rozand et al., 2015; Van Cutsem et al., 2017a), the AX-CPT (AX-continuous performance test; e.g., Marcora et al., 2009; Pageaux et al., 2013) or computerized decision-making tasks (Otani et al., 2017) have been used, generally for a duration ranging from 45–90 min, to induce mental fatigue. However, during prolonged cognitive tasks, both mental fatigue and boredom interfere, making it difficult to study mental fatigue *per se*. To differentiate between mental fatigue and boredom, shorter but more demanding cognitive tasks have been used to induce mental fatigue without boredom. For example, Borragan et al. (2016) used a cognitive task called the Time Load Dual Back (TLDB) task combining a traditional N-back working-memory updating task and an interfering second task (odd/even decision task) lasting 16 min. After completion of the TLDB task, the participants reported a significant increase in the feeling of mental fatigue, and their vigilance decreased during a subsequent psychomotor vigilance task (PVT; Borragan et al., 2017). While subjective measures [e.g., visual analog scale (VAS)] can be used to evaluate mental fatigue (Smith et al., 2019), measures based on electrophysiological recordings can provide more objective evidence of mental fatigue.

Studies using electroencephalography (EEG) have shed light on the neural mechanisms involved in mental fatigue, notably changes in brain oscillations. Mainly an increase in alpha power has consistently been observed, which might reflect a decrease in arousal and alertness (Paus et al., 1997; Boksem et al., 2005; Zhao et al., 2012). Although alpha power changes are a robust marker of mental fatigue, a recent meta-analysis suggested that an increase in theta power would be a more reliable biomarker of the presence of mental fatigue (Tran et al., 2020). According to this meta-analysis, mental fatigue results in large increases in theta power through the whole brain (i.e., frontal, central, and posterior regions), while increases in alpha power are mainly observed in central and posterior regions and to a lesser extent in frontal regions. In addition to brain oscillations, event-related potentials (ERPs) have also been considered. Consistent effects of mental fatigue have been observed on N100, N2, and P300 components. For these three components, a decrease in amplitude over time has been observed and interpreted as reflecting a top–down modulation of sensory processing for the N100 (Boksem et al., 2005; Faber et al., 2012), a decrease in cognitive control for the N2 (Boksem et al., 2006; Möckel et al., 2015) and a decrease in attention for the P300 (Murata et al., 2005; Schmidt et al., 2009). In parallel with electrophysiological changes, performance can also be impaired by mental fatigue.

It has been established that mental fatigue may negatively impact subsequent cognitive (e.g., van der Linden et al., 2003; Boksem et al., 2005) or physical (for review: Van Cutsem et al., 2017b; Pageaux and Lepers, 2018) activities. Concerning physical performance, not all physical activities are negatively impacted by mental fatigue. While previous studies have shown that mental fatigue impaired endurance performance (e.g., Marcora et al., 2009; Pageaux et al., 2014) as well as decision making (e.g., Smith et al., 2016; Harris and Bray, 2019) and motor skills (e.g., Rozand et al., 2015; Le Mansec et al., 2018), the maximal voluntary force/power production capacity seems to be preserved (e.g., Pageaux et al., 2013; Duncan et al., 2015). To evaluate the effects of mental fatigue on motor skills, Rozand et al. (2015) used an arm-pointing task consisting of reaching visual targets as fast as possible. Following mental fatigue, induced by 90 min of a modified Stroop task, these authors observed an ∼10% increase in actual movement duration, indicating an impairment of motor skills following a prolonged cognitively demanding task.

While the effects of mental fatigue on motor performance have been clearly demonstrated, questions about the persistence of these effects and the time course of recovery from mental fatigue have been poorly investigated. To our knowledge, Rivers (1896) was one of the first to address this question by evaluating the effect of a 30- or 60-min rest period following 30 min of mental work (i.e., addition calculation). Results indicated that 30 min of total rest was insufficient to neutralize the effects of mental fatigue and that 60 min of total rest only partially eliminated the effects. More recently, Smith et al. (2019) investigated the effects of three different cognitive tasks (Stroop task, AX-CPT, and PVT), lasting 60 min, on reaction times, feeling of fatigue and electrophysiological markers, as well as their persistence over time. They observed that the feeling of mental fatigue (reported on a VAS) increased immediately after the three cognitive tasks but led to a decrease in vigilance only after the Stroop task and the AX-CPT task. The level of mental fatigue decreased gradually over time, suggesting that a recovery mechanism came into play. However, the feeling of mental fatigue remained high for 10 min after the PVT, 50 min after the Stroop task, and 60 min after the AX-CPT. Concerning performance, no negative effect of mental fatigue was reported on vigilance 30 min after completion of the three different cognitively demanding tasks. These observations suggest that performance may recover faster than the subjective feeling of mental fatigue.

The main objective of the present study was to investigate the time course of mental fatigue following a 32-min cognitively demanding task on both subjective (VAS) and electrophysiological (i.e., brain oscillations) markers, as well as on motor performance. An arm-pointing task was used to evaluate the impact of mental fatigue on motor performance, due to its involvement in many activities of daily living. We firstly hypothesized that during the completion of the TLDB task, an increase over time in both theta and alpha power should be observed. It should concern all the brain regions for theta power, and more specifically central and parietal regions and to a lesser extend the frontal regions for alpha power. Concerning ERPs, we hypothesized that the amplitude of the N100, the N200, and the P300 should decrease over time as observed in previous studies (Boksem et al., 2005, 2006; Murata et al., 2005; Faber et al., 2012). Secondly, we also expected that immediately after the cognitively demanding task the subjective feeling of mental fatigue would increase and the motor performance decrease. These alterations would be associated with an increase in both theta and alpha power during the rest period immediately following the cognitively demanding task. Finally, we assumed that, during the recovery period following the completion of the cognitively demanding task, a progressive decrease in the subjective feeling of mental fatigue would be associated with a return toward initial levels of brain oscillations recorded during rest period and motor performance.

MATERIALS AND METHODS

Participants

The study was conducted with 15 healthy active adults, eight males and seven females (mean ± SD; age: 21.9 ± 1.8 years). It included two sessions: a familiarization session and an experimental session. All participants reported normal or corrected-to-normal vision and none of them had a history of neurological disorders. They completed the Edinburgh questionnaire, which confirmed that all participants were right-handed. All participants were given instructions to sleep for at least 7 h, not to consume alcohol, and to refrain from vigorous physical activity the day before each visit. Participants were also instructed not to consume caffeine and nicotine at least 3 h before testing and were asked to declare if they had taken any medication or had any acute illness, injury, or infection. All the participants provided their written informed consent. The experiment was conducted in accordance with the Declaration of Helsinki (1964).

The Cognitively Fatiguing Task: The Time Load Dual Back Task

The TLDB task is a dual task combining a classic N-back working-memory updating task (Kirchner, 1958) and an interfering second task (odd/even decision task). Stimuli were letters and digits displayed alternately on the screen (i.e., letter/digit/letter/. . .). When letters were presented, participants were instructed to press the space bar with their left hand every time the displayed letter was the same as the previous one (1-back task). When digits were displayed, participants had to press a key on the numeric keypad, using their right index and middle fingers: "1" if it was an odd number, "2" if it was even. A total of eight letters (A, C, T, L, N, E, U, and P) and eight numbers (1, 2, 3, 4, 6, 7, 8, and 9) were used. Stimulus presentation was managed by the Experiment Builder software (SR Research). Stimuli were presented in Arial font size 120, in the center of a 16-inch computer screen (refresh rate 60 Hz). They were presented in blocks of 60 trials with a break of 30 s between each block.

The Physical Task: The Arm-Pointing Task

Participants had to point at a target as accurately and as fast as possible with a pencil held in their right hand. They were sitting down, and in the start position their arm was aligned with their right shoulder and all targets to be pointed at were located on a 45° diagonal to the left to limit joint stress. The targets were black squares displayed on a touch screen in front of the participant (**Figure 1A**). The targets had different indices of difficulty (Nitschke et al., 2006), which were calculated using the formula $ID = \log_2(\frac{2D}{W})$, where D represents the center-to-center distance between the start point and the target, and W represents the width of the target. Using different distances (between 5 and 32 cm) and widths (between 0.5 and 2.5 cm), 40 targets were created with eight IDs ranging from 2.5 to 6 in steps of 0.5. Each ID included five different targets with different widths and distances from the start point.

The time measured for the pointing movement began when participants took the pen from the start point and stopped when the pen touched the touch screen. If the participant landed on the target, the trial was considered as "correct"; if not, it was a miss. At each trial, the participant had to return to the start point. The pointing task lasted approximately 1.5 min.

The arm-pointing task was performed before the TLDB task, immediately after the TLDB task (Post), 10 min (Post 10) and 20 min (Post 20) after the TLDB task. For each arm-pointing task, the difficulty did not increase over time. The same trials were presented in pretest and in all the posttests, but with a different random order.

Psychological Measurement

The Saint Mary's Hospital Sleep Questionnaire was used to evaluate the participants' sleep quality the night before the experiment (at each session). It contains 14 items concerning sleep quality, such as depth, awakening in the middle of the night, satisfaction, refreshed feeling upon awakening, difficulty in falling asleep, and early awakening. Participants reported that they slept on average 7 h and 41 min (±12 min).

Motivation related to the entire protocol was measured at the beginning of the experiment, using the motivation questionnaire developed and validated by Matthews et al. (2001). It has two subscales, evaluating success motivation and intrinsic motivation. Each subscale has seven questions, with a choice of five answers: 0 = not at all, 1 = a little bit, 2 = somewhat, 3 = very much, 4 = extremely. Therefore, total scores for these motivation scales range between 0 and 28. A low score reveals low motivation, and a high score indicates high motivation. Participants' scores were 17.7 (±1.3) for success motivation and 23.3 (±0.9) for intrinsic motivation.

The participants also rated their level of mental fatigue on a VAS at four points of the experimental session: at the start of the session, after the TLDB task, and before Post 10 and Post 20

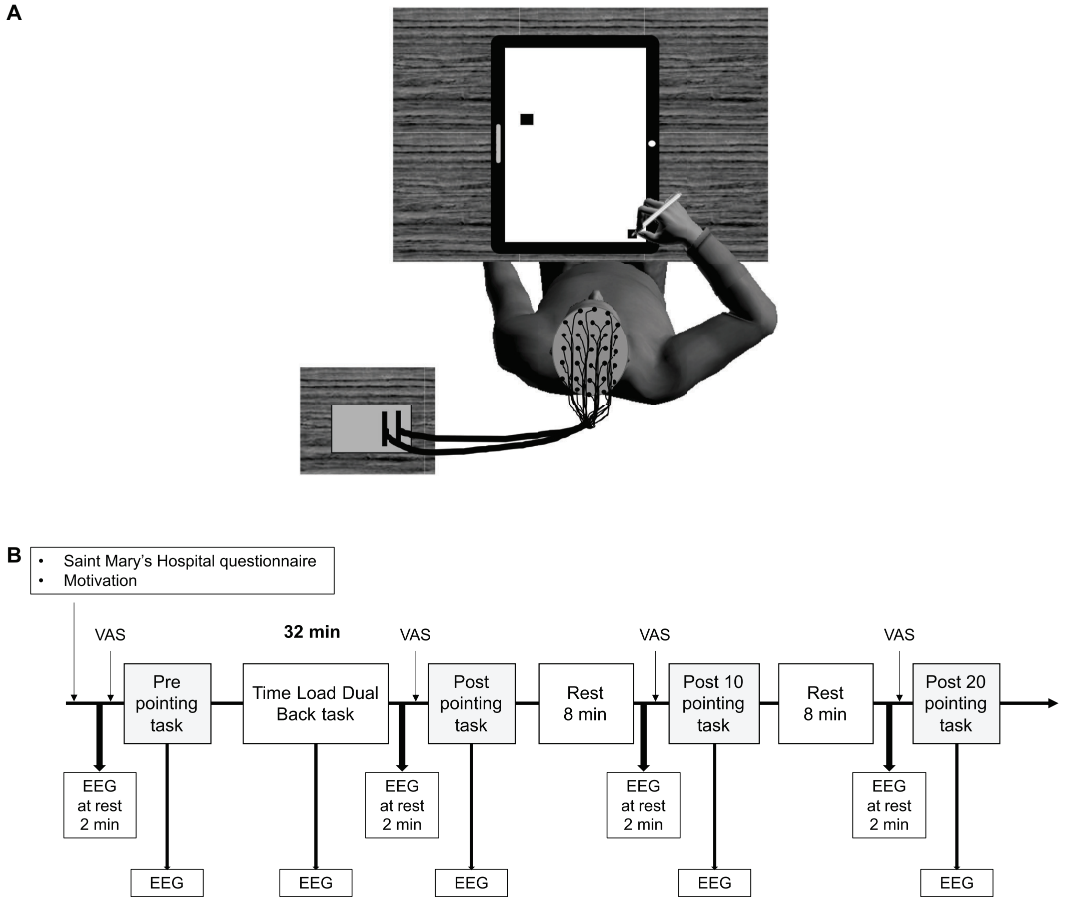

FIGURE 1 | A schematic representation of the participants' position while performing the arm-pointing task **(A)**, and an overview of the experimental protocol **(B)**. VAS, fatigue visual analog scale; EEG, electroencephalography.

tests. The VAS was a line 100 mm long, with bipolar end anchors (0 mm = "Not mentally fatigued at all"; 100 mm = "Extremely mentally fatigued") on which participants placed a mark to estimate their level of mental fatigue.

Procedure

The experiment included two sessions: a familiarization session and an experimental session (**Figure 1B**). During the familiarization session, participants were shown the questionnaires used in the experiment, and were trained on both arm-pointing and TLDB tasks. The threshold of stimulation duration, corresponding to the highest level of TLDB task performance, was determined for each participant using the same procedure as previous studies (Borragan et al., 2016, 2017). All participants started the familiarization session with a 1500 ms presentation of each event (i.e., letters and digits) without inter-stimulus intervals. At the end of each block, if the performance accuracy was equal to or greater than 85%, the duration of the presentation was reduced by 100 ms, and so on until the accuracy was lower than 85%. At this point, participants performed two more blocks with the same duration to pursue familiarization; the threshold for the experimental session was fixed at the last successful duration. The familiarization session also aimed to reduce learning effects, which could counteract the effect of mental fatigue on performance.

The experimental session took place 48–96 h after the familiarization session, at the same time of day. Before installing the EEG recording system, participants were reminded how to perform the arm-pointing task and carried out another practice trial. While the EEG was being set up, participants completed the Saint Mary's Hospital questionnaire and the motivation questionnaire. They were then installed on a comfortable chair in an acoustically and electrically isolated booth (**Figure 1A**). First, EEG activity was recorded at rest for 2 min while the participants were sat in front of a black screen, and were asked to rest and not to think about anything. After that, they were asked to rate their level of mental fatigue on the VAS and to perform the first arm-pointing task. They then performed the TLDB task, which lasted 32 min[1] (excluding the

[1] In a pilot experiment, 16 min of the TLDB task were used to induce mental fatigue as in Borragan et al. (2016). However, while 16 min of the TLDB task induced a subjective feeling of mental fatigue, it was not long enough to induce a decrement in arm-pointing task performance. The duration of the TLDB task was doubled

rest periods between each block). Immediately after the TLDB task, the EEG activity was recorded at rest for 2 min, and participants rated their level of mental fatigue and performed the arm-pointing task again (Post). After 8 min of rest, the same procedure was repeated (i.e., recording EEG activity at rest for 2 min, rating mental fatigue level and arm-pointing task; Post 10). Finally, after a second rest period of 8 min, the same procedure was repeated for the last time (Post 20; see **Figure 1B**). During rest periods, the experimenter talked with the participant. Talking to the participant during the rest period was chosen as an ecological condition, mirroring what happens in daily life at work, for instance. The conversation was similar for each participant, and dealt with their job (or education), their hobbies (e.g., sport, nature), and their perspectives for the future (e.g., studies, jobs). These topics were always addressed in the same order.

EEG Recording and Preprocessing

The electroencephalogram was recorded continuously through the Active Two BioSemi system from 64 electrodes in accordance with the 10–20 International system. Horizontal eye movements were monitored with electrodes placed on the outer left and right canthi, while eye blinks were monitored with an electrode placed under the left eye. Two additional electrodes were placed on the left and right mastoids (A1, A2). During recording, the BioSemi system's common-mode sense electrode served as the reference electrode. Electrophysiological signals were digitized at 2048 Hz sampling rate and acquired with ActiView software. Offline data analyses were performed using MATLAB (MathWorks, Natick, MA, United States) and the EEGLAB toolbox (Delorme and Makeig, 2004). Continuous data were downsampled to 256 Hz, band-pass filtered at 0.01–100 Hz, and re-referenced to the average of A1 and A2. Noisy electrodes were identified with the *probability* and *spectrum* methods proposed in EEGLAB (threshold, Z = 5) and interpolated when necessary with a spherical method. To correct eye-movement artifacts, EEGLAB's Runica routine was used to perform independent component analyses, and components reflecting eye artifacts were removed by visual inspection. Continuous data were segmented into 500 ms epochs from 50 ms before to 450 ms after the onset of the stimuli and were baseline corrected using the pre-trial period from −25 ms to 0 ms.

Data and Analysis

Pointing Movement

Movement durations less than 100 ms and more than 1400 ms were excluded from the analysis. We estimated that movement duration below 100 ms and above 1400 ms were not coherent for the performed movements. Altogether, only two trials over 2400 were excluded from the data. Movement durations beyond two standard deviations (SDs) of the mean were also excluded. Analyses were performed on 88% of trials.

to 32 min and, under these circumstances, both an increase in subjective mental fatigue and a decrement in arm-pointing task performance were observed.

EEG Data

For the TLDB task, only trials with correct responses (93% of trials) were considered for ERP analysis. Epochs were averaged separately for each condition and each participant. ERPs were obtained by computing the mean amplitude in the time window for each ERP component, and by grand-averaging data across participants. Because no studies have previously analyzed ERPs during the TLDB task and because ERPs are stimulus and task dependent, we determined our time windows for the ERP analyses based on visual inspection, according to the peak of the ERP and its shape, to be in complete adequation with the observed ERPs. Different time windows were used for ERP components for letters on fronto-central region (N100: 90–150 ms, N200: 230–315 ms, P300: 325–445 ms) and on parietal region (N200: 130–200 ms, P300: 325–445 ms). Using the same method, ERP components were also identified for digits on fronto-central region (P50: 25–75 ms) and parietal region (N200: 135–210 ms).

For EEG spectral analysis, during the TLDB task, the 2-min rest and the arm-pointing task, EEG power of individual epochs was computed by Fast Fourier Transform (FFT), using the *spectopo* function of the EEGLAB software. It was divided into five frequency bands: delta, 1–4 Hz; theta, 4–7 Hz; alpha, 8–12 Hz; beta 13–30 Hz; and gamma, 30–40 Hz to be analyzed. Spectral analyses during the TLDB task and the arm-pointing task were performed irrespective of the occurrence of stimuli. Nine regions of interest (ROIs) were constituted to perform analysis on both ERPs and spectral data: Frontal Left (FL; mean of FP1, AF3, AF7, F3, F5, F7, FC3, FC5, FT7), Frontal Median (FM; mean of FPz, AFz, F1, Fz, F2, FC1, FCz, FC2), Frontal Right (FR: mean of FP2, AF4, AF8, F4, F6, F8, FC4, FC6, FT8), Central Left (CL: mean of C3, C5, T7, CP3, CP5, TP7), Central Median (CM: mean of C1, Cz, C2, CP1, CPz, CP2), Central Right (CR: mean of C4, C6, T8, CP4, CP6, TP8), Posterior Left (PL: mean of P9, P7, P5, P3, PO7, PO3, O1), Posterior Median (PM: mean of P1, Pz, P2, POz, Oz), and Posterior Right (PR: mean of P4, P6, P8, PO4, PO8, O2). Statistical evaluation was performed with repeated-measures ANOVAs on each ROI.

Statistics

All data are presented as means ± standard error of the mean. Degrees of freedom were corrected using the Greenhouse–Geisser procedure when sphericity was violated (corrected degree of freedom and *p*-values are reported). Only significant effects are reported, except when non-significance is relevant for the hypotheses being tested.

To evaluate mental fatigue effects on the VAS, one-way repeated measures ANOVA with time (Pre, Post, Post 10, and Post 20) as within-subject factor was conducted. The effects of mental fatigue on movement duration and errors on arm-pointing movements were evaluated using two-way repeated measures 4 × 8 ANOVAs with time (pre, post, post 10, and post 20) and ID (2.5, 3, 3.5, 4, 4.5, 5, 5.5, and 6) as within-subject factors.

For the TLDB task, analyses were performed separately for letters and digits. Two-way repeated measures 2 × 4 ANOVAs including the within-subject factors of Stimulus type (for letters: same/different; for digits: even/odd) and time-on-task (part 1, 0–8 min; part 2, 8–16 min; part 3, 16–24 min; and part 4, 24–32 min) were performed for RTs, accuracy, and ERPs. Only ERPs interesting for mental fatigue effects are reported in the "Results" section.

Brain oscillations were analyzed using one-way repeated measures ANOVAs with time-on-task (part 1, 0–8 min; part 2, 8–16 min; part 3, 16–24 min; and part 4, 24–32 min) as within-subject factor during the TLDB task and, with time (Pre, Post, Post 10, and Post 20) for the recordings at rest. Only results about theta and alpha power are reported in the "Results" section, the other brain oscillations are reported in **Supplementary Data**.

All analyses were performed using the Statistical Package for the Social Sciences, version 24 for Windows (SPSS Inc., Chicago, IL, United States). Significant main effects of time and significant interactions were followed up by contrast tests, and planned comparisons using *t*-tests with Bonferroni correction for multiple comparisons as appropriate. Only significant results are reported, with adjusted *p*-values. For each ANOVA, partial eta squared is reported. Thresholds for small, moderate, and large effects were set at 0.01, 0.07, and 0.14, respectively (Cohen, 1988). Cohen's *d* was calculated for each paired *t*-test using JASP (Version 0.9.1.0) [Windows software]. Thresholds for small, moderate, and large effects were set at 0.2, 0.5, and 0.8, respectively (Cohen, 1988).

RESULTS

Mental Fatigue Assessment

Subjective Measure of Mental Fatigue

Visual analog scale

As displayed in **Figure 2**, analysis revealed an increase in the feeling of fatigue over time [$F_{(3,42)} = 26.784$, $p < 0.001$, $\eta_p^2 = 0.657$], qualified by a cubic trend [$F_{(1,14)} = 40.109$, $p < 0.001$, $\eta_p^2 = 0.741$], with a significant increase in feeling-of-fatigue score between Pre and Post [$t_{(14)} = 6.629$, $p < 0.001$, $d = -1.711$], followed by a reduction of the fatigue score between Post and Post 10 [$t_{(14)} = 4.601$, $p = 0.002$, $d = 1.188$], but not between Post 10 and Post 20 [$t_{(14)} = 3.876$, $p > 0.05$, $d = 0.163$]. The subjective feeling of fatigue was still significantly higher at Post 10 [$t_{(14)} = 4.713$, $p = 0.002$, $d = 1.217$] and post 20 [$t_{(14)} = 5.863$, $p < 0.001$, $d = 1.514$] compared to Pre.

Performance During the TLDB Task

Reaction time

Mean reaction time was 535.2 ms (±10.0) for digits and 498.8 ms (±11.0) for letters. No significant effect of time or interaction with the factor time was observed on reaction time during the TLDB task for letters or digits (all *ps* > 0.15, $\eta_p^2 < 0.13$).

Accuracy

Mean accuracy was 90.9% (±0.5) and 92.8% (±0.8) for digits and letters. No significant effect of time or interaction with the factor time was observed on reaction time during the TLDB task for letters or digits (*ps* > 0.35, $\eta_p^2 < 0.07$), suggesting that performance was maintained over time.

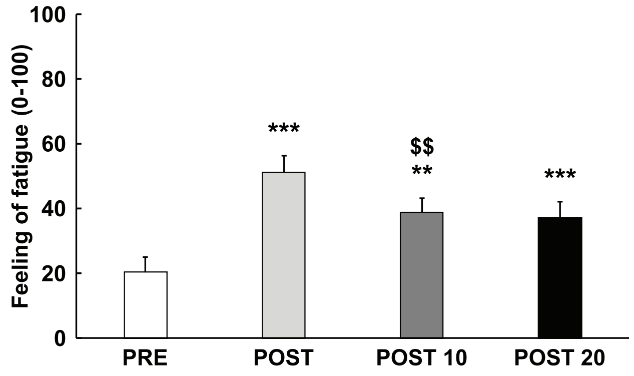

FIGURE 2 | Time course of subjective feeling of mental fatigue. ** and ***: Significantly different from Pre ($p < 0.01$ and $p < 0.001$, respectively). $$: Significantly different from the previous measurement ($p < 0.01$). Data are presented as mean ± SE.

EEG Data

Event-related potentials

N100. Analysis showed a significant time-on-task effect only for letters in CL region [$F_{(3,42)} = 4.019$, $p = 0.033$, $\eta_p^2 = 0.223$], qualified by a linear trend [$F_{(1,14)} = 5.293$, $p = 0.030$, $\eta_p^2 = 0.293$], reflecting a decrease in the N100 amplitude over time (**Figure 3**).

N200. For letters, ANOVA revealed a significant time-on-task effect for FL [$F_{(3,42)} = 6.755$, $p < 0.001$, $\eta_p^2 = 0.326$], FM [$F_{(3,42)} = 6.388$, $p = 0.001$, $\eta_p^2 = 0.313$], FR [$F_{(3,42)} = 5.755$, $p = 0.002$, $\eta_p^2 = 0.291$], CL [$F_{(3,42)} = 7.064$, $p < 0.001$, $\eta_p^2 = 0.335$], CM [$F_{(1.906,26.690)} = 6.623$, $p = 0.005$, $\eta_p^2 = 0.321$], and CR regions [$F_{(3,42)} = 6.274$, $p = 0.001$, $\eta_p^2 = 0.309$], indicating a linear decrease in N200 amplitude over time for all these regions (*p*s < 0.012, $\eta_p^2 > 0.378$). For digits, analysis showed a significant main effect of time-on-task for LF [$F_{(3,42)} = 4.278$, $p = 0.021$, $\eta_p^2 = 0.234$] and CL regions [$F_{(3,42)} = 4.090$, $p = 0.020$, $\eta_p^2 = 0.226$], qualified by a linear trend for both FL [$F_{(1,14)} = 7.576$, $p = 0.016$, $\eta_p^2 = 0.351$] and CL regions [$F_{(1,14)} = 7.882$, $p = 0.019$, $\eta_p^2 = 0.360$] indicating, as for letters, a decrease in the N200 amplitude over time (see **Figures 3**, **4**).

P300. A significant time-on-task effect was reported only for letters on the P300 amplitude for CR [$F_{(3,42)} = 8.465$, $p = 0.002$, $\eta_p^2 = 0.377$], qualified by a linear trend [$F_{(1,14)} = 11.415$, $p = 0.005$, $\eta_p^2 = 0.449$], indicating an increase in the P300 amplitude over time. On PR region, analysis indicated a significant time-on-task effect on the P300 amplitude [$F_{(3,42)} = 5.854$, $p = 0.004$, $\eta_p^2 = 0.295$], qualified by a quadratic trend [$F_{(1,14)} = 9.208$, $p = 0.009$, $\eta_p^2 = 0.397$], reflecting an increase in P300 amplitude over time between the first and the second part [$t(_{14}) = -2.460$, $p = 0.028$, $d = -0.635$], and between the second and the third part of the task [$t_{(14)} = -5.863$, $p = 0.022$, $d = -0.667$], but a decrease in the P300 amplitude between the third and the fourth part [$t_{(14)} = 2.164$, $p = 0.048$, $d = -0.559$] (**Figure 3**).

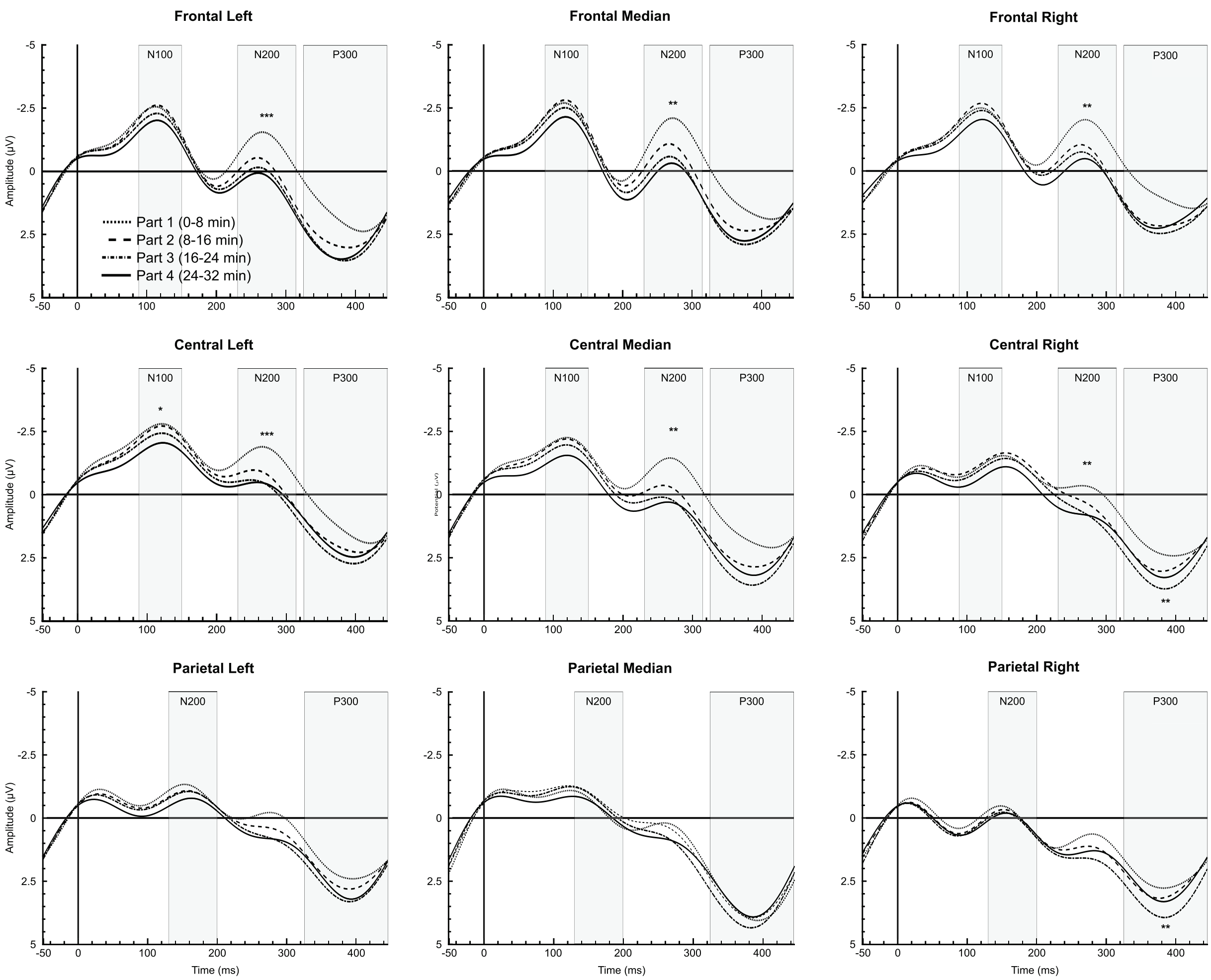

FIGURE 3 | Time-on-task effect for letter stimuli during the 32-min time load dual back task in all regions of interest. *, ** and ***: Significantly time-on-task effect ($p < 0.05$, $p < 0.01$ and $p < 0.001$, respectively).

Brain oscillations

All the results of brain oscillations are presented in the **Supplementary Table S1**. Only significant differences related to brain oscillations useful to test our hypothesis, i.e., theta and alpha powers, are presented below.

Theta. Analysis showed a significant time-on-task effect on FL [$F_{(3,42)} = 6.779$, $p < 0.001$, $\eta_p^2 = 0.326$], FM [$F_{(3,42)} = 12.828$, $p < 0.001$, $\eta_p^2 = 0.478$], FR [$F_{(3,42)} = 11.840$, $p < 0.001$, $\eta_p^2 = 0.458$], CL [$F_{(3,42)} = 13.411$, $p = 0.028$, $\eta_p^2 = 0.489$], CM [$F_{(3,42)} = 13.570$, $p < 0.001$, $\eta_p^2 = 0.492$], CR [$F_{(3,42)} = 9.616$, $p < 0.001$, $\eta_p^2 = 0.407$], PM [$F_{(3,42)} = 7.754$, $p < 0.001$, $\eta_p^2 = 0.356$], and PR regions [$F_{(1.888,26.438)} = 5.908$, $p = 0.008$, $\eta_p^2 = 0.297$], qualified for all these regions by a linear trend (all $ps < 0.011$, $\eta_p^2 > 0.230$), indicating a decrease in theta power over time (see **Figure 5**).

Alpha. ANOVA revealed a significant time-on-task effect on FL [$F_{(1.699,23.781)} = 6.827$, $p = 0.006$, $\eta_p^2 = 0.328$], FM [$F_{(1.447,20.253)} = 7.351$, $p = 0.007$, $\eta_p^2 = 0.344$], FR [$F_{(1.600,22.397)} = 5.811$, $p = 0.013$, $\eta_p^2 = 0.293$], CL [$F_{(1.447,20.256)} = 4.849$, $p = 0.028$, $\eta_p^2 = 0.257$], CM [$F_{(1.601,22.426)} = 6.528$, $p = 0.008$, $\eta_p^2 = 0.318$], CR [$F_{(1.617,22.634)} = 5.426$, $p = 0.016$, $\eta_p^2 = 0.279$], PL [$F_{(1.579,22.108)} = 5.329$, $p = 0.018$, $\eta_p^2 = 0.276$], and PM regions [$F_{(1.621,22.692)} = 7.779$, $p = 0.004$, $\eta_p^2 = 0.357$]. This main effect of time-on-task was qualified by a quadratic trend for all these regions (all $ps < 0.020$, $\eta_p^2 > 0.330$), highlighting a slight decrease in alpha power between the first and the second part of the TLDB task, followed by an increase in alpha power over time (see **Figure 5**).

Effect of Mental Fatigue on the Arm-Pointing Task

Movement Duration

There was a significant main effect of ID [$F_{(1.245,17.428)} = 136.045$, $p < 0.001$, $\eta_p^2 = 0.907$] qualified by a linear trend [$F_{(1,14)} = 10.607$,

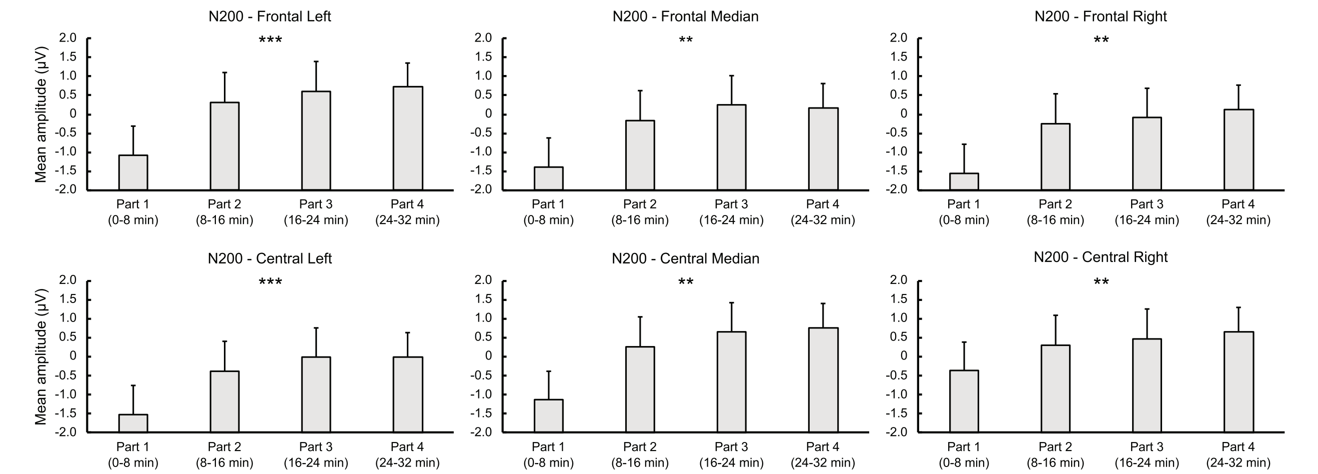

FIGURE 4 | Time-on-task effect on N200 for letter stimuli during the 32-min time load dual back task in the frontal and central regions. ** and ***: Significantly time-on-task effect ($p < 0.01$ and $p < 0.001$, respectively).

$p = 0.006$, $\eta_p^2 = 0.431$], indicating an increase in movement duration with increasing ID, accompanied by a main effect of time [$F_{(3,42)} = 5.259$, $p = 0.005$, $\eta_p^2 = 0.273$]. The decomposition of this time effect revealed a linear increase in the duration of arm-pointing movements over time [$F_{(1,14)} = 10.607$, $p = 0.006$, $\eta_p^2 = 0.431$] as shown by comparison with Pre [*vs.* Post: $t_{(14)} = -0.717$, $p = 1.000$, $d = -0.185$; *vs.* Post 10: $t_{(14)} = -2.527$, $p = 0.121$, $d = -0.653$], up to significance at Post 20 measurement [$t_{(14)} = -2.977$, $p < 0.050$, $d = -0.769$] (see **Figure 6**).

Pointing Errors

There was a significant main effect of ID on errors during the pointing task [$F_{(2.305,32.275)} = 26.782$, $p < 0.001$, $\eta_p^2 = 0.657$]. The decomposition of this main effect revealed a significant linear effect, with more errors as the ID of the target increased [$F_{(1,14)} = 48.455$, $p < 0.001$, $\eta_p^2 = 0.776$]. However, analyses showed no significant effect of time or ID × time interaction.

Effects of Mental Fatigue on Brain Oscillations at Rest

All the results of brain oscillations at rest are presented in the **Supplementary Table S2**. Significant differences related to alpha and theta powers are presented below.

Theta

Analysis showed a significant time effect on FL region [$F_{(3,42)} = 4.359$, $p = 0.009$, $\eta_p^2 = 0.237$], qualified by a linear trend [$F_{(1,14)} = 7.220$, $p = 0.018$, $\eta_p^2 = 0.340$], indicating an increase in theta power over time (see **Figure 7**).

Alpha

ANOVA revealed a significant time effect on FL [$F_{(3,42)} = 4.641$, $p = 0.005$, $\eta_p^2 = 0.261$], FM [$F_{(3,42)} = 3.174$, $p = 0.034$, $\eta_p^2 = 0.185$], FR [$F_{(3,42)} = 4.177$, $p = 0.011$, $\eta_p^2 = 0.230$], CL [$F_{(3,42)} = 4.017$, $p = 0.013$, $\eta_p^2 = 0.223$], CR [$F_{(3,42)} = 3.107$, $p = 0.036$, $\eta_p^2 = 0.182$], PM [$F_{(3,42)} = 3.107$, $p = 0.037$, $\eta_p^2 = 0.312$], and PR regions [$F_{(3,42)} = 6.711$, $p = 0.021$, $\eta_p^2 = 0.324$]. This main effect of time-on-task was qualified by a linear trend for all these regions ($ps < 0.011$, $\eta_p^2 > 0.230$], indicating an increase in alpha power over time (see **Figure 7**).

DISCUSSION

The objective of the present study was to investigate the time course of mental fatigue effects following a cognitively demanding task (i.e., TLDB task) on subjective and electrophysiological markers as well as on arm-pointing task performance. The results showed that the subjective feeling of fatigue increased following the cognitively demanding task and then progressively decreased during the 20 min of recovery, but without returning to initial values. Brain oscillations during the rest periods showed a linear increase in both theta and alpha powers, suggesting that mental fatigue persisted after completion of the cognitively demanding task, and that no recovery mechanism really occurred. Contrary to expectations, motor control performance was worse during the recovery period than immediately after the cognitively demanding task, movements becoming even slower 20 min after completion of the task.

Subjective Feeling of Mental Fatigue

In agreement with previous studies (Borragan et al., 2016, 2017), 32 min on the TLDB task induced an increase in the subjective feeling of mental fatigue, indicating that this cognitively demanding task successfully induced mental fatigue. The feeling of mental fatigue decreased 10 min after completion of the task and remained above the initial value 20 min later, indicating that recovery was not complete. Recently, similar effects were observed by Smith et al. (2019), who found that the subjective feeling of mental fatigue following 45 min performing mentally fatiguing tasks remained higher than pre-treatment values for 10 min after a PVT, 50 min after a Stroop task and 60 min after an AX-CPT task. These observations indicate

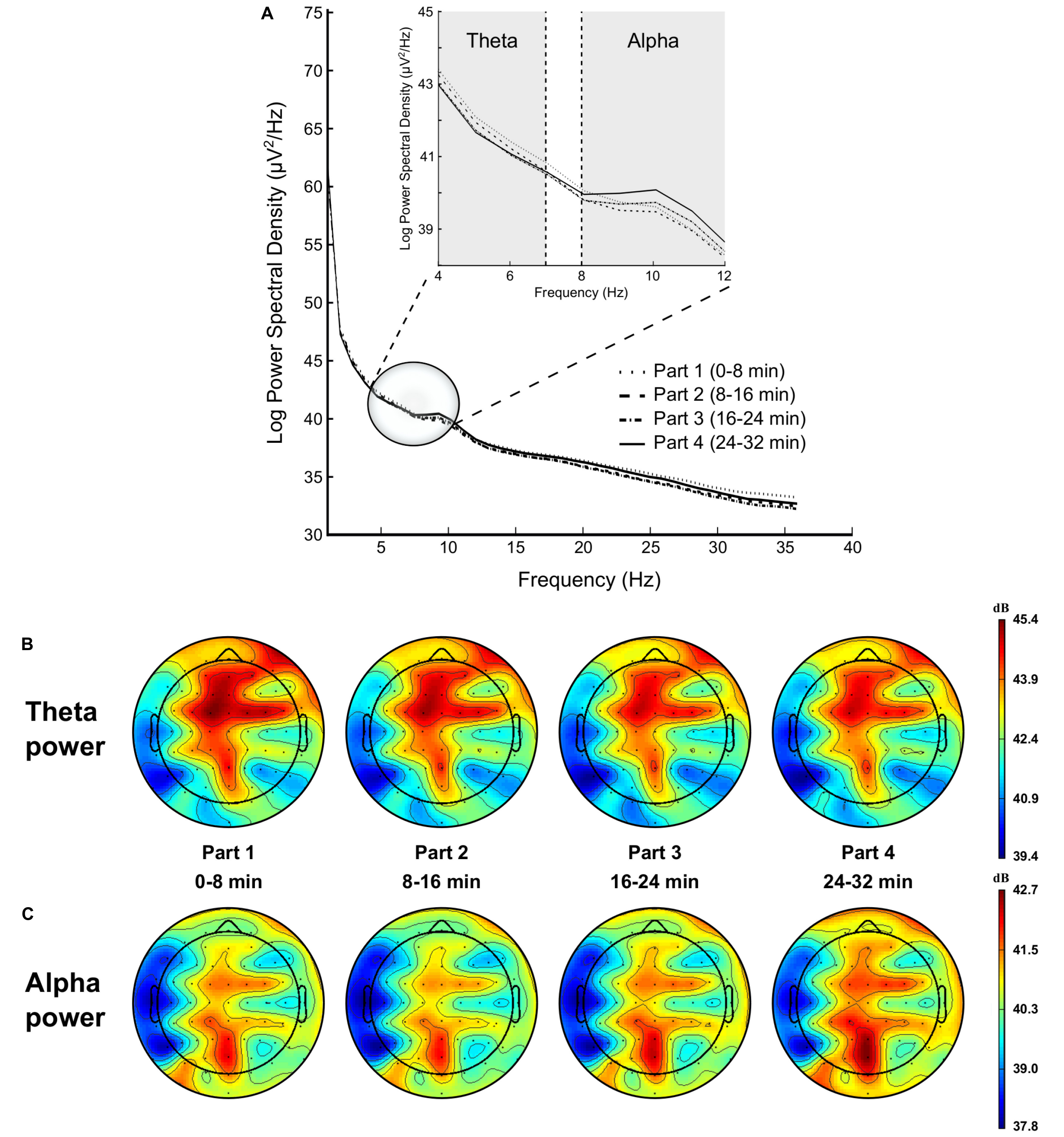

FIGURE 5 | Modulation of brain power spectrum for part 1 (0–8 min), part 2 (8–16 min), part 3 (16–24 min), and part 4 (24–32 min) of the time load dual back task **(A)**, with specifically time course of the theta power **(B)** and alpha power **(C)**.

that the recovery of the subjective level of mental fatigue may require more time than the duration of the cognitively demanding task itself.

Performance During the Cognitively Demanding Task

During the 32-min TLDB task, maintenance of performance (reaction time and accuracy) was observed, in contrast to previous studies by Borragan et al. (2016, 2017), who found a decrease in accuracy over time. It is worth noting that not all studies investigating mental fatigue found impaired performance during the cognitively demanding tasks used to induce mental fatigue; while some studies reported a decrease in performance (e.g., Marcora et al., 2009; Head et al., 2017), others found that it was maintained (e.g., Pageaux et al., 2015). Wang et al. (2016) postulated that the maintenance of performance despite mental fatigue could be explained by a compensatory strategy

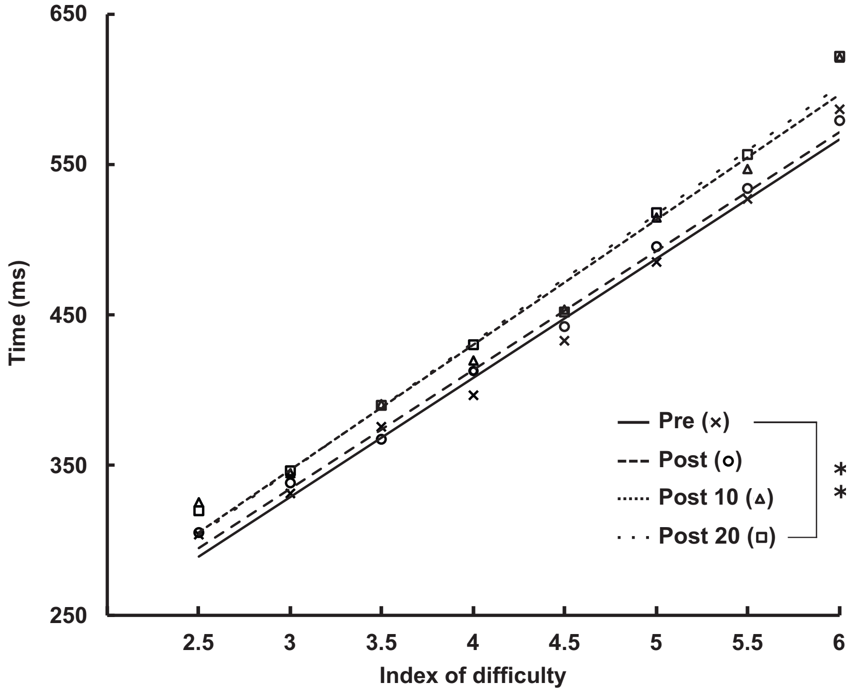

FIGURE 6 | Time course of the duration of arm-pointing movement at Pre, Post, Post 10, and Post 20 with a significant effect of index of difficulty (ID) ($p < 0.001$). **: Significantly different from Pre ($p < 0.01$).

reflected by an increase in anterior frontal brain activity during a prolonged cognitive task. Herlambang et al. (2019) found that performance of a prolonged cognitive task was maintained when participants had a high level of motivation, and decreased when their motivation was low. These observations suggest that motivation is an important factor when considering mental fatigue and its effects. In the present study, the participants showed high motivation to perform the experiment (success motivation = 17.7 ± 1.3 and intrinsic motivation = 23.3 ± 0.9 overall on a 0–28 scale), which could explain the maintenance of performance during the cognitively demanding task.

Electrophysiological Markers During the Cognitively Demanding Task

In addition to subjective and behavioral measures, changes in ERP amplitude during the cognitively demanding task could provide an objective marker of mental fatigue, and explain the maintenance of behavioral performance over time. In the present study, the decrease in N100, considered as an indicator of mental fatigue (Boksem et al., 2005; Faber et al., 2012), might reflect an increase in mental workload. However, the maintenance of performance (i.e., reaction time and accuracy) suggests that a compensatory mechanism was involved during the cognitively demanding task. Previous studies reported a decrease in P300 amplitude (Koelega et al., 1992; Murata et al., 2005), interpreted as a decrease in attention over time during a prolonged cognitive task (Zhao et al., 2012), associated to a decline in behavioral performance with mental fatigue (Cheng et al., 2007; Guo et al., 2015). In the present study, the linear increase in P300 amplitude reported in central regions for letter stimulus could be attributed to an increase in cerebral resources needed to maintain task performance despite progressive mental fatigue.

In addition to the changes observed in the ERP components, brain oscillations also seemed to indicate a mental fatigue state. The increase in alpha power with mental fatigue induction has been widely reported in the literature and has been interpreted as reflecting a decrease in arousal and alertness (Paus et al., 1997; Lal and Craig, 2002; Boksem et al., 2005; Trejo et al., 2005; Zhao et al., 2012). Another interpretation of the increase in alpha power during the TLDB task could be that, in presence of mental fatigue, the participants had to allocate more cognitive resources to maintain their performance. However, an increase in alpha power has been also observed with performance impairment (Boksem et al., 2005; Zhao et al., 2012). The increase in alpha power was also observed when comparing rest periods (i.e., before and after the cognitively demanding task) in the present study but also in previous studies (e.g., Li et al., 2020). These findings suggest that the increase in alpha power observed in mental fatigue studies, and more especially in time-on-task design, is likely not related to the allocation of more cognitive resources to maintain task performance. This increase in alpha power with mental fatigue has often been associated with an increase in theta power mainly over the prefrontal cortex (Lal and Craig, 2002; Trejo et al., 2005). Furthermore a recent meta-analysis suggests that an increase in theta power is a robust biomarker of mental fatigue, whereas an increase in alpha power is a second-line biomarker due

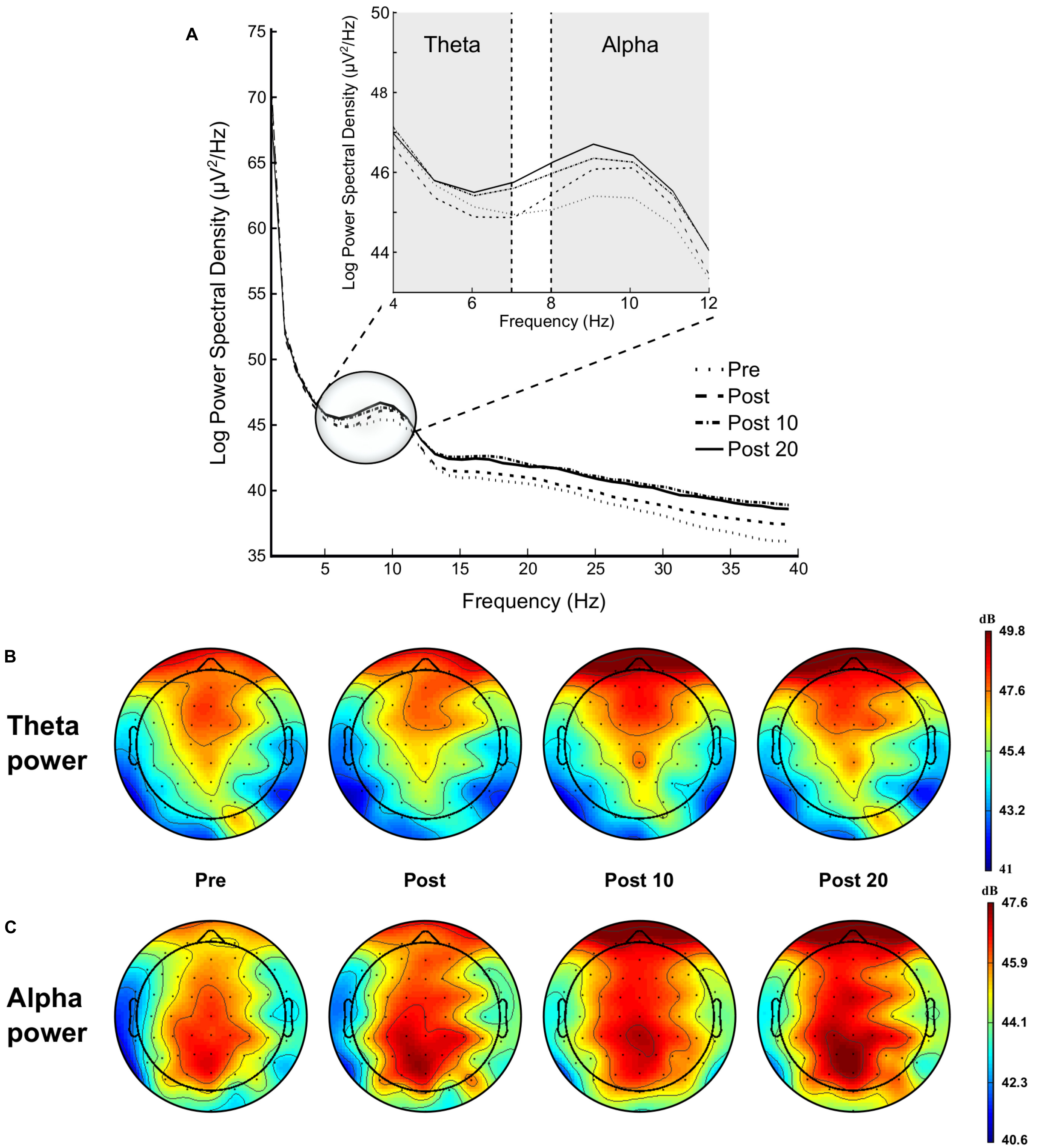

FIGURE 7 | Modulation of brain power spectrum during the 2-min rest periods at Pre, Post, Post 10, and Post 20 **(A)**, with specifically time course of the theta power **(B)** and alpha power **(C)**.

to considerable individual variability (Tran et al., 2020). In the present study, a decrease in theta power was reported over time in frontal regions during the cognitively demanding task. Although theta power decrease has previously been attributed to boredom (Katahira et al., 2018), the relatively short duration of the cognitively demanding task used here (i.e., 32 min) limited the boredom effect.

Brain Oscillations at Rest

In contrast to the brain oscillations analyzed during the cognitively demanding task, those recorded at rest showed an increase in both theta and alpha powers, confirming previous results of the effects of mental fatigue on brain oscillations (Tran et al., 2020). This finding supports the importance of recording brain oscillations not only during the cognitively demanding task but also during the subsequent recovery period, in order to avoid confusion between changes due to the task *per se*, and those actually related to mental fatigue.

While the subjective feeling of mental fatigue decreased during the recovery period, the increase in both theta and alpha powers recorded at rest suggests that subjective (i.e., feeling of mental fatigue) and objective (i.e., brain oscillations) markers could provide contradictory data for interpreting the state of mental fatigue following a cognitively demanding task. Although evaluation of subjective mental fatigue using a VAS is considered to be the most practical way of assessing mental fatigue induction (Smith et al., 2019), it appears that objective electrophysiological markers of mental fatigue, such as changes in brain oscillations, could lead to a different conclusion than subjective evaluation. The present results suggest that when studying mental fatigue, it could be relevant to use different subjective and objective measures to have a more comprehensive view of the effects of mental fatigue.

Absence of Motor Performance Recovery

In accordance with the literature, we observed an increase in the duration of movement as the index of difficulty increased during an arm-pointing task (Rozand et al., 2015; Gueugneau et al., 2017) and maintenance of the speed/accuracy trade-off despite the presence of mental fatigue (Rozand et al., 2015, 2016).

In the present study, movement duration remained constant immediately after the cognitively demanding task, which differs from the results of Rozand et al. (2015). Those authors found that after 90 min of a modified Stroop task, the duration of arm-pointing movement increased by an average of 9%, irrespective of the index of difficulty of the target. The impairment of motor control under mental fatigue was confirmed in a second study by Rozand et al. (2016), who observed a 6% increase in movement duration after a cognitively demanding task inducing mental fatigue. However, the authors indicated that the negative effects of mental fatigue on movement duration seemed to be very short-lived. Indeed, from the second arm-pointing movement, movements were as fast as pretest measurements. In our study, the effects of mental fatigue on motor control were evaluated using the average time of 40 arm-pointing movements. Thus, if a mental fatigue effect occurred only on the first movement it was not enough to be observed on the average of 40 arm-pointing movements. It is worth noting that our experimental design does not allow us to verify this hypothesis, because movement times depend on the ID, and the ID associated with the first movement changed at each pointing task due to the use of different random orders.

When the arm-pointing task was repeated 10 and 20 min after the cognitively demanding task, movement duration was more and more affected, becoming significantly slower 20 min after the task compared to initial performance. A possible interpretation could be that the deterioration of the pointing performance over time was related to muscular fatigue. However, in our pilot experiment the participants repeated five times an arm-pointing task, consisting in 40 movements, after 16 min of TLDB task without a deterioration of the performance. This observation suggested that the decrease in performance in this present study was not related to the repetition of arm-pointing movements or to a possible muscular fatigue, but rather to the effects of mental fatigue.

Neuroanatomically, one possible explanation could be the involvement of the ACC, one of the main brain structures involved in the mental fatigue process (Lorist et al., 2005; Boksem and Tops, 2008; Marcora et al., 2009). Although Tanaka et al. (2014) found reduced activation of the ACC with mental fatigue during a subsequent physical activity, Johnston et al. (2007) observed that switching tasks could lead to an increase in ACC activity. This increase could lead to more resources being engaged to perform the new task. In the present study, the cognitively demanding task was different from the arm-pointing task, and as a consequence the task switching might have increased the activity of the ACC and, in the short term, could have counteracted the effect of mental fatigue. Immediately after the cognitively demanding task, this could result in more resources engaged in arm-pointing movements and could have boosted speed of movements, even in the presence of mental fatigue. During the recovery period, there was no longer task switching due to the repetition of the arm-pointing task. This might lead to a reduced ACC activity, which no longer counteracted the effect of mental fatigue. The persistence of mental fatigue could explain the impaired motor performance 20 min after completion of the cognitively demanding task.

CONCLUSION

The present study found that a 32-min cognitively demanding task induced mental fatigue. As predicted, the subjective feeling of mental fatigue decreased gradually during the 20 min recovery period but remained higher than before the cognitively demanding task. The increase in both theta and alpha power of brain oscillations during the recovery period suggests that participants remained mentally fatigued despite their lower subjective feeling of mental fatigue. The persistence of fatigue during the 20 min period following the cognitively demanding task is in accordance with behavioral results of the arm-pointing task indicating that motor control performance remained impaired. These findings indicate that even if subjective indicators of mental fatigue, such as VASs, are a practical method for assessing mental fatigue, objective indicators such as behavioral and electrophysiological markers are required to have a better characterization of the state of mental fatigue following cognitively demanding tasks.

DATA AVAILABILITY STATEMENT

The raw data supporting the conclusions of this article will be made available by the authors, without undue reservation.

ETHICS STATEMENT

Ethical review and approval was not required for the study on human participants in accordance with the local legislation and institutional requirements. The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

TJ was involved in the design of the study, data collection, analysis, and interpretation, as well as in the draft of the main document. BP-C participated to the elaboration of the experimental design, data collection, analysis, data interpretation, and in drafting the document. PB took part in study programming and data analyses. RL participated to the elaboration of experimental design, the data interpretation, and had also an active role in the manuscript writing. All the authors approved the final version and agree to be accountable for all aspects of the work.

SUPPLEMENTARY MATERIAL

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fpsyg.2020.588253/full#supplementary-material

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An Electromyographic Analysis of the Effects of Cognitive Fatigue on Online and Anticipatory Action Control

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Cognitive fatigue is a problem for the safety of critical systems (e.g., aircraft) as it can lead to accidents, especially during unexpected events. In order to determine the extent to which it disrupts adaptive capabilities, we evaluated its effect on online and anticipatory control. Despite numerous studies conducted to determine its effects, the exact mechanism(s) affected by fatigue remains to be clarified. In this study, we used distribution and electromyographic analysis to assess whether cognitive fatigue increases the capture of the incorrect automatic response or if it impairs its suppression (online control), and whether the conflict adaptation effect is reduced (anticipatory control). To this end, we evaluated the evolution of the performance over time during the Simon task, a classic conflict task that elicits incorrect automatic responses. To accentuate the presence of fatigue during the Simon task, two groups previously performed a dual-task with two different cognitive load levels to create two different levels of fatigue. The results revealed that time on task impaired online control by disrupting the capacity to suppress the incorrect response but leaving unaffected the expression of the automatic response. Furthermore, participants emphasized speed rather than accuracy with time on task, with in addition more fast guesses, suggesting that they opted for a less effortful response strategy. As the implementation of the suppression mechanism requires cognitive effort, the conjunction of these results suggests that the deficits observed may be due to disengagement of effort over time rather than reflecting an incapacity to make an effort.

Keywords: cognitive fatigue, effort, action control, electromyography, gratton effect, inhibition

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INTRODUCTION

Some complex activities, such as piloting an airplane, require a sustained cognitive effort that can lead to cognitive fatigue. This state, which is distinct from drowsiness, can be defined as a difficulty in initiating or sustaining voluntary activities [Adams et al., 1997; for a review see Chaudhuri and Behan (2004)]. There is no consensus on the factors that cause cognitive fatigue. Among the proposed factors, some authors suggest that a decrease in metabolic resources [e.g., glucose; Muraven and Baumeister (2000)] is central while others emphasize the importance of effort and argue that cognitive fatigue should occur when the costs of cognitive effort to perform the activity are higher than the expected benefits (Boksem and Tops, 2008; Kurzban et al., 2013). In this case, after performing an effortful task, disengagement from the current task or unwillingness to sustain the effort on a second task is likely (Inzlicht et al., 2014; Müller and Apps, 2019). However, these two proposals are not mutually exclusive (Christie and Schrater, 2015; André et al., 2019).

Cognitive fatigue can appear in two distinct forms. Changes in performance may be observed (Holtzer et al., 2010). These changes are sometimes referred to as fatigability. Many cognitive processes can be disrupted as cognitive flexibility (Plukaard et al., 2015) or planning (Lorist et al., 2000; van der Linden et al., 2003), which can interfere with the ability to adapt to unexpected situations. Overall, the proper functioning of cognitive control processes appears to be impaired (Lorist and Faber, 2011). Consequently, a decrease in performance can be observed, including an increase in the number of errors (Boksem et al., 2006). Cognitive fatigue can also be subjective, in which case, a feeling of exhaustion or a decrease in motivation can be reported (Gergelyfi et al., 2015). The relation between these two manifestations has often been studied but rarely observed, so they are sometimes considered to be independent (Kluger et al., 2013). Some models suggest that this dissociation is related to the fact that these two manifestations do not appear at the same time. The performance decrement would be later than subjective fatigue because the latter would signal the need to maintain the performance (e.g., Hockey, 2013). But another reason that could contribute to this absence of relation is the lack of sensitivity of the measures used. In this regard, Wang et al. (2014) reported a correlation between trait fatigue perception and the coefficient of variation of RT, but not with other behavioral measures (i.e., RT and accuracy). In addition, only subjective fatigue is generally evaluated but cognitive fatigue is accompanied by other subjective manifestations. Perceived effort is particularly important since cognitive fatigue increases effort costs and when these costs are considered too high, it can lead to disengagement of the task (Hockey, 2013; Inzlicht et al., 2014).

When operators of critical systems (aircraft, nuclear plants, train…) are subject to cognitive fatigue, fatigability can have dramatic consequences. Given the likelihood of the occurrence of cognitive fatigue in the operational context and the role of adaptive capabilities in the safety of these critical systems, it is necessary to understand how cognitive fatigue interferes with the cognitive mechanisms involved in adaptation capabilities and to better understand the relationship between subjective fatigue and fatigability. In this article, we explore this impact through the evaluation of action control during sensorimotor activities.

Action control is defined as the capacity to limit impulsive actions and favor goal-directed ones. Indeed, to adapt to the constraints of a dynamic environment and to limit errors, we must often choose actions adapted to our goals among many others. To this end, two types of control can be distinguished: *online* and *anticipatory* control. These two controls involve different processes and cerebral networks and do not occur at the same time (Ridderinkhof et al., 2011). Online control refers to the processes that inhibit and resist the activation of an automatic and unwanted response for another according to our goals. This control acts after the stimulus presentation and before the incorrect response is emitted. The online control is therefore transitory, changing from trial to trial. Unlike online control, anticipatory control prepares for the correct action. It strengthens the online control or limits its use. Ridderinkhof et al. (2011) consider that anticipatory control can be divided into two parts, *reactive* and *prospective* control [see Braver (2012), for another conception of a dual mechanism of action control]. In the first case, the control is adjusted based on past performance and events (e.g., I strengthen online control after I made a mistake). In the second case, the control is adjusted according to task regularities or instructions, allowing the prioritization of relevant information or anticipating the need for online control.

These two controls, online and anticipatory, have been studied using conflict tasks such as the Simon task (Simon, 1969). In this task, participants must give a lateralized response based on a non-spatial attribute of the stimulus. Although not relevant to the task at hand, the stimulus position automatically activates the hand located ipsilaterally while the relevant attribute activates the hand associated with the instruction. Thus, a conflict may arise when the stimulus is presented on the side opposite to the instruction-based response (incompatible trials). In this case, higher error rates and longer response time (RT) are observed, which is often referred to as the "compatibility effect," indexing the cost of the automatic activation and its subsequent suppression.

The compatibility effect, however, is sensitive to context. In particular, past events can strongly modulate it. The compatibility effect is largely reduced after an incompatible trial compared to a compatible trial (Gratton et al., 1992; Egner, 2007). This reduction in the compatibility effect after an incompatible trial, called conflict adaptation effect or Gratton effect, is thought to reflect an adjustment of the adaptive control [reactive control; Botvinick et al., 2001, see however Mayr et al. (2003), Hommel et al. (2004) for alternative accounts]. Thus, through the magnitude of the reduction in the compatibility effect and its evolution after an incompatible trial (i.e., the Gratton effect), the Simon task allows the evaluation of both online and anticipatory control mechanisms. Both the mean interference effect and its modulation have been used to assess the origin of cognitive fatigue. We will now briefly review this literature before pointing to the limitation of simply assessing mean behavioral compatibility effects.

Several authors have observed a disruption in online and anticipatory control with cognitive fatigue, but the results are far from being consistent. Concerning online control, in a study requiring the completion of a Simon task for more than 3 h, Möckel et al. (2015) observed an increase in the compatibility effect with time on task (Möckel et al., 2015) suggesting that cognitive fatigue interferes with online control. But the opposite has also been observed in longer studies [Wascher et al., 2014; see also Boksem et al. (2006), Xiao et al. (2015) for similar results]. Studies specifically evaluating the effect of cognitive fatigue on the Gratton effect are relatively scarce, but the same uncertainty seems to apply to anticipatory control in other contexts in which fatigability may be observed. Von Gunten et al. (2018) observed that the Gratton effect remained present throughout an Eriksen flanker task [Von Gunten et al., 2018; see Lorist and

Jolij (2012), for similar results]. However, in a sleep-deprived condition in which cognitive fatigue is important, the conflict adaptation effect was impaired, unlike online control (Gevers et al., 2015). Research has mainly focused on another adaptation effect, namely, post-error slowing, but as with online control, inconsistent results have also been observed (Lorist et al., 2005; Boksem et al., 2006; Xiao et al., 2015).

Regarding the different experiments on cognitive fatigue, these inconsistent results may lie in the use of metrics that do not accurately capture the functioning of action control. In this case, cognitive fatigue could be present but its behavioral effects would not be detected by the measures used. Indeed, the use of traditional measures only (e.g., average RT, accuracy) provides only a macroscopic view of the cognitive mechanisms involved in the control of actions.

The size of the compatibility effect (and its modulation) stems from at least two components: the strength of the automatic response activation and the capacity to overcome this initial automatic activation. However, mean compatibility effect measure on behavioral response does not allow to dissociate them. Nevertheless, some tools, however, exist to do so (see below). These tools evidenced that these two mechanisms are largely independent, as they can be specifically affected by different factors [i.e., some factors specifically affect one mechanism, sparing the other one, e.g., see Spieser et al. (2015), Fluchère et al. (2018), Korolczuk et al. (2020) for double dissociations]. Cognitive fatigue could hence either increase automatic activation and/or reduce the capacity to overcome this automatic activation. In this context, measuring these mechanisms separately promises to clarify the impact of cognitive fatigue on the control of the action.

This will be done using electromyographic measures and distribution analysis in a Simon task. The EMG recordings reveal a covert phenomenon evidencing the presence of automatic response activation. On some correct trials, subliminal muscle activation (i.e., that does not exceed the response activation threshold) is observed on the hand muscle associated with the incorrect response before the muscle activation associated with the correct response. Such "partial errors" are more numerous on incompatible trials and reflect (to a large extent) the automatic activation of the incorrect response by the stimulus position (Hasbroucq et al., 1999; Burle et al., 2002). The strength and time course of the automatic response activation can be evaluated by coupling these EMG measures with the conditional incorrect accuracy function, which plots the probability that the first EMG activation is observed in the correct hand, as a function of the latency of this first EMG activation. It is commonly observed that at short latencies, most EMG activations are incorrect on incompatible trials. The percentage of incorrect activations during short trials can be considered as an indicator of the strength of the automatic response activation (Ridderinkhof, 2002; van den Wildenberg et al., 2010). The analysis of the EMG recordings also provides a direct indicator of the suppression mechanism: the ability to overcome incorrect activations can be evaluated by calculating the correction ratio, which is the number of incorrect activations corrected divided by the total number of incorrect activations. A higher correction ratio means a better ability to inhibit incorrect automatic activations (Burle et al., 2002, 2014). Using these two independent measures, we intend to clarify the impact of cognitive fatigue on online control.

In this study, with the association of distribution analysis and EMG, we assessed the extent to which cognitive fatigue impacted online and anticipatory control. Cognitive fatigue has been manipulated in two ways: time on task and using a secondary task, the Time Load Dual Back (TLDB) task (Borragán et al., 2017). The time spent on the task is an important factor leading to cognitive fatigue. To this end, we evaluated the evolution of performance over time during the Simon task. Thus, participants completed a long version (45 min) of the Simon task. Its duration remained shorter than in other studies to limit the involvement of other factors such as boredom or decreased motivation that could explain the performance decline (Möckel et al., 2015). Nevertheless, it remains sufficient since several studies have observed performance decrement with shorter durations (e.g., Lorist et al., 2005). To assess the impact of cognitive fatigue induced by time on task, we will evaluate the measures defined above at the beginning, middle, and end of the experiment. In order to observe whether different levels of cognitive fatigue could be responsible for these differences, we also tried to induce two different levels of fatigue. To this end, our two groups previously performed the TLDB task which quickly induces two levels of cognitive fatigue in two different groups by modulating the cognitive load level of the task (Borragán et al., 2017; O'Keeffe et al., 2020).

Cognitive fatigue primarily affects top-down processes (Lorist and Faber, 2011). If cognitive fatigue disturbs online control, the suppression of the incorrect response (i.e., correction ratio) and/or the strength of the response capture should be impacted over time. For the same reason, the reduction of the compatibility effect after an incompatible trial should be lower over time. These negative effects are expected to be larger for participants who performed the TLDB task with the highest cognitive load, i.e., those for whom we tried to induce even more cognitive fatigue. We also assessed the subjective experience of participants. We made this choice to ensure that cognitive fatigue was induced but also to determine if perceived effort and/or subjective fatigue correlated with EMG measures. Some models indicate that subjective experience precedes the performance decrease (e.g., Hockey, 2013). Thus, we distinguished between perceived effort and subjective fatigue induced by the TLDB task and by the Simon task. Similarly, the accomplishment of a prolonged task can modulate other subjective manifestations like sleepiness and alertness. In addition, we will also control the evolution of these variables. Our hypotheses are therefore that subjective fatigue increases over time and that this increase, along with perceived effort, is greater for participants who performed the TLDB task with the highest cognitive load. Since the total duration of the study is important, we also expect an increase in sleepiness and a decrease in alertness with time on task. However, we should observe a correlation only between EMG measures and subjective fatigue and perceived effort, but not with sleepiness and alertness.

METHOD

Participants

Twenty four participants volunteered for this study and were randomly assigned to one of two groups differing in the amount of cognitive fatigue induced (see below). The "High Cognitive Load" group (HCL) was composed of 12 participants (3 men, M = 22; SD = 2.74) and the "Low Cognitive Load" group (LCL) as well. In this group, however, one participant's data could not be used due to a technical problem (3 men, M = 22.1; SD = 2.55). All participants had normal or corrected-to-normal vision and reported no history of psychiatric or neurological disease. They were paid 10 Euros/h. This experiment was approved by the Comité de Protection des Personnes Sud Méditerranée 1 (approval 1041). Participants gave their informed written consent according to the Declaration of Helsinki.

Materials

Participants were seated in a comfortable chair 70 cm in front of a CRT monitor with a refresh rate of 70 Hz and a screen resolution of 1,024 × 768. They were tested in a dark, sound-shielded Faraday cage. PsychoPy software (Peirce, 2007) was used to display stimuli and to collect behavioral and subjective data. Responses were made by pressing either a left or a right button with the corresponding thumb. The buttons were fixed to the tops of two plastic cylinders (3 cm in diameter, 9 cm in height) separated by 20 cm. Button releases were transmitted to the parallel port of the recording PC to reach high temporal precision.

Tasks Performed by the Participants

Participants performed different tasks, which will first be described separately. The time course of the different tasks will then be presented.

The Time Load Dual Back (TLDB) Task

This task is a dual-task combining a parity judgment task and an N-back task [see Borragán et al. (2017) for more details on the task]. Letters (A, C, T, L, N, E, U, and P) and digits (1 to 8) were displayed (Arial, size = 2°) in alternation. Participants were asked to indicate whether the digit was odd or even by pressing either the right or the left button and whether the displayed letter was the same as the penultimate letter (2-back task) by pressing either the right or the left button again. The response mapping was counterbalanced across participants. The task was divided into several blocks of 30 letters and 30 digits pseudo-randomly presented. In each block, there were 10 target letters. The number of blocks depended on the stimulus duration (STD) which was set individually for each participant to adjust the cognitive load. The computation of the individual STD was performed during a pre-test session on a different day from the test session. During this pre-test session composed of four tasks, participants were first trained on each task separately, then on the combination of the two (i.e., the core TLDB task) and finally the individual STD was computed during another TLDB task. The STD was initially set to 2,000 ms for the three training tasks. Training tasks stopped if the accuracy of the participants was more than 85%[1]. over a block. To compute the STD for each participant in the fourth task, the STD was set to 1,900 ms in the first block and if the accuracy score was ≧85%, the STD decreased by 100 ms for the next block. To reduce the duration of the pre-test session, the STD decreased by 200 ms if the accuracy score was ≧95%. This task was again interrupted when the accuracy dropped below 85%. The STD of the last successful block was assigned to the HCL condition. The STD in the LCL condition was made 50% longer than in the HCL condition. Regardless of the STD, the duration of the task lasted ∼24 min[2]. There was a slight variation to allow for the completion of the ongoing block. In all tasks, participants were instructed to respond quickly and accurately.

The Simon Task

Participants completed a training session of 48 trials and a test session of 15 blocks of 96 trials each. The blocks were separated by a break of up to 1 min. Each trial started with the apparition of a white fixation cross for 500 ms. Then a circle (diameter = 1.4°) red (RGB: 0.835, −1, −1) or blue (−1, −1, 0.835) was displayed at 3° to the left or right of the fixation cross and disappeared after 1,000 ms if no response was given. Half of the participants were asked to answer with their right hand when the circle was blue and with their left hand when the circle was red. The response mapping was reversed for the other half of the participants. An inter-trial interval of 500 ms ended the trial. Half of the trials were compatible which means that the stimulus was displayed on the same side as the required response, and the other half were incompatible (stimulus displayed on the opposite side to the required response). The trials were pseudo-randomized using Mix software (van Casteren and Davis, 2006) so that the compatibility sequences (i.e., compatible–incompatible CI, CC, IC, and II) occurred the same number of times. Participants were asked to respond as quickly and accurately as possible according to the color of the circle and regardless of its position.

Psychomotor Vigilance Task (PVT)

The purpose of this task was to assess each participant's vigilance level before the test. We focused on two measures that appear to be sensitive enough to detect low vigilance level (Basner and Dinges, 2011). We counted the number of omissions (i.e., RT > 500 ms) and the inverse of the RT. The duration of the task was 5 min. Each trial started with a timer being triggered after a random delay of between 2 and 10 s. The participant's task was to click on a mouse button as quickly as possible to stop the timer.

Subjective Scales

Usually, when a task is performed over a long time, different subjective manifestations can appear. Therefore, several scales

[1]On the TLDB task, a composite score was computed using a weighted formula where the 2-back task and the judgment parity task represented, respectively, 65 and 35% of the total score. Borragán et al. (2017) made this choice to emphasize the information-retrieval component of the task.

[2]The task stopped six times (every 4 min) allowing participants to answer two questions. They had to evaluate the accuracy they obtained during the last block and to indicate their level of certainty about this evaluation. They reported their response on a visual analog scale. However, these results are not included in the present report.

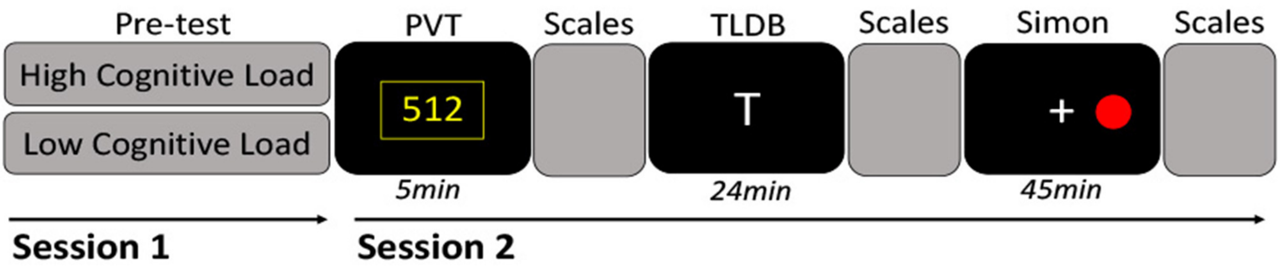

FIGURE 1 | Procedure. On the first session, the participants completed a pre-test to adapt the cognitive load level of the TLDB task according to their capacity. During the second session, which was scheduled on a different day, participants first performed the PVT to assess their vigilance level. Before (time 1) and after the TLDB task (time 2) and the Simon task (time 3), several scales were filled: the Visual Analogic Scale of cognitive fatigue (VASf) and sleepiness (VASs), the Samn-Perelli, and the NASA RTLX (only after each of these two tasks). The TLDB task was performed either in a high (HCL) or low (LCL) cognitive load. The Simon task was the same for both conditions. This second session lasted ∼80 min.

measuring different constructs were used. Subjective fatigue and sleepiness were measured by two visual analog scales [VASf and VASs, respectively; Lee et al. (1991)]. We also used the Samn-Perelli scale, which instead measures the level of alertness (Samn and Perelli, 1982). We also measured the cognitive load level that participants assigned to different tasks with the NASA RTLX (Hart, 2006). This scale is composed of six subscales assessing mental demand, physical demand, temporal demand, performance, effort, and frustration. In this study, apart from the evaluation of the average of the subscales, we focused on the subscale "effort" to evaluate the correlation between objective measures and perceived effort. Other measures such as mental demand could have been included but it mainly reflects the difficulty of the task.

Procedure

The study was divided into two sessions. The first session was the pre-test session. Participants were trained to perform the TLDB task and the STD was evaluated for each participant. In the second session, they performed the tasks in the following order: the PVT, the TLDB task (either in the high or low cognitive load condition), and the Simon task. The scales (i.e., VASf, VASs, and the Samn-Perelli) were completed before and after the TLDB task and the Simon task (i.e., three times in the experiment) while the NASA RTLX was filled out only after these two tasks (**Figure 1**). The average delay between the two sessions was 2.6 days (*SD* = 1.9). As far as possible, the two sessions were completed at the same time of day on different days. The sessions took place between 8:00 a.m. and 12:00 a.m. and between 2:00 p.m. and 6:00 p.m. Each participant was asked to have enough sleep the night before the experiment. They were not aware of the duration of the tasks and could not make an objective evaluation during the test.

EMG Recordings and Processing

The EMG activity of the flexor pollicis brevis from both hands was recorded with two surface Ag-AgCl electrodes (Biosemi, Amsterdam, The Netherlands) fixed ∼2 cm apart on the thenar eminences. The sampling rate was 2,048 Hz and the signal was high-pass filtered off-line at 10 Hz. The EMG signal was continuously monitored by the experimenter to avoid, as far as possible, any background activity that might interfere with the signal recording and mask small muscle activations. In the case where tonic muscular activity was observed or during the breaks between blocks, the experimenter asked the participant to relax their muscles. The EMG onsets were hand-scored after visual inspection. This method took longer than the automated algorithm, but the recognition of small muscle activations is better (Staude et al., 2001).

Data Analysis

Anticipations (trials with RT <100 ms) were excluded from the analysis for both tasks. Trials of the Simon task were classified into three categories. The correct trials were separated according to whether an EMG burst was recorded (partial-error trials) or not (pure-correct trials) on the incorrect side preceding the correct response. Trials were defined as errors when only the incorrect response was recorded. Trials that did not correspond to these three categories were rejected from the analysis. A total of 12.8% of the trials were excluded. From the distinction between these three categories, we were able to extract several variables (**Figure 2**). First, the RT was fractionated into different intervals: for all trials, we defined the pre-motor time (from stimulus presentation to correct EMG onset) and motor time (from EMG onset to mechanical response recording). For trials containing a partial error, a third chronometric variable was extracted: the partial error latency, which corresponds to the time from stimulus presentation to the onset of the incorrect EMG burst. Second, errors and partial errors were also extracted to compute the conditional incorrect accuracy function and the correction ratio. The conditional incorrect accuracy function was constructed by taking the first EMG activation, whether correct or incorrect, and spitted the distribution into five bins with the same number of trials. For each bin, we computed the proportion of correct EMG and the mean value of the latencies of this bin. The proportion is then plotted as a function of the mean bin latency to construct the conditional incorrect accuracy function. To evaluate anticipatory control, we analyzed the Gratton effect. Trials were classified according to the compatibility of the preceding trial. For this analysis, the first trial in each block was

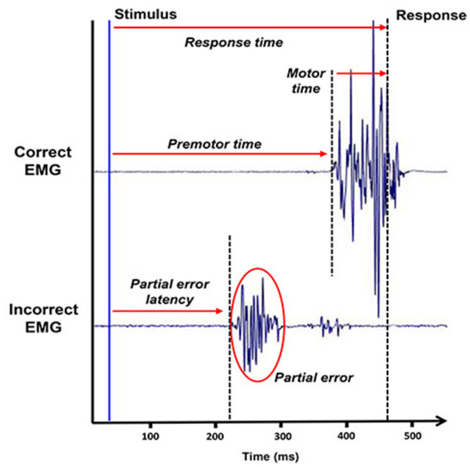

FIGURE 2 | The chronometric measures recorded during this task. The electromyographic recording of the two agonists associated with the two possible responses as a function of time (in ms) allows to observe partial errors and to distinguish different chronometric measures. It enables the separation of the classical response time into two different measures that we used in this study, the pre-motor time and the motor time.

excluded and all *n* trials were correct trials. The *N*-1 trials were correct trials when we analyzed RT.

Statistical Analysis

We proceeded in several steps for the statistical analysis. First, we analyzed the control variables to confirm that the two groups were equal in various aspects at the beginning of the test session, such as their level of alertness and their performance during the pre-test session. These different measures were subjected to an analysis of variance (ANOVA) with cognitive load level (HCL or LCL) as a between-subject factor.

We wanted to quantify the evolution over time of the different subjective experiences (e.g., subjective fatigue, sleepiness) felt by the participants during the experiment. Thus, we analyzed separately the evolution of scores after the TLDB task and after the Simon task. But first, we looked at the subjective ratings of the participants at the beginning of the test. It is important to assess whether participants were already tired or sleepy, as this could have a significant impact on performance and ratings. The data were submitted to multiple ANOVAs. The score from each of the different scales (i.e., VASf, VASs, Samn-Perelli, and NASA RTLX) was used as a dependant variable. The between-subject factor "cognitive load level" (HCL or LCL) was again included and, when necessary, the within-subject factor "time," referring to the different times the questionnaire was completed.

As explained above, one of the objectives of the TLDB task was to increase the presence of cognitive fatigue during the Simon task, which was already expected to be caused by the time spent on the task. In addition to assessing the evolution of subjective fatigue after performing the TLDB task, we evaluated the evolution of performance during this task. Observing a different decline in performance over time between the two groups would ensure that the task fulfilled its role. To this end, the TLDB task was divided into six blocks and the two subtasks were evaluated separately during the analyses. We extracted two behavioral markers (RT and accuracy) to explore whether changes in performance were observed across blocks (within-subject factor, block 1 to 6) and/or as a function of cognitive load level (between-subject factor, HCL or LCL).

Finally, to characterize online and anticipatory control, we combined the classical measures of the compatibility effect (i.e., RT, accuracy, and the Gratton effect) with complementary measures (pre-motor time, motor time, incorrect activation rate, conditional incorrect accuracy function, correction ratio) only accessible with EMG recordings. By using such measures, we wanted to clarify how cognitive fatigue affected automatic response activation, suppression mechanisms, and anticipatory control when performing the Simon Task. To assess the evolution of these measures as a function of time on task, blocks were included as a within-subject factor. We grouped the 15 blocks into 3 large blocks and compared only blocks 1 and 3 (i.e., the first 5 and last 5 blocks). The trial sequences were considered as a within-subject factor in the evaluation of the Gratton effect. For the conditional incorrect accuracy function analysis, the bin variable was added as a within-subject factor. An appropriate transformation was applied to the chronometric variables to meet the conditions of application of the ANOVA. The percentages were specifically submitted to arcsine transformation because it stabilizes the variance (Winer, 1962). Multiple pairwise comparisons were carried out with *p*-values adjustment using Tukey's method. In addition to *p*-values, the partial eta square was reported to assess relationships within the data.

To finish, Pearson correlations between objective and subjective measures were computed and *p*-values were corrected for multiple comparisons using the Bonferroni correction.

RESULTS

Control Variables and Pre-test

Analyses conducted on the two indicators of the PVT (i.e., number of omissions and the inverse of the RT) did not reveal any differences between the two groups, $Fs < 1$. We also analyzed whether the delay between pre-test and test session was equal between the two groups. It averaged 2.6 days ($SD = 1.9$) and there was no difference between the groups ($p > 0.1$). Finally, we controlled whether there was a difference between the two groups on the STD to ensure that neither group was better on the TLDB task. The STD (mean = 1,669; SD = 294) were statistically equivalent according to the cognitive load level ($p > 0.1$).

Subjective Scales

Beginning of the Test

To ensure that both groups reported equal levels of subjective fatigue, alertness, and sleepiness at the start of the task, we compared subjective assessments at the beginning of the study. On all scales, there was no difference between the two groups, $Fs < 2.94$, $ps > 0.1$.

The TLDB Task

To assess the evolution of the different subjective experiences induced by this task, we compared the scores of the scales completed before and after its completion. Regarding the two visual analog scales and the Samn–Perelli scale, the analysis showed an increase of their scores over time, $Fs_{(1, 21)} > 7.8$, $ps < 0.01$, $\eta_p^2 > 0.27$. However, no interaction was observed, $Fs_{(1, 21)} < 2.9$, $ps > 0.1$, $\eta_p^2 < 0.12$. This result suggests that it partially had the desired effect as we observed an increase of subjective fatigue equally for both groups. Indeed, we expected higher subjective fatigue in the HCL group. We also observed a main effect of cognitive load on the VASs score, $F_{(1, 21)} = 4.5$, $p < 0.05$, $\eta_p^2 = 0.18$, with HCL participants reporting higher levels of sleepiness. In other words, while the TLDB generated an increase in the subjective fatigue level, it seems that the manipulation of the cognitive load did not induce two different levels of fatigue. Finally, the mean scores obtained on the NASA RTLX scale were equal for both groups, $F_{(1, 21)} = 1.05$, $p > 0.1$, $\eta_p^2 = 0.05$. This result indicated that the participants attributed the same level of cognitive load to both tasks.

The Simon Task

This time we compared the scores before and after the Simon task. As after the TLDB task, participants in both groups reported a similar increase over time in scores on both visual analog scales and the Samn-Perelli scale, $Fs_{(1, 21)} > 8.2$, $p < 0.01$, $\eta_p^2 > 0.28$. No main effect of cognitive load or interaction between the two factors was observed, $Fs_{(1, 21)} < 2.9$, $ps > 0.1$, $\eta_p^2 > 0.12$. These results suggest that the level of subjective fatigue, sleepiness, and alertness continued to evolve in the same direction during the Simon task, regardless of the cognitive load initially used. Finally, participants in both groups reported the same level of cognitive load, $F_{(121)} = 2.4$, $p > 0.1$, $\eta_p^2 = 0.1$.

The Whole Study

We compared the scores at the beginning and the end of the study. Participants reported an increase over time in scores on both visual analog scales and the Samn-Perelli scale, $Fs_{(121)} > 18.7$, $ps < 0.001$, $\eta_p^2 > 0.47$. No effect of cognitive load, $Fs_{(1, 21)} < 2.2$, $ps > 0.1$, $\eta_p^2 < 0.09$, or interaction between cognitive load or time was observed, $Fs < 1$.

To sum up, these analyses indicated that subjective fatigue increased after the TLDB task and again after the Simon task. As expected, we observed that the TLDB task was effective in inducing subjective fatigue and we observed the presence of a time on task effect during the Simon task. However, the additional cognitive load in the HCL condition appears to have no impact on the level of subjective fatigue. The scores of each scale are presented in **Table 1**.

TABLE 1 | Subjective ratings for each scale according to the group, and the completion time.

	Scale				
	High/Low cognitive load				
Completion time	**VASf**	**VASs**	**Samn-Perelli**	**NASA RTLX**	**NASA RTLX–Effort**
Time 1	30.7/20.6	33.2/18.5	3.31/3		
Time 2	58.5/40.3	50.4/34.6	4.3/3.3	55/50	67.6/66.6
Time 3	67.4/55.9	59.2/56	4.9/4.4	53.8/40.9	63.9/59.5

VASf/s, visual analog scale evaluating cognitive fatigue/sleepiness.

The TLDB Task

Concerning RTs, on average participants were equally fast to respond during the two sub-tasks, $Fs < 1$. A main effect of block was observed on the 2-back task, $F_{(5,105)} = 2.9$, $p = 0.05$, $\eta_p^2 = 0.12$, and on the parity judgment task, $F_{(5,105)} = 6$, $p < 0.0001$, $\eta_p^2 = 0.22$. *Post-hoc* pairwise comparisons between the different blocks revealed a decrease in RT between the second and the last block (block2: 757 ms, block 6: 704 ms) during the 2-back task, $t_{(110)} = 3.04$, $p < 0.05$, and between the first and last block during the judgment parity task (block 1: 701 ms, block 6: 651 ms), $t_{(110)} = 4.3$, $p < 0.001$. The interaction of the factors was not significant for both tasks, $Fs < 1$.

Regarding accuracy, during the 2-back task participants in the LCL condition were more accurate than participants in the HCL condition (91 vs. 85%), $F_{(1, 21)} = 8.5$, $p < 0.01$, $\eta_p^2 = 0.29$. They were also better during the judgment parity task (98 vs. 95%), $F_{(1, 21)} = 9.7$, $p < 0.01$, $\eta_p^2 = 0.32$. This first analysis confirmed that the TLDB task performed by the HCL group was more difficult. In addition, the number of errors committed by both groups was stable across blocks in the 2-back task, $F_{(5,105)} = 1.5$, $p > 0.1$, $\eta_p^2 = 0.07$, and in the parity judgment task, $F < 1$. These two-way interactions between cognitive load and blocks were not significant for both tasks, $F_{(5,105)} = 1.4$, $p > 0.1$, $\eta_p^2 = 0.06$, (2-back task), $F_{(5,105)} = 1.3$, $p > 0.01$, $\eta_p^2 = 0.06$ (judgment parity task). We cannot infer from this result that cognitive fatigue was induced due to the stability of the performance relative to the time on task.

To summarize, while the analysis of the subjective measures seems to indicate that the TLDB task increases subjective fatigue over time, analysis of behavioral indicators show no degradation of performance over time. On the contrary, the decrease in RT over time suggests a learning effect. Crucially, the TLDB task failed to induce two different levels of cognitive fatigue both at the subjective and behavioral levels.

Effects of Cognitive Fatigue on Online Control During the Simon Task

Descriptive statistics of the behavioral measures assessed in the Simon task are presented in **Table 2**.

Classical Measures (RT and Accuracy)

Participants in the HCL group were not faster than participants in the LCL group, $F < 1$. RT was not modulated through blocks, $F < 1$. The compatibility effect was present (compatible: 349 ms; incompatible: 369 ms), $F_{(1, 21)} = 140.3$, $p < 0.0001$, $\eta_p^2 = 0.87$, but was not different between the two groups, $F < 1$, nor between blocks, $F_{(1, 21)} = 1.2$, $p > 0.1$, $\eta_p^2 = 0.05$. The interaction of all these factors was not significant, $F < 1$.

TABLE 2 | Descriptive statistics of the behavioral measures assessed in the Simon task.

	Block 1				Block 2				Block 3			
	Compatible		Incompatible		Compatible		Incompatible		Compatible		Incompatible	
	HCL	LCL	HCL	LCL	HCL	LCL	HCL	LCL	HCL	LCL	HCL	LCL
Acc (%)	95 (1)	96 (1)	93 (2)	94 (1)	95 (1)	97 (1)	91 (2)	93 (1)	95 (2)	97 (1)	90 (1)	92 (1)
RT (ms)	356 (19)	344 (13)	373 (20)	365 (14)	355 (20)	344 (14)	372 (20)	368 (15)	353 (19)	341 (12)	375 (22)	364 (13)
PMT (ms)	222 (12)	228 (11)	241 (13)	249 (11)	216 (14)	222 (13)	236 (14)	245 (14)	212 (12)	217 (10)	235 (15)	240 (11)
MT (ms)	134 (9)	161 (8)	132 (9)	116 (8)	139 (9)	122 (7)	136 (9)	123 (7)	141 (9)	124 (6)	140 (9)	123 (6)
IA (%)	19 (2)	15 (1)	37 (3)	32 (2)	20 (2)	16 (2)	39 (3)	34 (1)	21 (1)	18 (2)	40 (2)	37 (3)
CR (%)	76 (4)	78 (5)	82 (5)	80 (3)	78 (5)	78 (4)	78 (4)	79 (4)	78 (4)	83 (4)	74 (5)	79 (3)
	CC	CI	IC	II	CC	CI	IC	II	CC	CI	IC	II
RT (HCL)	346 (18)	387 (22)	366 (20)	360 (19)	345 (19)	378 (22)	365 (22)	363 (19)	347 (18)	382 (23)	357 (20)	367 (20)
RT (LCL)	335 (13)	375 (14)	352 (12)	357 (14)	334 (13)	374 (17)	354 (15)	362 (13)	332 (12)	369 (14)	348 (12)	354 (12)
Acc (HCL)	86 (2)	45 (4)	72 (3)	68 (4)	82 (2)	43 (4)	70 (3)	65 (3)	81 (2)	43 (4)	68 (1)	62 (4)
Acc (LCL)	89 (2)	53 (2)	75 (2)	77 (3)	87 (3)	51 (3)	77 (2)	72 (2)	84 (2)	46 (3)	74 (3)	67 (4)

Mean (standard error). HCL, High Cognitive Load; LCL, Low Cognitive Load; RT, Response Time; Acc, Accuracy; PMT, Pre-motor time; MT, Motor time; IA, Incorrect Activation rate; CR, Correction ratio; CC, Compatible–Compatible; CI, Compatible- Incompatible; IC, Incompatible–Compatible; II, Incompatible–Incompatible.

As for the RT, accuracy rate was statistically equal for both groups, $F < 1$, and did not decrease across blocks, $F_{(1, 21)} = 1.3$, $p > 0.1$, $\eta_p^2 = 0.061$. The compatibility effect was again observed (compatible: 96%; incompatible: 92%), $F_{(1, 21)} = 37.9$, $p < 0.0001$, $\eta_p^2 = 0.64$. It was also identical for both groups, $F < 1$, but as for RT, it increased through blocks (block 1: 3%; block 3: 5%), $F_{(1, 21)} = 24.8$, $p < 0.0001$, $\eta_p^2 = 0.54$. This evolution was not modulated by cognitive load, $F < 1$.

In summary, these results indicate that (1) the compatibility effect was present in both conditions, (2) it increased with time on task but (3) it was no different relative to the cognitive load level of the previous task. At this stage, we were unable to differentiate the role of automatic response activation and suppression mechanisms in the observed effect. The use of EMG measurements was intended to address this limitation.

Pre-motor Time and Motor Time

In order to determine whether cognitive fatigue influences decision and/or execution time, the latencies of pre-motor time and motor time were separated in the analyzes.

The pre-motor time was higher in incompatible trials compared to compatible trials (241 vs. 220 ms), $F_{(1, 21)} = 234.2$, $p < 0.0001$, $\eta_p^2 = 0.92$. It decreased with time on task (235 vs. 226 ms), $F_{(1, 21)} < 13.7$, $p < 0.001$, $\eta_p^2 = 0.39$ but was not modulated by cognitive load (228 vs. 234 ms for HCL and LCL group, respectively), $F < 1$. Moreover, no interaction was significant, $Fs < 1$.

We also observed a compatibility effect on motor time. It was higher for compatible trials (129 vs. 128 ms), $F_{(1, 21)} = 5.6$, $p < 0.05$, $\eta_p^2 = 0.21$. It increased with time on task (125 vs. 132 ms), $F_{(1,21)} = 4.5$, $p < 0.05$, $\eta_p^2 = 0.18$, but with the same extent for the two groups, $F_{(1, 21)} = 2.2$, $p > 0.1$, $\eta_p^2 = 0.16$. All interactions were not significant, $Fs < 1$. These measures are illustrated in **Figure 3**.

The decomposition of the RT into pre-motor time and motor time reveals that these two indicators were affected by cognitive fatigue and had an opposite dynamic with time on task.

Incorrect Activation Rate and Correction Ratio

An increase in the number of incorrect automatic activations only during incompatible trials with cognitive fatigue would correspond to an increase in response capture while a decrease in the correction ratio would inform on the disruption of the suppression mechanism.

Participants made more incorrect activations in incompatible trials (35 vs. 18%), $F_{(1, 21)} = 261.5$, $p < 0.0001$, $\eta_p^2 = 0.93$. In addition, compared to the first block, more incorrect activations were found during the last block (25 vs. 28%), $F_{(1, 21)} = 11.8$, $p < 0.01$, $\eta_p^2 = 0.36$. But there was not a main effect of cognitive load, $F_{(1, 21)} = 2.3$, $p > 0.1$, $\eta_p^2 = 0.1$. The difference observed according to the trial compatibility of the trial was not influenced by cognitive load, $F < 1$, or by blocks, $F < 1$. The interaction of the three factors was also not significant, $F < 1$. This analysis highlighted that, contrary to time on task, the cognitive load level of the TLDB task did not change the number of incorrect activations. However, they increased in both types of trials, whereas we expected an increase only during incompatible trials. The presence of incorrect activations during compatible trials can be interpreted as fast guesses. Thus, this result cannot be fully interpreted as an increase in the capture of incorrect responses over time because of the presence of fast guesses during compatible trials.

Analysis on the correction ratio showed that no effect was significant, $Fs < 1$, except the interaction indicating a change in the compatibility effect through blocks, $F_{(1, 21)} = 19.6$, $p < 0.001$, $\eta_p^2 = 0.48$. The correction ratio on compatible trials remained stable across blocks (77 vs. 80%), $F_{(1,22)} = 3.5$, $p > 0.1$, $\eta_p^2 = 0.14$, while it decreased on incompatible trials (81 vs. 76%), $F_{(1,22)} =$

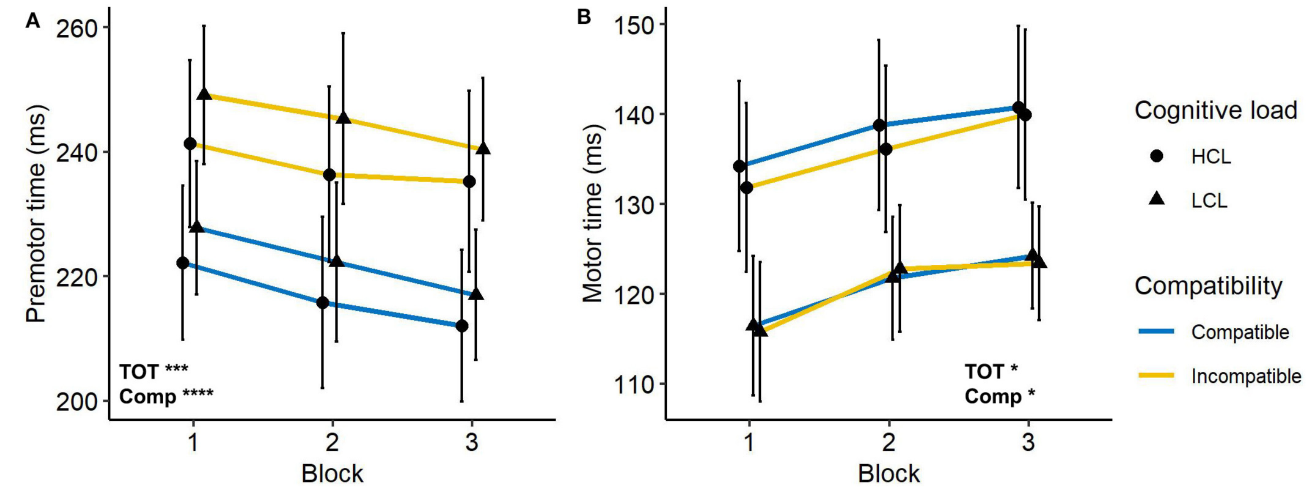

FIGURE 3 | (A) Pre-motor time and **(B)** motor time as a function of trial compatibility, cognitive load, and block. Error bars represent the standard error. TOT, main effect of Time-on-Task; Comp, main effect of Compatibility. *$p < 0.05$; ***$p < 0.001$; ****$p < 0.0001$.

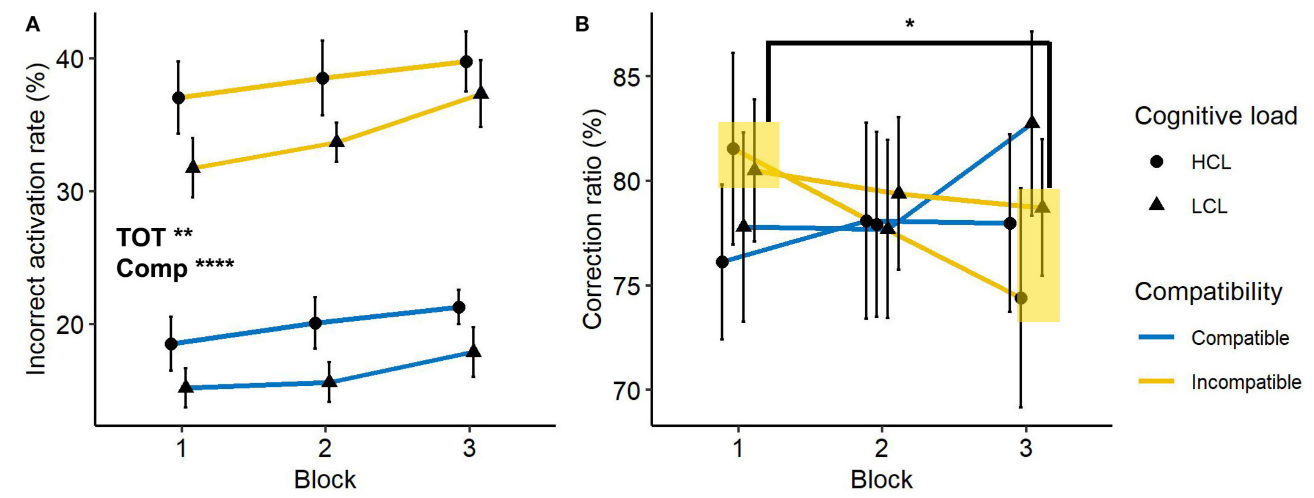

FIGURE 4 | (A) Incorrect activation rate and **(B)** correction ratio as a function of trial compatibility, cognitive load and block. Error bars represent the standard error. TOT, main effect of Time-on-Task; Comp, main effect of Compatibility. *$p < 0.05$; **$p < 0.01$; ****$p < 0.0001$.

6.4, $p < 0.05$, $\eta_p^2 = 0.22$. Besides making more errors, participants were also less able to correct them during an incompatible trial with time on task. These measures are illustrated in **Figure 4**.

Taken together, our results indicate that time on task affects both incorrect activation and correction ratio, whereas no effect of cognitive load was observed. Critically, incorrect activations appear to be impacted in both compatible and incompatible trials.

Distributional Analysis—The Conditional Incorrect Accuracy Function

The conditional incorrect accuracy function aimed to explore the strength and the time course of the automatic response activation. An increase in this strength with time on task should be observed in the first bins. Examination of the conditional incorrect accuracy function (**Figure 5**) revealed a significant interaction between compatibility and bins, indicating an uneven distribution of the compatibility effect between the different bins, $F_{(4,84)} = 57.7$, $p < 0.0001$, $\eta_p^2 = 0.73$. Multiple pairwise comparisons revealed that the compatibility effect was higher in the first bin than in the fourth (40 vs. −4%), $t_{(88)} = 7.9$, $p < 0.001$). This effect was most pronounced in the second bin (44 vs. −4%). Statistically, it was no larger than the effect observed in the first bin, $t_{(88)} = -1.05$, $p > 0.1$), but larger than the effect observed in the third bin, $t_{(88)} = 2.97$, $p < 0.05$). These results confirmed that a response capture occurred because the compatibility effect was higher during short trials and equalized as the pre-motor time lengthened. This interaction was not modulated by cognitive load, $F < 1$, but by blocks, $F_{(4,84)} = 2.8$, $p < 0.05$, $\eta_p^2 = 0.12$. We isolated the first and the second bins to see if the interaction was still present, but it was not, $Fs < 1$. The interaction of all these factors was not significant, $F_{(4,84)} =$

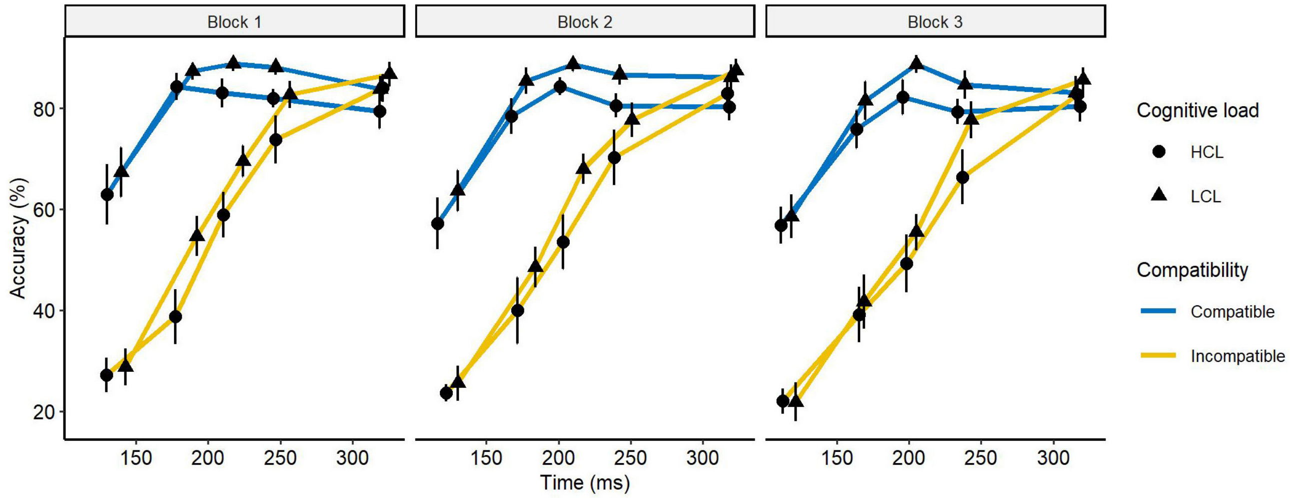

FIGURE 5 | Conditional incorrect activations function for compatible and incompatible trials as a function of cognitive load and block. Error bars represent the standard error.

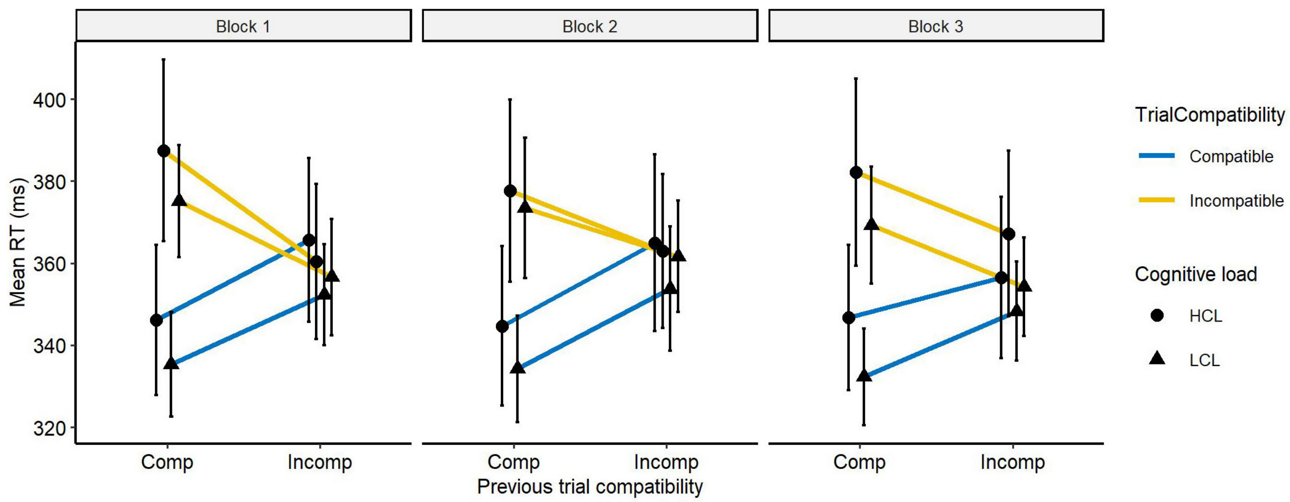

FIGURE 6 | Response time (in ms) as a function of cognitive load, trial compatibility (compatible and incompatible), and compatibility of the previous trial (compatible and incompatible). Error bars represent the standard error.

1.9, $p > 0.1$, $\eta_p^2 = 0.08$. The result of this analysis suggests that the strength of the automatic response remains the same without being modified by cognitive load or time on task.

In summary, our assessment of online control suggests that the increase over time in the number of incorrect activations during incompatible trials is caused by a suppression deficit since the strength of the automatic response remains stable over time. On the other hand, response capture does not appear to be impacted by time on task.

Effects of Cognitive Fatigue on Anticipatory Control

To evaluate the effect of cognitive fatigue on anticipatory control, we analyzed the evolution of the Gratton effect. A disruption in anticipatory control should be evidenced by an increase in the compatibility effect after an incompatible trial with time on task (**Figure 6**). A first analysis on accuracy revealed a larger compatibility effect after a compatible trial than after an incompatible trial (39 vs. 4%), $F_{(1, 21)} = 124.9$, $p < 0.0001$, $\eta_p^2 = 0.86$. Thus, according to the literature, participants adapted their behavior after an incompatible trial resulting almost by the disappearance of the compatibility effect after these trials. This observation was not modulated by the cognitive load, $F < 1$. Nevertheless, it increased through blocks, $F_{(1, 21)} = 5.5$, $p < 0.05$, $\eta_p^2 = 0.21$. But taken separately, the compatibility effect computed after an incompatible trial, $F_{(1, 21)} = 2.9$, $p > 0.1$, $\eta_p^2 = 0.12$, or a compatible trial, $F_{(1, 21)} = 3$, $p > 0.1$, $\eta_p^2 = 0.12$, did not increase across blocks. The interaction of all these variables was not significant, $F < 1$.

As with accuracy, the RT analysis indicates a higher compatibility effect after a compatible trial than after an incompatible trial, (38 vs. 4 ms), $F_{(1, 21)} = 48$, $p < 0.0001$, $\eta_p^2 = 0.7$. Cognitive load had no effect, $F < 1$. On the other hand, the compatibility effect was different with time on task, $F_{(1, 21)} = 9.4$, $p < 0.01$, $\eta_p^2 = 0.31$, and the interaction of these variables showed a trend, $F_{(1, 21)} = 4$, $p = 0.06$, $\eta_p^2 = 0.16$. Although the interaction was not significant, we chose to explore whether a group effect was present. Thus, we separated the analysis according to the compatibility of the preceding trial. After a compatible trial, the compatibility effect remained stable with time on task for both groups, $Fs < 1$. On the other hand, after an incompatible trial, this effect increased with time on task for participants in the HCL group (block 1: −5 ms, block 3: 11 ms), $F_{(1,11)} = 7.3$, $p < 0.05$, $\eta_p^2 = 0.4$, but this was not the case for the other group (block 1: 4 ms, block 3: 6 ms), $F < 1$.

To sum up, our results demonstrate no effect of cognitive fatigue on anticipatory control. There is a trend on RT showing that for the HCL group the compatibility effect after an incompatible trial seems to increase. However, these results are based on exploratory analyses and should be taken with caution.

Correlations Between Objective and Subjective Measures

We assessed whether the effects we observed were correlated with subjective measures. To this end, we computed the difference of the EMG measures (i.e., correction ratio, pre-motor time, motor time, and incorrect activations rate) obtained during the first and the third block of the Simon task and we evaluated the correlation between these differences and the evolution over time of subjective measures. More specifically, we separated the analyses according to the scores obtained during the TLDB task and those obtained during the Simon task. We proceeded in this manner because sometimes subjective experience precedes behavioral alterations. Therefore, we suggested that the perceived effort or increase in subjective fatigue following completion of the TLDB task could correlate with the behavioral effects of cognitive fatigue observed during the Simon task. We postulated that neither the evolution over time of sleepiness nor alertness should correlate with performance decrements. Given the large number of behavioral-subjective associations which was tested, we have considered the results of analyses below a threshold of $p < 0.0016$ to be significant.

We only observed a negative correlation between the reduction in the correction ratio and the subscale of the NASA RTLX measuring effort filled after the TLDB task (Pearson $r = -0.42$, $p < 0.05$). Thus, when participants reported a higher effort during the inducing task, they tended to be less effective to suppress the activation of the incorrect response during the Simon task (**Figure 7**). However, this result should therefore be considered with caution because the p-value was higher than the correction threshold we defined. Although the observed correlation was no longer present once the correction was applied, it confirms that separating perceived effort and subjective fatigue could be necessary. Importantly, neither the evolution over time of sleepiness or alertness correlated with performance decrements, as postulated.

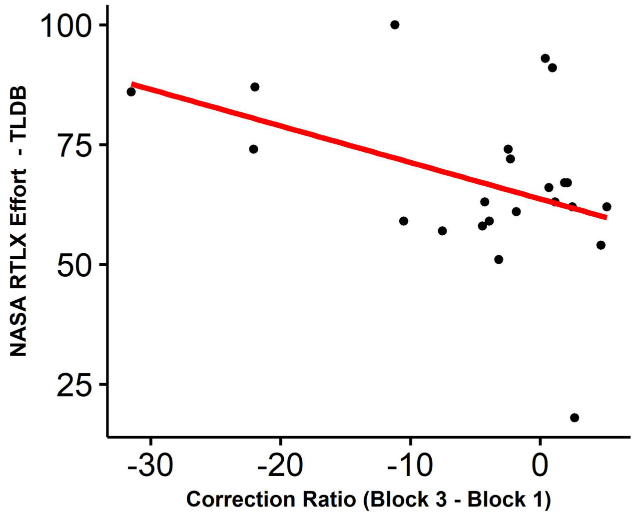

FIGURE 7 | Correlation among (x) the difference between the correction ratio obtained in the third and the first block and (y) one subscale of the NASA RTLX that measures subjective effort related to the TLDB task.

DISCUSSION

The objective of this study was 2 fold. The first was to clarify the effect of cognitive fatigue on the two types of action control, online, and anticipatory control, during a conflict task. To achieve this, we relied on tools allowing a more detailed evaluation of these controls, the EMG and distribution analyses. This allows us to evaluate separately the automatic response activation and the response suppression, two mechanisms constituting online control. Moreover, the distinction between correct trials from those containing a partial error improves the accuracy of their evaluation. We observed during the two types of trials of the Simon task (i.e., compatible and incompatible) an increase in the number of incorrect activations as a function of time on task. This result was not attributed to an increase in the strength of the response capture. The presence of fast guesses rather suggests that the suppression mechanism was less engaged by participants and that they adopted a faster response strategy. Anticipatory control was not modulated by cognitive fatigue. The second objective was to evaluate the relation between objective and subjective measures, and we did not observe any. In the following sections, we will discuss these results and in particular the fact that they can be explained by the disengagement of effort.

The Effect of Cognitive Fatigue on Action Control During the Simon Task

The results suggest that cognitive fatigue changed the way participants responded. Specifically, we observed that the suppression mechanism was less engaged with cognitive fatigue while the strength of the response activation remained the same.

To assess whether cognitive fatigue negatively impacted this suppression mechanism, we analyzed the correction ratio which reflects the efficiency of this mechanism. We observed for both groups that during incompatible trials the correction ratio decreased over time, suggesting that the suppression mechanism was less engaged with time on task. Importantly, this was not accompanied by an increase in the strength of automatic response activation. Indeed, the distribution of the number of incorrect activations obtained with the conditional incorrect accuracy function, especially on the first bins, remained stable over time. Since the reduction in the correction ratio over time was not accompanied by an increase in the strength of the automatic response activation, we can conclude that cognitive fatigue disrupts online control by impairing the suppression mechanism.

This result is consistent with those usually observed in the Go-noGo or stop-signal paradigms. Although these tasks rely on the involvement of different cerebral regions, in part because they do not require the choice of one response among several alternatives, these studies have frequently demonstrated that the suppression mechanism is less effective with cognitive fatigue, both at the behavioral and electrophysiological levels (e.g., Kato et al., 2009). However, as in conflict tasks, opposing results have been found (e.g., Falkenstein et al., 2002). It is possible that the strength of the automatic response activation was too large and exceeded the suppression capacity. For example, Freeman and Aron (2016) observed that when participants were fatigued, it was more difficult for them to inhibit a motor response when a high reward was associated with the stimulus, but this was not the case when the value of the reward was low (Freeman and Aron, 2016). It is assumed that assessing the strength of the response capture may provide clarifications in these paradigms. By differentiating these two mechanisms using EMG and distribution analysis, i.e., the strength of the automatic response activation and the response suppression mechanism, our results add new evidence in favor of an effect of cognitive fatigue on the suppression mechanism whereas no effect on the strength of the automatic response activation is observed.

Aside from the effects of cognitive fatigue on online control, we did not show that adaptation to the subsequent trial was less effective. The exploration of our results suggests a trend toward an increase in the Gratton effect over time but only for the group that performed the TLDB task with a high level of cognitive load. However, this result is not supported by a significant interaction, and should hence be considered as exploratory and be taken with caution. The low statistical power caused by our small sample size and the design of our experiment (i.e., between-subject comparison of the two groups) could be responsible for the absence of an observed effect.

The Relation Between Objective and Subjective Measures

In this study, we evaluated the correlation between subjective fatigue, perceived effort, and EMG measures. In particular, we evaluated whether subjective measures assessed during a first task correlated with performance decrements observed during a second task. We observed that only perceived effort in completing the TLDB task correlated with the decrease in the correction ratio observed during the Simon task. However, this correlation should be considered with caution as it was no longer significant once the correction for multiple comparisons was considered. If it had been significant, this observation would have been consistent with motivational models of cognitive fatigue (Müller and Apps, 2019). These models suggest that cognitive fatigue increases the cost of effort and, if it becomes too high, participants stop making effort.

We observed no correlation between subjective fatigue and performance decrements. However, we did observe an increase in subjective fatigue, already during the TLDB task, while no decrease in performance was observed. This result is in line with Hockey's model, which indicates that subjective fatigue reflects the presence of a compensatory phenomenon which aims to maintain the level of performance (Hockey, 2013). This increase was independent of cognitive load. It continued to evolve in this direction during the Simon task and therefore increased throughout the study. The absence of difference according to the cognitive load can be explained by the fact that we have not been able to induce two different levels of cognitive load. Anyway, our results confirm the absence of a relationship between subjective and objective fatigue and show that EMG measures are not more sensitive than traditional behavioral measures.

Finally, we observed in this study an increase in sleepiness and a decrease in alertness. This result is not surprising given the duration of the task. However, it should be noted that these measures also did not correlate with performance decrements, which was consistent with our hypotheses.

Cognitive Fatigue and Action Control: A Disengagement of Cognitive Effort

The previous results have shown that cognitive fatigue impaired only online control through a reduced involvement of the suppression mechanism. The implementation of this mechanism requires cognitive effort (Botvinick et al., 2001; Ridderinkhof et al., 2011; Ullsperger et al., 2014). Several results in our study suggest that with time on task, participants no longer engaged cognitive effort to the same extent. This assumption is consistent with models arguing that a decrease in the willingness to exert cognitive effort is associated with cognitive fatigue (Hockey, 2013; Massar et al., 2018; Müller and Apps, 2019).

We found that, for both groups, the number of incorrect activations increased with time on task regardless of trial compatibility. This increase observed in compatible trials means that participants may have changed their response strategy. More importantly, the presence of fast guess errors indicates that participants adapted their response strategy to respond more quickly. This suggests that they were no longer fully engaged in the task rather than an inability to perform the task, such as after a decrease of resources. In line with this, a speed-accuracy tradeoff has been observed. Indeed, in addition to the increase in the number of incorrect activations, a decrease in pre-motor time was observed with time on task. The evolution over time of this chronometric measure was observed regardless of the cognitive load level and trial compatibility. This association (i.e., reduction in the pre-motor time and increase in the number of incorrect activations) may suggest the presence of a speed-accuracy tradeoff. The presence of a speed-accuracy tradeoff has already been noted with cognitive fatigue. For example, Laurent et al. (2013) observed this speed-accuracy tradeoff during the last blocks of a switching task. With time on task, participants were faster but less accurate (Laurent et al., 2013). Importantly, this result is sometimes attributed to an effort disengagement, which is consistent with our observations. Indeed, this speed-accuracy tradeoff was not limited to incompatible trials, i.e., trials requiring effort. Since compatible trials were also concerned, a disengagement rather than an inability to exert effort may be suggested. This result has already been observed when disinvestment of effort was provoked, as in studies distributing a reward based on performance. In these studies, participants prefer to allocate effort on trials with high rewards but behaved inversely on trials with lower rewards. In this case, they exhibited avoidance behavior, choosing not to exert their effort and emphasizing speed over accuracy (Hübner and Schlösser, 2010; Otto and Daw, 2019).

Several studies have noted that cognitive fatigue leads to difficulties in sustaining cognitive effort. However, they have not always observed a speed-accuracy tradeoff (Wascher et al., 2014). In these studies, the RT was not separated into motor time and pre-motor time and errors into partials errors and "true" errors, which may explain some of the variability in results. Indeed, in our study, the two components showed an opposite trend (i.e. motor time increased and pre-motor time decreased with time on task). But when combined, a marginal increase with time on task was observed. Therefore, it is likely that this pattern of results was also present in previous studies, but that it was masked by the evaluation of conventional measures only.

However, the presence of a speed-accuracy tradeoff in our data could be questioned since it was not observed when we considered the motor time. Nevertheless, it is widely accepted that speed-accuracy tradeoff would mainly affect decision processes (Bogacz et al., 2010). Mathematical models of decision-making (e.g., Ratcliff and McKoon, 2008) argue that when speed is emphasized over accuracy, the amount of information accumulated to generate a response is faster due to a lower decision threshold, which could more easily lead to an incorrect decision and therefore to an error. The proposals of these models are supported by brain imaging studies (e.g., fMRI) showing, for example, that only a fluctuation in brain activity of the regions involved in decision-making (e.g., dorsolateral prefrontal cortex and pre-supplementary motor area) was observed when instructions emphasize speed (Forstmann et al., 2008; van Veen et al., 2008). Recently, observations have shown that the non-decision components, including motor components, could be also affected by this speed-accuracy tradeoff. For example, Spieser et al. (2017) observed that during a conflict task, when instructions emphasized speed, motor time was also reduced (Spieser et al., 2017). However, unlike our study, effort demand was not manipulated. Furthermore, the decrease in motor time may be masked by changes induced by the presence of cognitive fatigue. In contrast to pre-motor time, an increase in motor time with cognitive fatigue has been previously reported after sleep deprivation (Ramdani et al., 2013). A prolonged cognitive effort also generates an increase in sleepiness, which is also observed in our study. As proposed by Ramdani et al. (2013), sleepiness, especially induced by sleep deprivation, may decrease cortico-spinal excitation and muscle tension, which have been previously reported to affect motor time (Possamai et al., 2002; De Gennaro et al., 2007). Thus, sleepiness may have increased motor time in our study.

To conclude, all our results suggest that cognitive fatigue causes disengagement from cognitive effort. With cognitive fatigue, participants implemented online control to a lesser extent. Besides this result, they opted for an effortless response strategy by emphasizing speed over accuracy. These results are consistent with the motivational view of cognitive fatigue (Hockey, 2013; Müller and Apps, 2019). Although the observed correlation between the decrease in correction ratio and perceived effort during the TLDB task was no longer observed once the correction was applied, it is consistent with this interpretation.

LIMITATIONS AND PERSPECTIVES

Some limitations can be mentioned in this study. First, our sample size was small. The low statistical power could be responsible for the absence of difference between the two groups. However, it could also be explained by the proximity of the two TLDB tasks. We distinguished the two tasks by manipulating the cognitive load. Therefore, it is likely that the manipulation did not induce a large difference between the groups. The TLDB task, even in the simplest configuration, was still a complex dual-task. The small sample size also implies to be cautious with the interpretation of the results of the correlation analysis since it can be influenced by extreme values. In our opinion, our results fit well with motivational theories of cognitive fatigue. But we relied on indirect indicators. Assessing participant motivation to accomplish the task could have been important. Also, it might have been interesting to assess whether the response suppression and response capture mechanisms would have been modulated according to trial compatibility to broaden our understanding of the effects of cognitive fatigue on anticipatory control [see for e.g., Wylie et al. (2010) for such analyses]. However, this analysis was not possible because of the limited number of trials in our experiment. Our results showed that cognitive fatigue disturbs only online control rather than anticipated control. The design of our Simon task does not emphasize the use of anticipatory control. Thus, online control may have been primarily hampered because it was more widely used by participants. But this remains to be tested.

CONCLUSION

To conclude, our results show the important contribution of EMG and distribution analyses. The measures they provide have led to a better understanding of the effect of cognitive fatigue on action control than traditional measures. This study demonstrated that cognitive fatigue leads to disengagement of effort resulting in impaired online and anticipatory control. Given the importance of adaptive capabilities for the safety of critical systems, these results are important as they provide a better understanding of the effects of fatigue on these capabilities.

DATA AVAILABILITY STATEMENT

The raw data supporting the conclusions of this article will be made available by the authors, without undue reservation.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by Comité de Protection des Personnes Sud Méditerranée 1. The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

All authors listed have made a substantial, direct and intellectual contribution to the work, and approved it for publication.

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Using Signal Detection Theory to Better Understand Cognitive Fatigue

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When we are fatigued, we feel that our performance is worse than when we are fresh. Yet, for over 100 years, researchers have been unable to identify an objective, behavioral measure that covaries with the subjective experience of fatigue. Previous work suggests that the metrics of signal detection theory (SDT)—response bias (criterion) and perceptual certainty (*d*')—may change as a function of fatigue, but no work has yet been done to examine whether these metrics covary with fatigue. Here, we investigated cognitive fatigue using SDT. We induced fatigue through repetitive performance of the *n*-back working memory task, while functional magnetic resonance imaging (fMRI) data was acquired. We also assessed cognitive fatigue at intervals throughout. This enabled us to assess not only whether criterion and *d*' covary with cognitive fatigue but also whether similar patterns of brain activation underlie cognitive fatigue and SDT measures. Our results show that both criterion and *d*' were correlated with changes in cognitive fatigue: as fatigue increased, subjects became more conservative in their response bias and their perceptual certainty declined. Furthermore, activation in the striatum of the basal ganglia was also related to cognitive fatigue, criterion, and *d*'. These results suggest that SDT measures represent an objective measure of cognitive fatigue. Additionally, the overlap and difference in the fMRI results between cognitive fatigue and SDT measures indicate that these measures are related while also separate. In sum, we show the relevance of SDT measures in the understanding of fatigue, thus providing researchers with a new set of tools with which to better understand the nature and consequences of cognitive fatigue.

Keywords: signal detection theory, fMRI, cognitive fatigue, working memory, striatum

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INTRODUCTION

Fatigue resulting from cognitive work (cognitive fatigue) is a common experience, caused by tasks that require care and skill such as air traffic control (Orasanu et al., 2012; Kuo et al., 2017) or driving (Matthews and Desmond, 2002). Furthermore, cognitive fatigue is a common sequela following brain injury [e.g., traumatic brain injury (TBI) or stroke] or disease [e.g., multiple sclerosis (MS) or Parkinson's disease]. Intuitively, we feel that performance should decline as cognitive fatigue increases, yet a large body of research shows that this is not the case (Craig and Cooper, 1992;

Stoner, 1996; Torres-Harding and Leonard, 2005). The disappointing lack of correlation between the subjective feelings of cognitive fatigue and objective measures of performance such as response time (RT) and accuracy has hampered research in this area. However, fatigue has been linked to decrements in perceptual sensitivity [i.e., a reduced ability to distinguish stimuli requiring a response (targets) from stimuli that do not require a response (non-targets)]—or *d*', a measure derived from signal detection theory (SDT) (Green and Swets, 1966; Lynn and Barrett, 2014)—in the human factors literature (Matthews and Desmond, 2002), which may be linked to well-documented decrements in *d*' associated with vigilance tasks (See et al., 1995). For example, Matthews and Desmond (2002) found that perceptual sensitivity was reduced and fatigue was increased following a difficult "drive" in a driving simulator (relative to an easier drive). Thus, while simple RT and accuracy correlate poorly with fatigue, the tools of SDT (and perceptual sensitivity in particular) may provide better objective indices of fatigue. However, while decrements in *d*' have been demonstrated after fatigue has been induced (i.e., before vs. after fatigue induction), it has not been shown that progressive increases in fatigue are associated with progressive decreases in perceptual sensitivity. Showing a correlation of this sort between *d*' and fatigue would provide researchers with a powerful tool to better understand fatigue.

While perceptual sensitivity (*d*') has been shown to be worse after fatigue induction (Matthews and Desmond, 2002), the effect of fatigue on bias (β), or criterion, which is the other main SDT measure, has not been investigated. In the context of SDT, criterion refers to the amount of evidence one requires before releasing a response: a liberal criterion means that one requires relatively little evidence that a stimulus is a target before releasing a response; a conservative criterion means that one requires relatively more evidence before releasing a response. It is somewhat surprising that changes in criterion have not been investigated in the fatigue literature since recent investigations into fatigue have suggested that fatigue reflects, at least in part, a change in the balance between effort and reward (Dobryakova et al., 2015; Wylie et al., 2017b; Massar et al., 2018; Müller and Apps, 2018). Signal detection theory predicts that changes in the payoff matrix—that is, the balance between effort and reward—will be reflected in changes in criterion. It has been repeatedly shown that changing the payoff matrix by increasing the reward subjects receive reduces fatigue (Matthews and Desmond, 2002; Boksem et al., 2006; Lorist et al., 2009), but hitherto, there have been no investigations into whether changes in fatigue are correlated with changes in criterion.

In the work described here, we induced fatigue by asking subjects to repeatedly perform two conditions of the *n*-back working memory task: the 0-back condition and the 2-back condition (Wylie et al., 2017a,b, 2018). By using the accuracy on different types of trials (correct rejections and false alarms), we calculated subjects' sensitivity and their response bias, using SDT (Green and Swets, 1966; Lynn and Barrett, 2014). Furthermore, at baseline, and after each of the eight runs of the tasks, we assessed subjects' cognitive fatigue using the visual analog scale of fatigue (VAS-F) (Shahid et al., 2011). This design allowed us to assess whether changes in perceptual sensitivity and criterion were correlated with subjective reports of cognitive fatigue. Finally, both structural and functional magnetic resonance imaging (fMRI) data were acquired while subjects performed the tasks. This allowed us to assess whether brain areas that were sensitive to changes in cognitive fatigue were also sensitive to changes in perceptual certainty and/or criterion. Based on the literature (Chaudhuri and Behan, 2004), and on our previous work (Dobryakova et al., 2017), we hypothesized that the striatum of the basal ganglia would play a central role. Several studies, both from our lab (e.g., Dobryakova et al., 2013, 2017; Wylie et al., 2017a) and from others (e.g., Chaudhuri and Behan, 2004; Tang et al., 2013; Nakagawa et al., 2016), have indicated that the striatum in general and the caudate in particular are implicated in fatigue. The role of the striatum was assessed both from a structural standpoint—investigating whether the volume of the striatum covaried with cognitive fatigue and SDT measures—and from a functional standpoint—investigating whether activation in the striatum covaried with cognitive fatigue and SDT measures.

MATERIALS AND METHODS

Subjects

Forty-eight healthy volunteers participated in this study. The behavioral data from nine of these subjects were not available due to equipment failure. Of the remaining 39 subjects, their mean age was 43.8 years (± 11.7), and their mean education was 15.4 years (± 2.3), and 15 were women.

Neuroimaging Acquisition

Neuroimaging data collection began on a 3-Tesla Siemens Allegra scanner (24 subjects) and was completed on a 3-Tesla Siemens Skyra scanner (15 subjects). For this reason, a regressor for scanner was included in all group-level analyses, as has been done in previous research utilizing more than one scanner (Stonnington et al., 2008; Biswal et al., 2010; Wylie et al., 2018). A T2*-weighted echo planar sequence was used to collect functional images during eight blocks (four at each of two difficulty levels), with 140 brain volume acquisitions per block (Allegra: echo time = 30 ms; repetition time = 2,000 ms; field of view = 22 cm; flip angle = 80°; slice thickness = 4 mm, 32 slices, matrix = 64 × 64, in-plane resolution = 3.438 × 3.438 mm^2; Skyra: echo time = 30 ms; repetition time = 2,000 ms; field of view = 22 cm; flip angle = 90°; slice thickness = 4 mm, 32 slices, matrix = 92 × 92, in-plane resolution = 2.391 × 2.391 mm^2). A high-resolution magnetization-prepared rapid gradient echo (MPRAGE) image was also acquired (Allegra: TE = 4.38 ms; TR = 2,000 ms, FOV = 220 mm; flip angle = 8°; slice thickness = 1 mm, NEX = 1, matrix = 256 × 256, in-plane resolution = 0.859 × 0.859 mm^2; Skyra: TE = 3.43 ms; TR = 2,100 ms, FOV = 256 mm; flip angle = 9°; slice thickness = 1 mm, NEX = 1, matrix = 256 × 256, in-plane resolution = 1 × 1 mm^2) and was used to register the functional data into standard MNI space for group analysis and for the volumetric analyses.

Behavioral Paradigm and Data

Behavioral data acquisition and stimulus presentation were administered using the E-Prime software (Schneider et al., 2002). During the fMRI scan, participants were presented with the *n*-back working memory task in which task difficulty was varied by presenting the 0-back condition, which places a low load on working memory, and the 2-back condition, which places a higher load on working memory. There were four blocks of each level of the *n*-back task (eight blocks total), with 65 trials per block. The four blocks of each task were always presented together (that is, the two tasks were not interleaved), and the order of presentation (0-back first vs. 2-back first) was counterbalanced across subjects. During the 0-back task (control task), participants were asked to respond each time the target letter "K" was presented on the screen, while during the 2-back task, participants were asked to respond when the target letter corresponded to the letter presented two trials before (e.g., R N Q N. . .). Letters were presented in white (Arial 72 point font) on a black background. Of the 26 letters in the English alphabet, 10 were excluded to enhance the discriminability of the letters used as stimuli. The following letters were used (with equal frequency): A, B, C, D, F, H, J, K, M, N, P, Q, R, S, T, V, and Z. The letter stimuli remained on the screen for 1.5 s, followed by a 500 ms inter-trial interval (ITI), and the time between successive trials was jittered to allow for the data to be deconvolved as an event-related design. The jittering was optimized using the Optseq2 program[1]. The jittering was achieved by inserting between zero and six null events between successive trials. The duration of each null event was a multiple of the length of the trial (in this case, 2 s), drawn from a distribution following a power function. The majority of inter-trial intervals were 500 ms (zero null events), followed by 2 s (one null event) and so on. The average ITI was 1,587.87 ms (±1,769.7). All subjects practiced both tasks prior to the scanning session.

In order to ensure comparable stimulation across subjects, the stimuli always remained on the screen for 1.5 s (that is, they were not removed when subjects responded), and each run lasted the same amount of time (260 s). The average amount of time between successive blocks was 2 min 04 s (*SD* = 2 min 17 s).

The following behavioral data were analyzed: overall accuracy, which was the number of trials in which the correct response was made divided by the total number of trials, the reaction times (RTs) of the correct trials, and signal detection metrics. Signal detection analysis was used to separate discrimination sensitivity from response bias—factors that can independently affect accuracy (Macmillan and Creelman, 1991; Anderson et al., 2011). The ability to correctly identify target stimuli was measured using the discriminability index (*d*'), calculated as (zFA - zHR), where z is the inverse of the standard normal cumulative distribution, FA is the false-alarm rate (the proportion of responses made to stimuli that were not targets), and HR is the hit rate (the proportion of correct identifications of target stimuli). In the context of this experiment, where all stimuli were readily discernable, *d*' is best thought of as perceptual certainty rather than as sensitivity to stimulation. Response bias was measured using "criterion" (β), calculated as $-1/2(z\text{HR} + z\text{FA})$ with higher values (fewer false alarms and fewer hits) indicating reduced response bias or more conservative responding. Lower criterion values (more hits and more false alarms) indicated increased response bias and more liberal responding.

[1]https://surfer.nmr.mgh.harvard.edu/optseq/

VAS-F

To evaluate the level of on-task or "state" fatigue, participants were presented with a visual analog scale (VAS) before and after each block of the *n*-back task. Participants were asked: "How mentally fatigued are you right now?" and were asked to indicate their level of fatigue on a scale from 0 to 100, with 0 being not fatigued at all and 100 being extremely fatigued. In order to mask the purpose of the study, five additional VASs were administered as well, in randomized order. These assessed happiness, sadness, pain, tension, and anger.

Because VAS-F scores were obtained before and after each run, the amount of fatigue during each block was estimated by using the mean of the scores before and after the relevant block; this value was used in the correlational analyses. Furthermore, because we were specifically interested in cognitive fatigue, we divided the data into blocks on which subjects reported at least some fatigue and blocks on which they reported no fatigue (zero on the VAS-F; see **Table 1**). This was done because it is reasonable to hypothesize that when at least some fatigue was reported, subjects were engaged in the task and that fatigue-related areas should be active. However, when no fatigue was reported, it is less clear what to hypothesize. This may have represented a failure of introspection, in which case it would be a mistake to attempt to relate the fatigue score to brain activation. Alternatively, it could represent zero fatigue, which might be related to minimal activation (or even deactivation) in fatigue-related areas, or it could represent some combination of these cases. Because of this, we felt it more straightforward to analyze only those data for which we had clear hypotheses. A chi-squared test showed the number of runs with and without fatigue was comparable across the two tasks ($\chi^2(1) = 1.40$, $p = 0.24$). The blocks on which subjects reported at least some fatigue were used for the main analyses. Finally, because the VAS-F scores were skewed, they were transformed using the Box-Cox method to ensure that assumptions of normality were not violated (Box and Cox, 1964). The Box-Cox method is a power transformation in which a range of power transformations are considered and the one that best transforms the data into a normal distribution is selected.

TABLE 1 | Number and percentages of runs on which subjects reported no fatigue relative to runs where they reported at least some fatigue, as a function of task (0-back vs. 2-back).

	0-Back	2-Back
Fatigue	115 (76%)	102 (69%)
No fatigue	36 (24%)	45 (31%)

Analyses

RT and Accuracy

Mean RT was calculated using accurate trials. For both the RT and accuracy data, a linear mixed effects [LME; using the R statistical package (version 3.4.3)] was used with the factors of task (0-back vs. 2-back), run (runs 1–4 of each task), and VAS-F (the visual analog scale of fatigue) as a quantitative variable; subject was a random factor.

SDT Measures (*d*' and Bias)

For each of the SDT measures [sensitivity (*d*') and response bias], an LME was used with the factors of task (0-back vs. 2-back), run (runs 1–4 of each task), and VAS-F (using the same transformed and averaged values as were used for the RT and accuracy analyses), as a quantitative variable and subject was included as a random factor.

Neuroimaging

The neuroimaging data was preprocessed using *fMRIPrep* 1.4.1 (Esteban et al., 2019; RRID:SCR_016216), which is based on *Nipype* 1.2.0 (Gorgolewski et al., 2011; RRID:SCR_002502).

Anatomical Data Preprocessing

For anatomical preprocessing, the T1-weighted (T1w) image from each subject was corrected for intensity non-uniformity (INU) with N4BiasFieldCorrection (Tustison et al., 2010), distributed with ANTs 2.2.0 (Avants et al., 2008; RRID:SCR_004757), and used as T1w-reference throughout the workflow. The T1w-reference was then skull-stripped with a *Nipype* implementation of the antsBrainExtraction.sh workflow (from ANTs), using OASIS30ANTs as target template. Brain tissue segmentation of cerebrospinal fluid (CSF), white matter (WM), and gray matter (GM) was performed on the brain-extracted T1w using fast (FSL 5.0.9, RRID:SCR_002823, Zhang et al., 2001).

Anatomical Normalization

Volume-based spatial normalization to one standard space (MNI152NLin2009cAsym) was performed through non-linear registration with antsRegistration (ANTs 2.2.0), using brain-extracted versions of both T1w reference and the T1w template. The following template was selected for spatial normalization: *ICBM 152 Non-linear Asymmetrical template version 2009c* (Fonov et al., 2009, RRID:SCR_008796; TemplateFlow ID: MNI152NLin2009cAsym).

Anatomical Volumetric Calculations

For each subject, the normalized volume of the striate was calculated using the results generated by Freesurfer's segmentation. Specifically, the volume of the nucleus accumbens, the caudate, and the putamen (bilaterally) were added together and the result was divided by the total intracranial volume. This was used for our volumetric analyses in which we correlated the normalized striatal volume with subjects' VAS-F, criterion, and *d*' scores.

Functional Data Preprocessing

For functional data preprocessing, the following preprocessing was performed on each of the eight BOLD runs of fMRI data per subject (i.e., four runs of each task). First, a reference volume and its skull-stripped version were generated using a custom methodology of *fMRIPrep*. The BOLD reference was then co-registered to the T1w reference using flirt (FSL 5.0.9, Jenkinson and Smith, 2001) with the boundary-based registration (Greve and Fischl, 2009) cost-function.

Co-registration

Co-registration was configured with nine degrees of freedom to account for distortions remaining in the BOLD reference volume. Head-motion parameters with respect to the BOLD reference (transformation matrices and six corresponding rotation and translation parameters) were estimated before any spatiotemporal filtering using mcflirt (FSL 5.0.9, Jenkinson et al., 2002). BOLD runs were slice-time corrected using 3dTshift from AFNI 20160207 (Cox and Hyde, 1997, RRID:SCR_005927).

Resampling

The BOLD time-series (including slice-timing correction) were resampled onto their original, native space by applying a single, composite transform to correct for head-motion and susceptibility distortions. These resampled BOLD time-series will be referred to as *preprocessed BOLD in original space*, or just *preprocessed BOLD*. The BOLD time-series were resampled into standard space, generating a *preprocessed BOLD run in MNI space (using the "MNI152NLin2009cAsym" template)*.

Confounding Variables

Several confounding time-series were calculated based on the *preprocessed BOLD*: framewise displacement (FD), DVARS, and three region-wise global signals. FD and DVARS are calculated for each functional run, both using their implementations in *Nipype* (following the definitions by Power et al., 2014). The three global signals were extracted within the CSF, the WM, and the whole-brain masks. Additionally, a set of physiological regressors was extracted to allow for component-based noise correction (*CompCor*, Behzadi et al., 2007). Principal components were estimated after high-pass filtering the *preprocessed BOLD* time-series (using a discrete cosine filter with 128 s cut-off) for the two *CompCor* variants: temporal (tCompCor) and anatomical (aCompCor). tCompCor components were then calculated from the top 5% variable voxels within a mask covering the subcortical regions. This subcortical mask was obtained by heavily eroding the brain mask, which ensured that it did not include cortical GM regions. For aCompCor, components were calculated within the intersection of the aforementioned mask and the union of CSF and WM masks calculated in T1w space, after their projection to the native space of each functional run (using the inverse BOLD-to-T1w transformation). Components were also calculated separately within the WM and CSF masks. For each CompCor decomposition, the *k* components with the largest singular values were retained, such that the retained components' time series were sufficient to explain 50% of variance across the nuisance mask (CSF, WM, combined, or temporal). The remaining components were dropped from consideration. The head-motion estimates calculated in the correction step were also placed within the corresponding confound file. The confound time series derived from head-motion estimates and global signals were expanded with the inclusion of temporal derivatives and quadratic terms for each (Satterthwaite et al., 2013). Frames that exceeded a threshold of 0.5 mm FD or 1.5 standardized DVARS were annotated as motion outliers. The CompCor components, motion parameters, and FD values were included in the deconvolution as regressors of no interest.

Interpolation

All resamplings were performed with *a single interpolation step* by composing all the pertinent transformations (i.e., head-motion transform matrices, susceptibility distortion correction when available, and co-registrations to anatomical and output spaces). Gridded (volumetric) resamplings were performed using antsApplyTransforms (ANTs), configured with Lanczos interpolation to minimize the smoothing effects of other kernels (Lanczos, 1964). Non-gridded (surface) resamplings were performed using mri_vol2surf (FreeSurfer).

Deconvolution

The resulting data were then deconvolved. In the deconvolution, signal drift was modeled with a set of basis functions; the motion parameters were used as regressors of no interest, and TRs with motion exceeding 1.7 mm (half a voxel, in native space) were excluded from analysis [resulting in the exclusion of an average of 3.8 TRs (2.8%) per subject and an average of 0.5 TRs (0.4%) across the dataset]. The CompCor components and FD values were also included as regressors of no interest. The regressors of interest were the correct trials of each block. Each block was deconvolved separately, and the coefficient of fit of the correct trials was entered into the group-level analysis.

Group-Level Analyses

Because correlations were found between *d*' and VAS-F, criterion and VAS-F, and between *d*' and criterion (formal analysis described below), three group-level analyses were conducted: one for VAS-F, one for *d*', and one for criterion. In all cases, an LME was used (3dLME from the AFNI suite of processing tools) with the factors of task (0-back vs. 2-back) and run (runs 1–4 of each task) and with subject included as a random factor. For the analysis of fatigue, the VAS-F scores were included as a quantitative variable. For the analysis of perceptual sensitivity, the *d*' scores were included as a quantitative variable. For the analysis of bias, the criterion scores (β) were included as a quantitative variable.

The results of these whole-brain analyses were corrected for multiple comparisons by using an individual voxel probability threshold of $p < 0.001$ and a cluster threshold of 13 voxels (voxel dimension = 3 × 3 × 3 mm). Monte Carlo simulations, using 3dClustSim (version AFNI_17.2.16, compile date: Sept 19, 2017), showed this combination to result in a corrected alpha level of $p < 0.05$. Furthermore, because we were specifically interested in the striatum, we also calculated the cluster threshold necessary to correct for multiple comparisons in an area restricted to the nucleus accumbens, the caudate nucleus, and the putamen, based on the anatomical location of these structures. This calculation showed that with an individual voxel probability threshold of $p < 0.001$ and a cluster threshold of three voxels, the corrected alpha level would be $p < 0.05$.

RESULTS

RT and Accuracy

For RT, the main effects of task and run were significant with no evidence for an interaction. The main effect of task [$F(1, 186.5) = 29.10$, $p < 0.0001$] was due to subjects responding with longer latencies for the 2-back task (771 ms) than for the 0-back task (615 ms). The main effect of run [$F(3, 180.8) = 2.97$, $p < 0.05$] was due to subjects responding with progressively longer latencies during the first three runs and then faster latencies on the fourth run: 667, 703, 715, and 687 ms for runs 1–4, respectively. Importantly, there was neither an effect of VAS-F nor did VAS-F interact with any of the factors.

For the accuracy data, the main effect of task was significant [$F(1, 191.6) = 15.64$, $p < 0.0001$]. This resulted from greater accuracy on the 0-back task (93.9%) than on the 2-back task (88.8%). No other effects or interactions were significant: as with the analysis of the RT data, there was neither an effect of VAS-F nor did VAS-F interact with any of the factors.

SDT Measures

Preliminary Analysis

We first tested the independence of *d*' and criterion by analyzing *d*' as a function of task, criterion, and run using an LME. There was a strong negative relationship between *d*' and criterion [$F(1, 131) = 192.39$, $p < 0.0001$], showing that *d*' and criterion were not independent (see **Supplementary Figure 4**). The coefficient was −1.69, indicating that as subjects' perceptual certainty (*d*') increased, they became less conservative in their response bias. Additionally, to ensure that our tasks induced fatigue, we analyzed the VAS-F scores as a function of task and run (also using an LME). The only significant effect in this analysis was that of run [$F(3, 177.12) = 4.51$, $p < 0.005$]. This resulted from subjects reporting increasingly more fatigue across the four runs of the task (runs 1–4: 22.9, 24.0, 27.3, and 27.9, respectively).

Analysis of Criterion

For the analysis of criterion (response bias), there was a main effect of task [$F(1, 174.5) = 11.56$, $p < 0.001$), which was due to a higher criterion (conservative bias) during the 0-back task (0.70) than during the 2-back task (0.60). There was also a significant relationship between criterion and VAS-F [$F(1, 169.6) = 4.55$, $p < 0.05$]. As **Figure 1** shows, this was a positive correlation [coefficient (or slope of the linear relationship) = 0.08]: the more fatigue subjects reported, the higher their criterion (i.e., the more conservative their response bias).

Analysis of Sensitivity

The analysis of sensitivity (*d*') showed a main effect of task [$F(1, 175.3) = 200.97$, $p < 0.001$], which was due to higher perceptual certainty (sensitivity) on the 0-back task (3.12) than on the 2-back task (2.22). The main effect of VAS-F was also significant [$F(1, 147.9) = 3.86$, $p = 0.05$]. As **Figure 2** shows, this was due to a

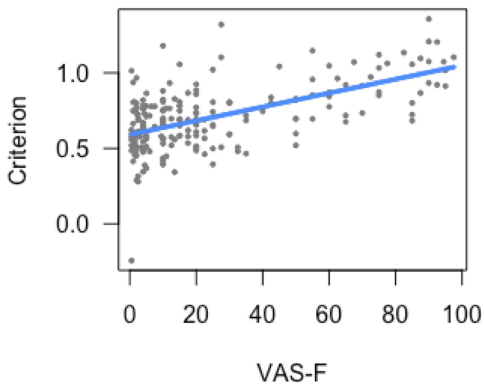

FIGURE 1 | Bias (response criterion) as a function of cognitive fatigue (VAS-F). As cognitive fatigue increased, subjects increased their response criterion. For ease of interpretation, the "raw," un-transformed VAS-F scores are shown in the plot. VAS-F, visual analog scale of fatigue.

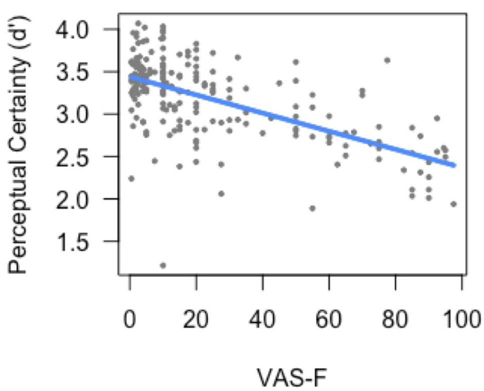

FIGURE 2 | Perceptual certainty (*d'*) as a function of cognitive fatigue (VAS-F). As cognitive fatigue increased, subjects' perceptual certainty decreased. For ease of interpretation, the "raw," un-transformed VAS-F scores are shown in the plot. VAS-F, visual analog scale of fatigue.

negative correlation between perceptual certainty and cognitive fatigue scores (coefficient = −0.19): as subjects became more fatigued, their perceptual certainty decreased. No other effects or interactions were significant.

Structural Neuroimaging Results

We performed three volumetric analyses: we correlated striatal volume with 1) VAS-F, 2) *d'*, and 3) criterion. In all cases, the volumetric data was correlated with the average of the fatigue and SDT measures, which were averaged across task and run (using only those runs where fatigue was reported). To correct for multiple comparisons, we used the Bonferroni approach, in which family-wise errors are corrected by requiring that the *p*-values are less than 0.05/3 (0.017). The correlation between striatal volume and *d'* was significant ($r = 0.51$, $p < 0.005$), as was the correlation between striatal volume and criterion ($r = -0.52$, $p < 0.005$). However, the correlation between striatal volume and VAS-F was not significant ($r = -0.27$, $p = 0.13$). Because the caudate nucleus has been associated with cognitive fatigue in previous work (Chaudhuri and Behan, 2004; Wylie et al., 2017a), we performed two exploratory analyses in which the volumes of the left and right caudate were correlated with VAS-F. The correlation between VAS-F and the left caudate was not significant ($r = -0.28$, $p = 0.12$), but the correlation between VAS-F and the right caudate did reach conventional levels of significance ($r = -0.36$, $p < 0.05$).

Functional Neuroimaging Results

In the behavioral analyses above, we found a significant relationship between *d'* and criterion, as well as a significant relationship between VAS-F and both *d'* and criterion. Therefore, for the analyses of the neuroimaging data, we performed separate analyses for VAS-F, *d'*, and criterion.

Fatigue (VAS-F) Effects

Brain activation correlated with the VAS-F in the caudate of the basal ganglia and the superior frontal gyrus (see **Table 2** and **Figure 3**). **Figure 3** shows the negative relationship between the BOLD signal and VAS-F in the caudate (coefficient = −0.047). Furthermore, there was an interaction between task and VAS-F in several frontal areas including the superior frontal gyrus, the insula, and the inferior frontal gyrus (see **Table 2**). **Figure 4** shows the interaction in the insula, which resulted from a negative relationship between the BOLD signal and VAS-F for the 0-back task (coefficient = −0.015) and a positive relationship for the 2-back task (coefficient = 0.050). This pattern was also shown in the superior and inferior frontal gyri.

Criterion Effects

The BOLD signal correlated with criterion in the caudate and putamen of the basal ganglia (see **Table 3** and **Figure 3**). **Figure 3** shows the negative relationship between the BOLD signal and criterion (coefficient = −0.059) in the caudate. There were also interactions between task and criterion in frontal areas [superior

TABLE 2 | Fatigue (VAS-F) effects.

Condition/Location	BA	X	Y	Z	Voxels	F statistic
VAS-F						
Basal ganglia						
Caudate	–	−6.6	9.0	14.0	3	11.38
Frontal						
Superior medial gyrus	*10*	−0.1	67.4	10.0	16	17.64
Task × VAS-F						
Frontal						
Superior frontal gyrus	*10*	−27.2	64.0	10.0	20	18.94
Insula	*45*	−34.1	26.1	6.0	28	23.13
Inferior frontal gyrus	*11*	41.5	36.5	−10.0	21	17.05

The brain areas associated with the main effect of VAS-F (top) and with the interaction of task and VAS-F (bottom). BA, Brodmann's area; X Y Z, the location of the voxel with peak intensity in each cluster; Vox, the number of voxels in the region of overlap.

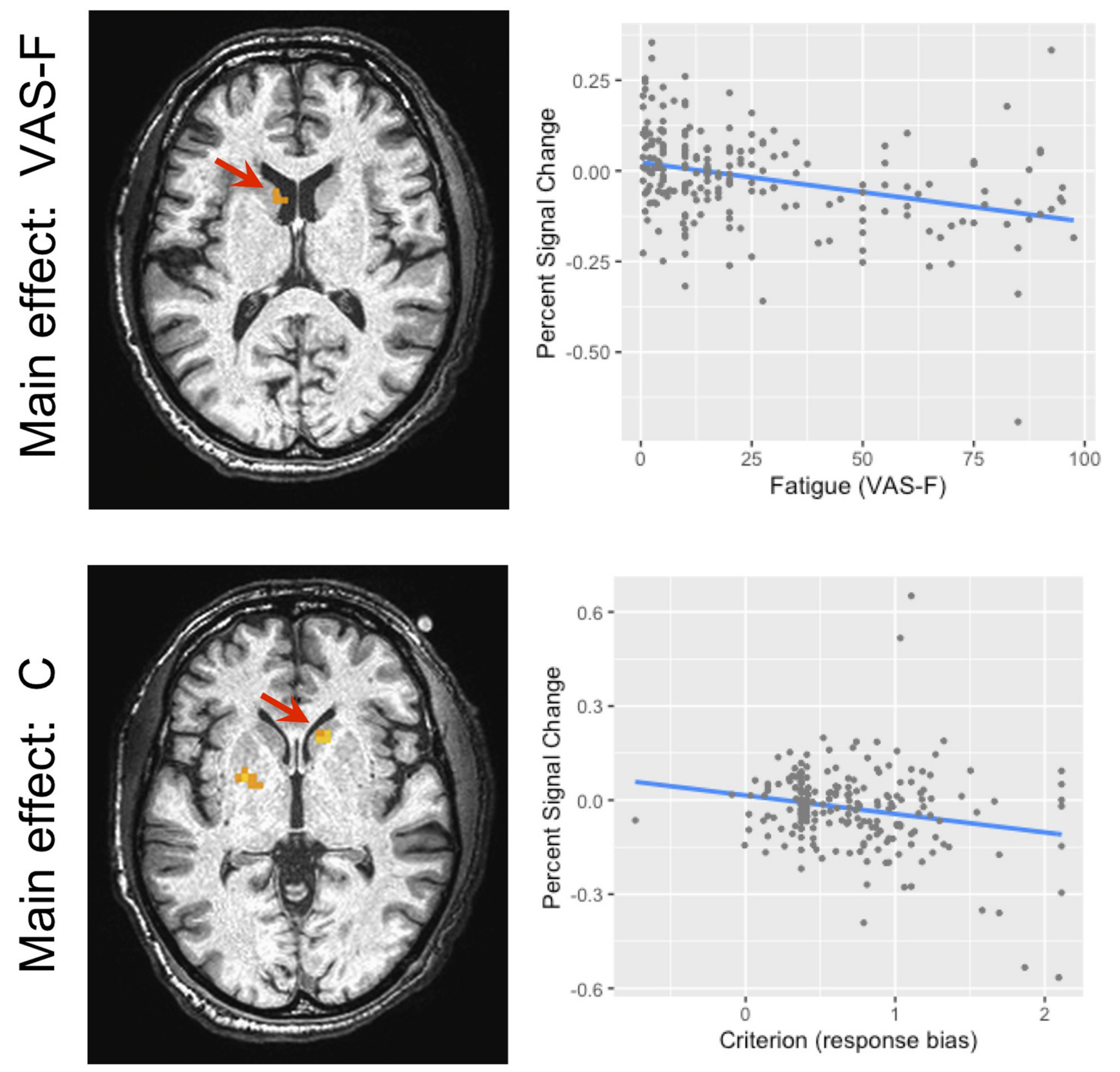

FIGURE 3 | The main effect of fatigue (VAS-F) and criterion in the caudate nucleus of the basal ganglia. Because this was within our region of interest, the cluster level threshold was $k \geq 3$ voxels. The location of the local maxima of activation is shown in the panels on the left. For the main effect of VAS-F, the location was −7, 9, 14 (*X Y Z*); for the main effect of criterion, the location was 14, 19, −2 (*X Y Z*). The panels on the right show the relationship between brain activation and VAS-F (top) and criterion (bottom). For ease of interpretation, the "raw," un-transformed VAS-F scores are shown in the plot. VAS-F, visual analog scale of fatigue.

orbital and superior frontal gyri, supplementary motor area (SMA), and precentral gyrus], and parietal areas (superior and inferior parietal lobules) (see **Table 3**). The interaction in the SMA is shown in **Figure 5**, where the relationship between the BOLD signal and criterion was weakly positive for the 0-back task (coefficient = 0.011), but is strongly negative for the 2-back task (coefficient = −0.157). A similar pattern was shown in the other areas where an interaction was found. For example, **Figure 5** shows a similar pattern in the superior parietal lobule: the relationship between the BOLD signal and criterion was positive for the 0-back (coefficient = 0.056) and strongly negative for the 2-back (coefficient = −0.108).

d' Effects

There were no areas where there was a main effect of perceptual certainty (*d'*) on the BOLD signal. However, there were interactions between task and perceptual certainty (*d'*) in the putamen of the basal ganglia, frontal areas (SMA

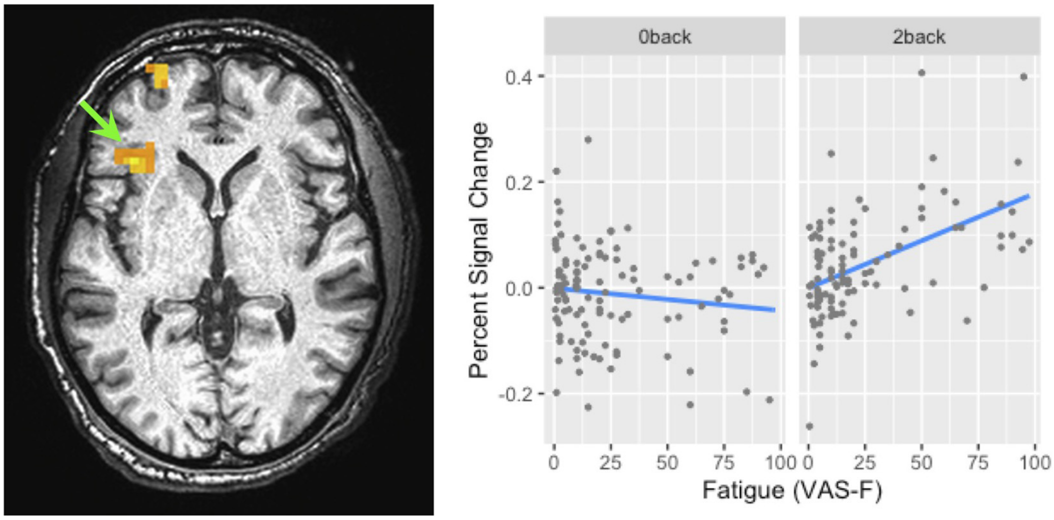

FIGURE 4 | The interaction of task and fatigue (VAS-F) in the insula. The location of this interaction is shown by the green arrow (local maxima *X Y Z*: -34, 26, 6). Because this was not within our region of interest, the cluster level threshold was $k \geq 13$ voxels. The pattern of the interaction was similar in the superior frontal/orbital gyrus (the location of which can be seen in the left panel). For ease of interpretation, the "raw," un-transformed VAS-F scores are shown in the plot. VAS-F, visual analog scale of fatigue.

TABLE 3 | Criterion effects.

Condition/location	BA	*X*	*Y*	*Z*	Voxels	*F* statistic
Criterion						
Basal ganglia						
Caudate nucleus	–	14.0	19.3	−2.0	6	14.88
Putamen/thalamus	–	−16.9	−8.2	−6.0	14	14.43
Task × criterion						
Frontal						
Superior orbital/frontal gyrus	*10*	24.3	64.0	2.0	16	17.15
Superior frontal gyrus	*6*	−23.8	−1.4	46.0	53	21.09
SMA	*6*	−6.6	2.1	62.0	45	19.90
Precentral gyrus	*6*	−51.3	2.1	50.0	17	18.38
Precentral gyrus	*6*	24.3	−4.8	46.0	14	15.27
Parietal						
Superior parietal lobule	*7*	−20.4	−63.2	50.0	39	19.87
Inferior parietal lobule	*7*	−34.1	−52.9	54.0	37	19.80

The brain areas associated with the main effect of criterion (top) and with the interaction of task and criterion (bottom). BA, Brodmann's area; X Y Z, the location of the voxel with peak intensity in each cluster; Vox, the number of voxels in the region of overlap.

and precentral gyrus), and in parietal areas (superior and inferior parietal lobule) (see **Table 4**). As **Figure 5** shows, the relationship between the BOLD signal and *d*' in the SMA was weakly negative for the 0-back task (coefficient = −0.009), but markedly positive for the 2-back task (coefficient = 0.084). This was also the case in the superior parietal lobule (see **Figure 5**): the relationship between the BOLD signal and *d*' was weakly negative for the 0-back task (coefficient = −0.028) and more strongly positive for the 2-back task (coefficient = 0.064).

DISCUSSION

Previous work has indicated that the two central metrics of SDT—perceptual certainty and criterion—may be related to cognitive fatigue. Perceptual certainty has been shown to decrease after subjects complete a fatiguing task (Matthews and Desmond, 2002), and changes in fatigue have been linked to changes in the effort–reward payoff matrix (Dobryakova et al., 2015; Müller and Apps, 2018). Here, we assessed whether changes in cognitive fatigue correlated with changes in both perceptual certainty and criterion and also how these measures changed as a function of changes in brain activation. Behaviorally, changes in subjects' VAS-F scores were not correlated with RT or accuracy (see **Supplementary Figures 2**, **3**) but were correlated with both criterion and *d*', supporting the idea that SDT metrics can be used to better understand subjective cognitive fatigue. The fMRI data also support this idea, inasmuch as activation in the striatum was associated with VAS-F, criterion, and *d*'. Together, these data not only show that these metrics are related but also provide some insight into why they are related.

In the behavioral data, there was a positive relationship between cognitive fatigue and response bias (criterion), such that as subjects reported more fatigue, their response bias became more conservative. When we investigated the areas of the brain that were responsive to cognitive fatigue and to response bias, the striatum was involved in both, though the areas responsive to each did not overlap. Furthermore, the pattern of activation in the striatum associated with cognitive fatigue was comparable to the pattern associated with response bias (see **Figure 3**). Taken together, these results offer support for the idea that cognitive fatigue is related to response bias.

Additionally, we found that cognitive fatigue was negatively related to perceptual certainty (*d*'). That is, as subjects reported

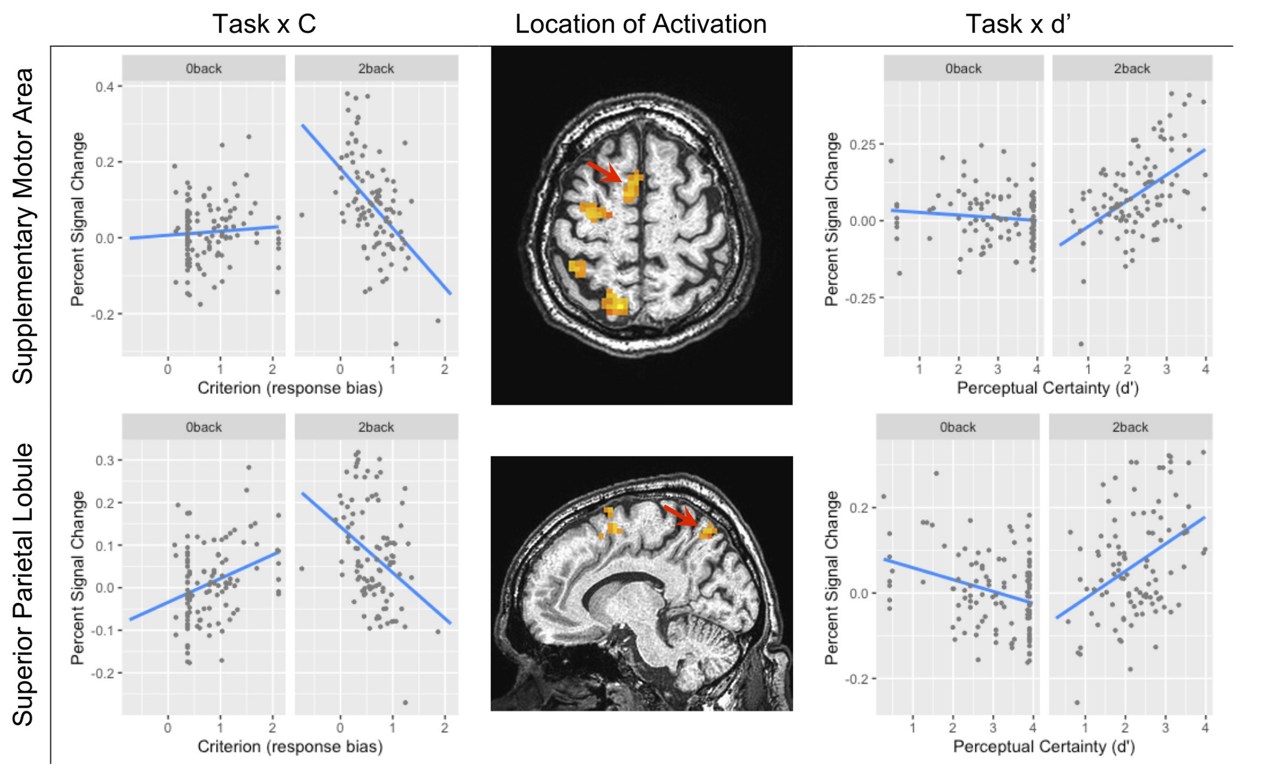

FIGURE 5 | The task × criterion interaction (left column) and task × *d*' interaction (right column) in the supplementary motor area (SMA) (top row local maxima; *X Y Z*: −7, 2, 62) and in the superior parietal lobule (bottom row local maxima; *X Y Z*: −20, 63, 50). In both rows, the location of the interaction is shown by the red arrow. Because this was not within our region of interest, the cluster level threshold was $k \geq 13$ voxels.

TABLE 4 | Perceptual certainty (*d*') effects.

Condition/Location	BA	*X*	*Y*	*Z*	Voxels	*F* statistic
Task × *d*'						
Basal ganglia						
Putamen	–	−20.4	−1.4	10.0	6	14.10
Frontal						
SMA	*6*	−6.6	5.5	74.0	37	22.56
Precentral gyrus	*6*	−30.7	−11.7	58.0	30	21.85
Precentral gyrus	*6*	−41.0	−4.8	38.0	13	16.38
Pre/postcentral gyrus	*6*	−58.2	−1.4	22.0	17	21.95
Parietal						
Superior parietal lobule	*7*	−20.4	−63.2	50.0	71	25.29
Inferior parietal lobule	*40*	−44.4	−39.2	46.0	33	20.58

The brain areas associated with the main effect of d' (top) and with the interaction of task and d' (bottom). BA, Brodmann's area; X Y Z, the location of the voxel with peak intensity in each cluster; Vox, the number of voxels in the region of overlap.

more cognitive fatigue, their perceptual certainty declined. This conforms to everyday experience—when we are fatigued, we feel "less sharp" and less confident in our assessment of our surroundings—and is also consistent with previous findings (Matthews and Desmond, 2002). However, this current result is the first time that changes in cognitive fatigue have been shown to be correlated with changes in *d*'. Furthermore, we also found that the volume of the striatum was related to SDT measures during working memory processing. The relationship was negative for criterion and positive for *d*', meaning that individuals with greater striatal volume showed a more liberal response criterion and higher perceptual certainty, whereas individuals with a smaller striatum showed a more conservative response criterion and lower perceptual certainty. As in the fMRI results, the directionality of relationship between striatal volume and VAS-F was the same as that between striatal volume and criterion. For the VAS-F, this relationship was not significant when the entire striatal volume was considered. This relationship was significant when only the caudate nucleus was investigated (albeit, only on the right) as motivated by prior research (e.g., Dobryakova et al., 2015, 2018; Wylie et al., 2017a)—further supporting the importance of the caudate nucleus in the experience of cognitive fatigue. Taken together, the volumetric results accord well with the results of the functional neuroimaging data and suggest that cognitive fatigue is related not only to the activation in the caudate of the basal ganglia but also to the volume of the caudate.

More broadly, these findings support our hypothesis that changes in VAS-F would be related to changes in response bias and extend our prediction toward a fuller definition of cognitive fatigue: one of the signatures of cognitive fatigue appears to be a more conservative response bias and lower perceptual certainty. This is seen in the behavioral data and in the relationships between the behavioral data and the BOLD signal. These results also help to explain why simple performance measures such as accuracy often fail to correlate with fatigue: fatigue affects not only the subjects' ability to distinguish targets from non-targets (*d*') but also their response bias (criterion). Thus, while subjects' inability to distinguish targets from non-targets does cause errors, they appear to compensate for this by requiring more evidence before releasing their responses. If one calculates accuracy by averaging across all types of error, this distinction is lost and fatigue-related changes in performance are not evident (a result replicated here in the analysis of the accuracy data, see **Supplementary Figure 3**). By using SDT on the behavioral data, we are better able to understand the types of performance decrements associated with fatigue; by investigating the associated changes in brain activation, we are able to better understand the mechanisms underlying these changes.

While changes in brain activation in the striatum were associated with both cognitive fatigue and with response bias, the manipulation of task difficulty showed differences in the brain areas associated with cognitive fatigue and SDT measures. For example, in a replication of previous work, we found activation in the insula to be associated with cognitive fatigue (Wylie et al., 2017a; Müller and Apps, 2019). As **Figure 4** shows, fatigue-related activation in the insula showed a strong positive relationship to brain activation during the difficult 2-back task and a weaker negative relationship during the easier 0-back task. Finding fatigue-related activation in the insula is consistent with the role of the insula in processing internal states such as fatigue (Müller and Apps, 2019); finding a different relationship between fatigue reported during the two tasks and activation in the insula may suggest that the fatigue experienced during the tasks was qualitatively different. For example, the fatigue experienced during the 0-back task may have been more closely related to boredom (Milyavskaya et al., 2019), whereas the fatigue experience during the 2-back task may have been more closely related to a decrease in the resources necessary to perform the task.

For criterion and *d*', the manipulation of task difficulty was related to brain areas more closely related to attention and response selection: superior parietal lobule (SPL) and SMA. As **Figure 5** shows, both of these areas showed a stronger relationship between SDT metrics and activation during the 2-back than during the 0-back. Furthermore, the relationship between brain activation and criterion and *d*' were reciprocal. That is, as activation in the SPL and SMA increased during the 2-back, subjects showed increased perceptual certainty and adopted a more liberal response bias. This was not the case during the 0-back task, which is likely due to the fact that the 0-back task is sufficiently easy that relatively small changes in brain activation had little effect on perceptual sensitivity and response bias.

Limitations and Future Directions

While we did support our hypotheses, our results are nevertheless currently limited by having been demonstrated using only the *n*-back task. It will be important to show that comparable results are found using different tasks. Furthermore, these results should be replicated in a larger sample. While our sample is relatively large, it is still difficult to generalize to the entire population based on approximately 40 healthy individuals. Moreover, having a larger sample would potentially allow us to tease apart the separate effects of VAS-F, criterion, and *d*' (which were correlated with one another in this sample) through stratifying the sample or performing mediation analyses. Going forward, it will be valuable to determine if these new metrics of cognitive fatigue are sufficiently sensitive to distinguish cognitive fatigue in neurotypical individuals from clinical populations that are particularly affected by fatigue (e.g., individuals with MS or TBI). Additionally, while we favor an interpretation of these data in terms of effort and reward, it is important to point out that reward was not explicitly manipulated in this experiment. The tasks likely differed in their reward value (e.g., the 2-back task was far more difficult than the 0-back task, and good performance on the 2-back was therefore likely to have been more implicitly rewarding than good performance on the 0-back task), but future work should manipulate reward explicitly to test this interpretation more directly.

CONCLUSION

The results presented here show that cognitive fatigue is related to changes in subjects' response bias (payoff matrix) and perceptual certainty. Not only are self-report metrics (VAS-F) related to these SDT metrics but also the striatum is sensitive to all three. These results may suggest that as cognitive fatigue increases, subjects make more errors because their perceptual sensitivity declines and they compensate for this by adopting a more conservative response bias. The mechanisms underlying these changes include brain areas associated with effort and reward (the striatum), attentional processes (fronto-parietal areas), and areas related to response conflict (SMA).

DATA AVAILABILITY STATEMENT

The data analyzed in this study is subject to the following licenses/restrictions: Kessler Foundation reserves the rights to this dataset. Requests to access these datasets should be directed to GW, gwylie@kesslerfoundation.org.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by the Kessler Foundation Institutional Review Board. The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

GW and JD conceived of the study concept. GW oversaw data collection, performed the analyses, and drafted the manuscript.

BY, JS, and JD provided critical revisions. All authors approved the final version of the manuscript for submission.

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SUPPLEMENTARY MATERIAL

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fpsyg.2020.579188/full#supplementary-material

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Stimulation in the Rat Anterior Insula and Anterior Cingulate During an Effortful Weightlifting Task

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When performing tasks, animals must continually assess how much effort is being expended, and gage this against ever-changing physiological states. As effort costs mount, persisting in the task may be unwise. The anterior cingulate cortex (ACC) and the anterior insular cortex are implicated in this process of cost-benefit decision-making, yet their precise contributions toward driving effortful persistence are not well understood. Here we investigated whether electrical stimulation of the ACC or insular cortex would alter effortful persistence in a novel weightlifting task (WLT). In the WLT an animal is challenged to pull a rope 30 cm to trigger food reward dispensing. To make the action increasingly effortful, 45 g of weight is progressively added to the rope after every 10 successful pulls. The animal can quit the task at any point – with the rope weight at the time of quitting taken as the "break weight." Ten male Sprague-Dawley rats were implanted with stimulating electrodes in either the ACC [cingulate cortex area 1 (Cg1) in rodent] or anterior insula and then assessed in the WLT during stimulation. Low-frequency (10 Hz), high-frequency (130 Hz), and sham stimulations were performed. We predicted that low-frequency stimulation (LFS) of Cg1 in particular would increase persistence in the WLT. Contrary to our predictions, LFS of Cg1 resulted in shorter session duration, lower break weights, and fewer attempts on the break weight. High-frequency stimulation of Cg1 led to an increase in time spent off-task. LFS of the anterior insula was associated with a marginal increase in attempts on the break weight. Taken together our data suggest that stimulation of the rodent Cg1 during an effortful task alters certain aspects of effortful behavior, while insula stimulation has little effect.

Keywords: effort, insula, cingulate cortex, persistence, rat, weightlifting

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INTRODUCTION

The ability to appropriately persevere or abandon effortful tasks is essential for optimal function (Hull, 1943). Persisting through effort is often needed to achieve highly valued rewards, but organisms must also know when to quit behaviors or tasks that are no longer optimal based on external and/or internal signals (Stephens and Krebs, 1986; Hockey, 2013). Behavioral disruptions in either direction are observed in certain human pathologies, including attention deficit hyperactivity disorder, obsessive-compulsive disorder, and depressive disorders (American Psychiatric Association, 2013; Chong et al., 2016; Pessiglione et al., 2018).

Neural activity during the acute decision phase of selecting a high-effort, high-reward course of action has been studied in multiple species, including humans (Croxson et al., 2009; Engstrom et al., 2014; Arulpragasam et al., 2018), laboratory rats (Bardgett et al., 2009; Ostrander et al., 2011;

Cowen et al., 2012), macaque monkeys (San-Galli et al., 2018), and marmosets (Enomoto et al., 2018). However, there are far fewer studies examining neural activity *after* the initial decision phase, i.e., what drives an animal to continue to persist in (or quit) an effortful task once the task has been initiated? Neurocognitive frameworks of fatigue suggest that extended effort expenditure recruits functional connectivity between the anterior cingulate cortex (ACC), the anterior insula, and the lateral prefrontal cortex (Muller and Apps, 2019). It is not known though whether modifying activity in any of these regions can alter persistence (or quitting) behaviors in a given task.

The ACC [cingulate cortex area 1 (Cg1) in rodent] is a region with known involvement in motivated behavior. Electrical stimulation of the human ACC at 50 Hz evokes subjective reports of motivation to accomplish goals and surpass challenges (Parvizi et al., 2013). Similarly, ablation of ACC in humans is sufficient to reduce some of the cognitive symptoms of obsessive-compulsive disorder (Sheth et al., 2013). In laboratory rats, neurons in Cg1 encode effort-outcome values (Hillman and Bilkey, 2010, 2012; Cowen et al., 2012) and manipulations of this region affect an animal's preference for high-effort, high-reward courses of action (Walton et al., 2003; Schweimer and Hauber, 2005, 2006; Rudebeck et al., 2006). Others report, however, that Cg1 activity might mediate some types of effortful action, but not all (Holec et al., 2014). This inconsistency regarding the precise role of the ACC/Cg1 in effort-laden motivated behavior is not surprising given the wide range of phenomena and functions ascribed to the ACC/Cg1, including autonomic regulation, fear and anxiety, nociception, and attention (Medford and Critchley, 2010). ACC/Cg1's involvement in diverse functions suggests it is a major node in high-order cognitive control circuitry, including that required for complex, dynamic decision making (Heilbronner and Hayden, 2016; Kolling et al., 2016; Wang et al., 2018).

Like the ACC, the anterior insula [broadly homologous to the agranular insular (AI) in rodent] is implicated in a wide range of phenomena, including aggression, fear, interoception, frustration, and food- and drug-seeking behaviors (Craig, 2009). Of note, the AI is involved in mediating behavioral responses to changes in cost-benefit parameters: both lesioning and GABAergic inhibition of AI in rodents promotes the pursuit of higher food outcomes in tasks with reward devaluation schedules (Balleine and Dickinson, 2000; Parkes et al., 2015). These findings have found resonance in similar experiments with cocaine (Moschak et al., 2018) and nicotine (Pushparaj et al., 2013). In human experiments when feelings of frustration are induced by blocking participants' progression in a task, there is coincident activation of a network that includes the anterior insula (Yu et al., 2014). In another human experiment, self-reported feelings of satisfaction after curiosity-inducing tasks are associated with insular activity (Lee and Reeve, 2017). Taken together, these results suggest that the anterior insula plays a central role in controlling internal motivational states which may influence persistence or quitting behaviors.

The ACC and anterior insula have been proposed to functionally interact as a Salience Network (Medford and Critchley, 2010; Menon and Uddin, 2010). This network – which can facilitate network shifts between the Default Mode Network and Central Executive Network – helps an animal appropriately respond to salient cues, whether those cues stem from challenges and changes in environment, expectations, preferences, and/or internal signals (Medford and Critchley, 2010; Scholl et al., 2015). All of these cue types dynamically change during an effort-laden task, suggesting that Salience Network node activity may be critical in driving – or dissuading – persistence in the task at hand.

Here we used a laboratory rat model to investigate whether electrical stimulations of ACC/Cg1 or AI change an animal's persistence in an effortful weightlifting task (WLT). We tested a low (10 Hz) and a high (130 Hz) frequency as behavioral effects of stimulation are often influenced by frequency (Mohan et al., 2020). For example in kindled rats, seizure activity can be precipitated with 10 Hz hippocampal stimulation but suppressed with130 Hz (e.g., Wyckhuys et al., 2010). We predicted that 10 Hz Cg1 stimulation would increase persistence in the WLT given that Cg1 activity in rodent is linked to high-effort, high-reward choice behavior, and that mid-frequency (50 Hz) pre-operative ACC stimulation in humans has been associated with a "will to persevere" (Parvizi et al., 2013). We predicted that 130 Hz AI stimulation would reduce task engagement and persistence, given that a previous study linked 130 Hz insular stimulation to reduced nicotine self-administration in a progressive ratio operant task (Pushparaj et al., 2013).

MATERIALS AND METHODS

Animals

Ten male Sprague-Dawley rats (n = 10) were sourced from the University of Otago's Hercus Taieri Resource Unit (Dunedin, New Zealand) and housed in 38 × 30 × 35 cm clear plexiglass, individually ventilated cages (Tecniplast, Italy). At the beginning of experiments, rats were approximately 7 months of age with an average body weight of 439 g (±5.8 g). At the end of experiments, rats were approximately 10 months of age with an average body weight of 436 g (±5.1 g). Animals were paired in cages but kept separate by a clear, perforated barrier so auditory and olfactory interaction could happen, but no direct physical contact could occur. All animals were kept on a 12 h reverse dark-light cycle, with all experimental procedures being conducted during the animal's dark phase. The rats were kept on a restricted diet of standard rat chow (Teklad diet, Envigo, United States) to limit their body weight and promote interest in food reward; all rats were maintained at ≥85% of their free-feeding body weight. Water was available *ad libtum*. All procedures were approved by the Animal Ethics Committee at the University of Otago, protocol 91/17.

Weightlifting Task

Before surgery, animals were trained in the WLT, a novel effort expenditure task that was recently developed and validated by our lab. For a detailed description of the WLT apparatus, materials, and full training procedures, see Porter and Hillman (2019); videos of the task in action are included in the supplementary material of that publication. In brief, the WLT consists of a 120 cm × 90 cm × 60 cm wooden open arena, painted black.

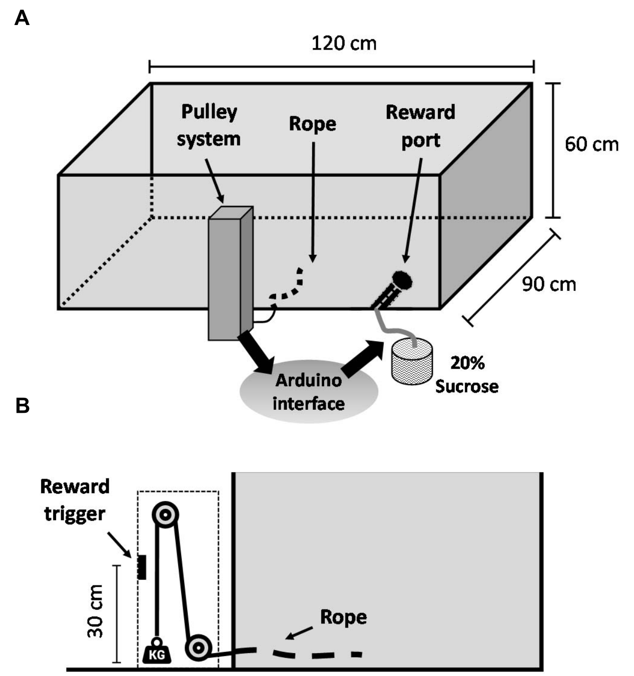

FIGURE 1 | Weightlifting task schematic. **(A)** General overview of the apparatus. **(B)** Profile view of the weighted pulley system that allows for effort manipulations within the task. Video footage of the apparatus and of the task in action is included in the supplementary material of Porter and Hillman (2019).

Inside the arena, two conduit pipes extend from the wall: one conduit contains a rope, which is connected outside the arena to a vertical pulley system; the other conduit contains a silicone tube, which is connected outside the arena to a peristaltic pump containing 20% sucrose liquid (**Figure 1A**). The animal must pull the rope 30 cm to trigger automated dispensing of 0.2 ml sucrose reward. The pulley system enables different weights to be added to the rope, ranging 45–225 g in 45 g increments, thus increasing the difficulty of rope pulling within the arena (**Figure 1B**). Animals were initially trained on a rope containing no weight ("0 g") and once proficient were challenged with 45 g. Training was considered complete once an animal was able to perform 10 successful pulls of 0 g, immediately followed by 10 successful pulls of 45 g, all within 5 min. The WLT is automated via an Arduino microcontroller, which is configured to send TTL signals to a nearby acquisition system (Digital Lynx SX; Neuralynx Inc.) for timestamping of all task events.

Surgery

Once trained in the WLT, animals were prepared for surgical implantation of electrodes. Rats were placed in an induction chamber and given 5% isoflurane in oxygen mixture (EZ-7000, EZ Anesthesia, United States). Once voluntary movement ceased, the animals were removed from the chamber and placed in a stereotaxic frame (Stoelting, United States) equipped with non-traumatic ear bars and a nose cone for anesthetic maintenance at 2–4%. The animals received subcutaneous doses of amphoprim (trimethoprim and sulphadimethyl pyrimidine, 30 mg/kg), atropine (0.065 mg/kg), and buprenorphine (0.05 mg/kg). The scalp was infused with subcutaneous bupivacaine (4 mg/kg). A single incision was made over the scalp and the skull exposed, bregma and lambda were located and the two landmarks were leveled via nose bar adjustment. Craniotomies were drilled for electrode implantations and for the placement of structural screws. Coordinates for the implants, taken from bregma, were: AP +3.7, ML +0.4, DV −1.0 for Cg1; and AP +2.7, ML +2.0, DV −5.8 for the AI, with the stereotaxic arm angled 20° toward the right from midline. Coordinates were based on the rat atlas of Paxinos and Watson (2007); all implants were right hemisphere. Half the rats received stimulating electrodes in the Cg1, and the other half received stimulating electrodes in the AI. Each animal was also implanted with recording electrodes for local field potential (LFP) capture; animals with stimulating electrodes in Cg1 had ipsilateral recording electrodes in AI, and vice versa. LFP data are not presented in this report. The electrodes were connected to gold pins inside a McIntyre plug which was cemented to skull screws.

Once surgery was finished, animals were administered a dose of carprofen (5 mg/kg, s.c.), ear bars were removed, and isoflurane was reduced to 0%. After 10 min of breathing oxygen mixture through the nose cone, animals were transferred to a clean, sterile cage and were monitored for 6 h. Animals were then returned to their home cage and closely monitored for 7 days; during this time there was free access to food and water. Animals were returned their pre-surgical food restriction regimen on post-surgical day eight.

Electrodes and Stimulation

Both stimulating and recording electrodes were made of twisted and PFA-insulated stainless steel wires (diameter 0.005″ bare, 0.008″ coated, SDR Scientific, Australia). The stimulating wires were twisted together, with each tip separated by 0.5 mm; this spacing was selected to limit current spread to a minimum (Bagshaw and Evans, 1976; Tehovnik, 1996). Recording electrodes were arrays constituted of three wires twisted together. The free ends of each electrode were soldered to gold pins and inserted into a McIntyre connector (Molino and McIntyre, 1972).

Electrical stimulation was generated by an isolated, constant current stimulator (Model 4100, A-M Systems, United States). Stimulation trains consisted of biphasic square pulses (100 ms pulse width, 75 μA amplitude); these parameters are based on previous ACC and AI stimulation experiments (Pushparaj et al., 2013; Lim et al., 2015) and preliminary trials in our laboratory. Three frequencies were used separately in different trials: sham stimulation (0 Hz), low frequency stimulation (LFS, 10 Hz), and high frequency stimulation (HFS, 130 Hz), which were delivered uninterrupted throughout the testing session. Stimulation sessions were performed with at least 24 h interval between each session. While in the testing apparatuses, animal movement was tracked via an overhead camera (CV-S3200, JAI, United States) and headstage mounted LEDs.

Experimental Design

After surgery and recovery, animals were returned to the WLT apparatus for testing, initially to confirm that they recalled the task and there were no post-surgical impairments in performance. The initial cohort of animals ($n = 4$; two with Cg1 stimulation electrodes and two with AI stimulation electrodes) was run through a counterbalanced A-B-C block design, with blocks of sham, LFS, or HFS. Each block contained 3 days of testing, one session per day. A second cohort ($n = 6$; three with Cg1 stimulation electrodes and three with AI stimulation electrodes) was run through a counterbalanced A-B-C-C-B-A block design, with blocks of sham, LFS, or HFS. Each block contained 3 days of testing, one session per day.

For each session, the animal was placed in the WLT arena (with no rope initially present), stimulation was initiated, and the animal was given 2 min of arena exploration. After this baseline period, the 0g rope was fed into the arena and the animals began the WLT. After 10 successful trials (i.e., pulling the rope 30 cm to trigger reward dispensing) on the 0 g rope, a 45 g weight was attached to the rope. The task continued in this progressive manner – where 45 g was added every 10 successful trials – until the animal quit the task or reached the max weight of 225 g. A quit was defined as a 60 s period of being off-task, i.e., no rope pull attempts and no sucrose consumption within 60 s. The weight on which the animal quit the task was deemed the "break weight." When a quit was determined, the experimenter retracted the rope from the arena and the animal was given a final 2 min of arena exploration. At the end of this 2 min period, two doses of sucrose reward were manually dispensed by the experimenter in order to ascertain if animals quit the task due to satiation. The animal was then returned to its home cage. The floor of the arena was cleaned with a disinfectant liquid (Tego 2001, Hugh Crane, United Kingdom; 1% solution) in between sessions, and illumination of the experimental room was kept to a minimum.

To determine if stimulation of either brain region affected general locomotor behavior, animals also completed a series of open field recording sessions. These sessions were completed in the weeks following completion of the WLT. The circular open field apparatus was 70 cm in diameter, 55 cm high and made of black flexible plastic. The apparatus was placed in the center of the WLT arena, allowing the same recording equipment and the same room parameters (inter-session cleaning, illumination) to be used. Each session was 5 min in duration, one session per day. Animals were given one initial day of habituation (no stimulation), and then stimulation sessions were carried out using the same block format as was used in the WLT, with counterbalanced blocks of sham, LFS, and HFS.

Perfusion and Histology

After all experiments were completed, rats were euthanized with isoflurane, and transcardially perfused with saline (0.9%) followed by paraformaldehyde solution (4%) and formalin-sucrose solution (30%). Brains were removed and stored in formalin-sucrose for at least 48 h to allow fixation and to prepare for frozen sectioning. Sectioning was conducted in a microtome-cryostat (Leica CM1860 UV, Germany) at 80 μm thickness, and slices were mounted on clear glass slides. Sections were then stained with thionin and digitally captured, and the locations of the recording and stimulating tips were established according to the rat brain atlas of Paxinos and Watson (2007). All animals had stimulating tips confirmed to be within the borders of the target region (**Figure 2**).

Data Analysis

Initial data analyses were carried out using Matlab R2018a and custom Matlab scripts. Video tracking and TTL signals exported from the acquisition system were used to calculate behavioral metrics such as trial duration, attempts-to-success ratio, and time-on-task; all metrics have been detailed previously (Porter and Hillman, 2019; Porter et al., 2020). Collated data were then exported to GraphPad Prism 8.4.3 for statistical analyses and graphing.

To determine if behavioral metrics from the overall session differed between stimulation conditions, normality was first assessed by Shapiro–Wilk. Parametric (one-way ANOVA) or non-parametric (Kruskal–Wallis) tests were then used accordingly to compare conditions, with *post hoc* Dunn's comparisons made for each stimulation condition versus sham. To determine if intra-session metrics (trial duration and attempts-to-success) differed between stimulation conditions, two-way ANOVA tests were used with factors of Stimulation Condition and Pulling Weight, and *post hoc* Dunnett's. Asterisks are used throughout to denote significant differences as follows: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Since the WLT is designed to allow an animal to quit at any point, and most animals quit before reaching the highest weights of 180 and 225 g, there was a scarcity of trial data at these higher weights. For this reason, we constrained our trial-based comparative analyses to the first four weights – 0, 45, 90, and 135 g – where all ten animals routinely contributed data.

RESULTS

Effects of Stimulation on General Motor Behavior

No overt motoric effects were observed during delivery of LFS or HFS to either brain region (**Figure 3**). When tested in a circular open field, stimulation of Cg1 or AI did not affect distance traveled as compared to sham [*Cg1:* $F(2,16) = 0.51$, $p = 0.61$; *AI*: $H(2) = 0.92$, $p = 0.21$]. Speed was no different between sham, LFS and HFS conditions [*Cg1*: $F(2,14) = 0.53$, $p = 0.60$; *AI*: $F(2,8) = 0.97$, $p = 0.42$]. Likewise the different stimulation conditions did not differ in thigmotaxis [*Cg1*: $F(2,16) = 0.26$, $p = 0.77$; *AI*: $H(2) = 1.6$, $p = 0.47$] or freezing behavior [*Cg1*: $F(2,16) = 0.12$, $p = 0.89$; *AI*: $H(2) = 1.05$, $p = 0.63$].

WLT Performance

The WLT has recently been detailed and validated by our lab using male Sprague-Dawley rats (Porter and Hillman, 2019). Here we used a specific version of the WLT – the progressive WLT – to challenge the animals in terms of effortful persistence.

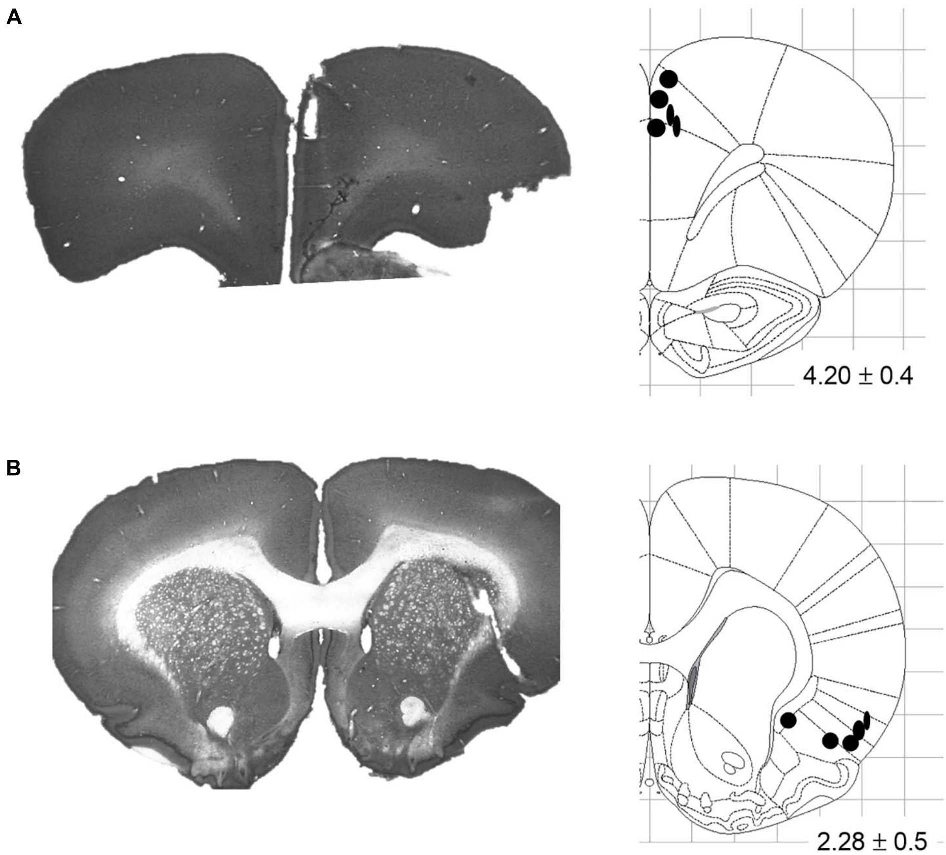

FIGURE 2 | Stimulating electrode placements. **(A)** Representative coronal slice and schematic illustrating terminal tip locations for Cg1-targeted electrodes. **(B)** Representative coronal slice and schematic illustrating terminal tip locations for AI-targeted electrodes. Schematics adapted from the Rat Brain Atlas of Paxinos and Watson (2007); distance from bregma indicated in mm ± anterior-posterior span.

Rats initially pull a 0 g rope 30 cm to trigger a sucrose reward, thereby completing one trial. After every 10 successful trials the rope is weighted with an additional 45 g. This is repeated every 10 successful trials until the animal quits, or reaches a maximum pulling weight of 225 g (see sections "Weightlifting Task" and "Experimental Design").

Under sham stimulation conditions (n = 43 sessions), animals in this study performed the WLT as expected (**Figure 4**) and behavioral metrics aligned with metrics observed in previous cohorts (Porter and Hillman, 2019; Porter et al., 2020). As the pulling weight progressively increased, the time to complete a successful pull and earn reward (trial duration) significantly increased [$H(3) = 50.2, p < 0.001$], as did the attempts-to-success ratio [$H(3) = 26.6, p < 0.001$]. The most common break weight was 135 g, occurring in 51% of sham sessions.

Effects of Cg1 Stimulation on WLT Performance

We predicted that low-frequency Cg1 stimulation would increase persistence in the progressive WLT, that is, stimulation would cause animals to work longer and harder in each task session. Session duration did differ between conditions [$H(2) = 9.7, p = 0.008$; **Figure 5A**, left], but stimulation did not result in longer WLT sessions. Rather, *post hoc* comparisons revealed that Cg1 LFS sessions were shorter in duration as compared to sham ($p = 0.04$). Within each session, percentage of time-on-task also differed between conditions [$H(2) = 19.4, p < 0.001$; **Figure 5B**, left], but stimulation did not result in more dedicated time-on-task as predicted. Rather, Cg1 HFS produced a decrease in time-on-task ($p = 0.03$).

Despite this decrease in time-on-task under HFS, HFS sessions were associated with higher break weights (**Figure 5C**, left). When break weight distributions were fitted with Gaussian curves, bidirectional differences in mean break weight were observed in Cg1 stimulation conditions [$F(2,9) = 39.9, p < 0.001$]. Under Cg1 sham conditions, the most common break weight was 135 g (occurring in 59% of Cg1 sham sessions) with a fitted mean ± SD of 121 ± 27 g. Under HFS a rightward shift was observed as compared to sham: the most common HFS break weights were 135 and 180 g (each occurring in 36% of HFS sessions) with a fitted mean of 153 ± 45 g. Under LFS, there was a leftward shift as compared to sham: the most common LFS break weight was 90 g (occurring in 50% of LFS sessions) with a fitted mean of 99 ± 32. Under Cg1 LFS the animals quit the task sooner – as indicated by

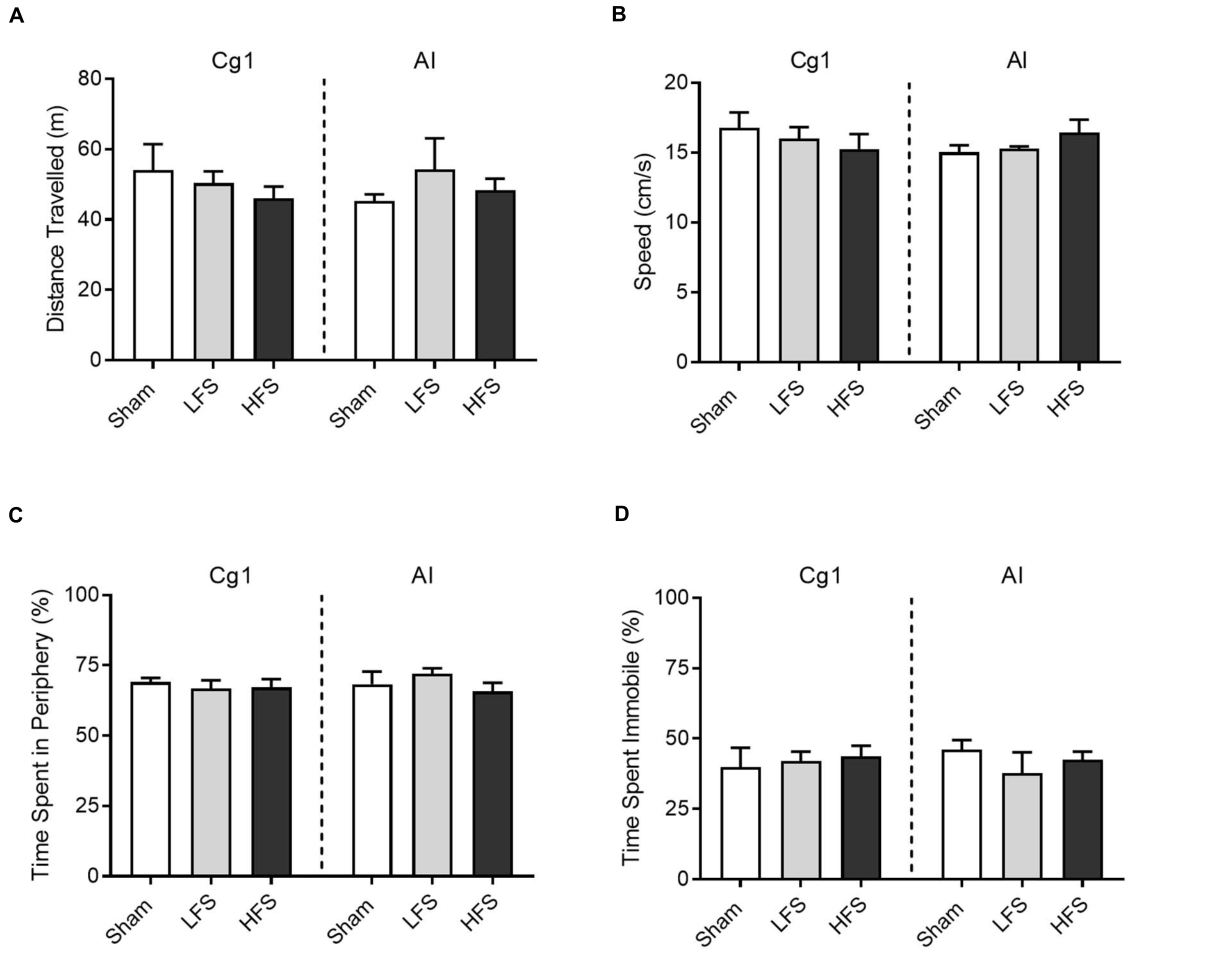

FIGURE 3 | Effects of stimulation on open field behavior. **(A)** Distance traveled. **(B)** Average speed across the 5 min session. **(C)** Time spent in the periphery of the open field, demarcated as within 20 cm of the apparatus' wall. **(D)** Time spent immobile, defined as moving speed <10 cm/s. Data are shown as mean ± SEM.

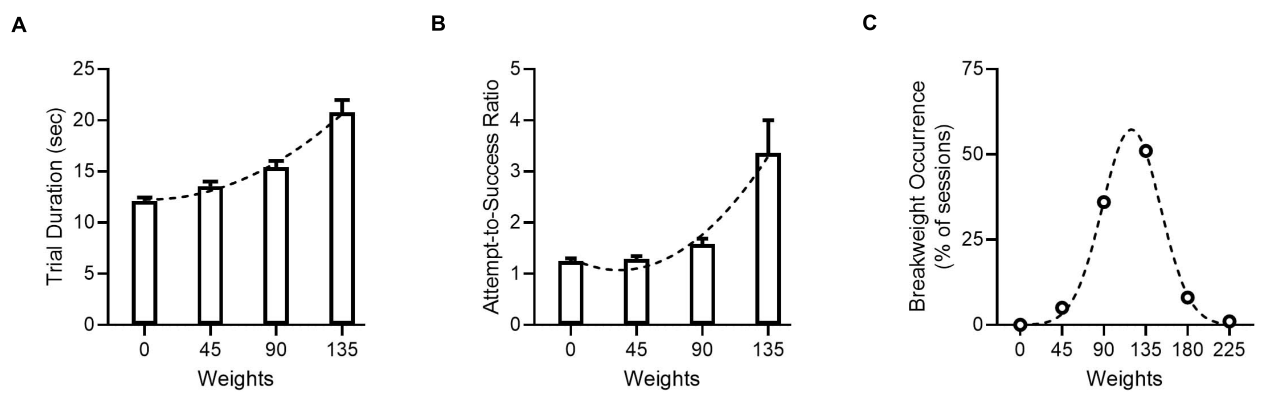

FIGURE 4 | Progressive WLT performance under sham stimulation. **(A)** Trial duration, with one trial being defined as the time from rope pull initiation to reward consumption. Dotted line denotes second order polynomial. **(B)** Attempts-to-success ratio. A ratio of one indicates that a single rope pull attempt was successful and resulted in one reward being dispensed; values >1 indicate that multiple pull attempts were needed to successfully trigger reward. Dotted line denotes second order polynomial. **(C)** Break weight distribution across sham sessions. Open circles denote observed frequencies, dotted line denotes Gaussian fit. Data for panel **(A,B)** are shown as mean ± SEM.

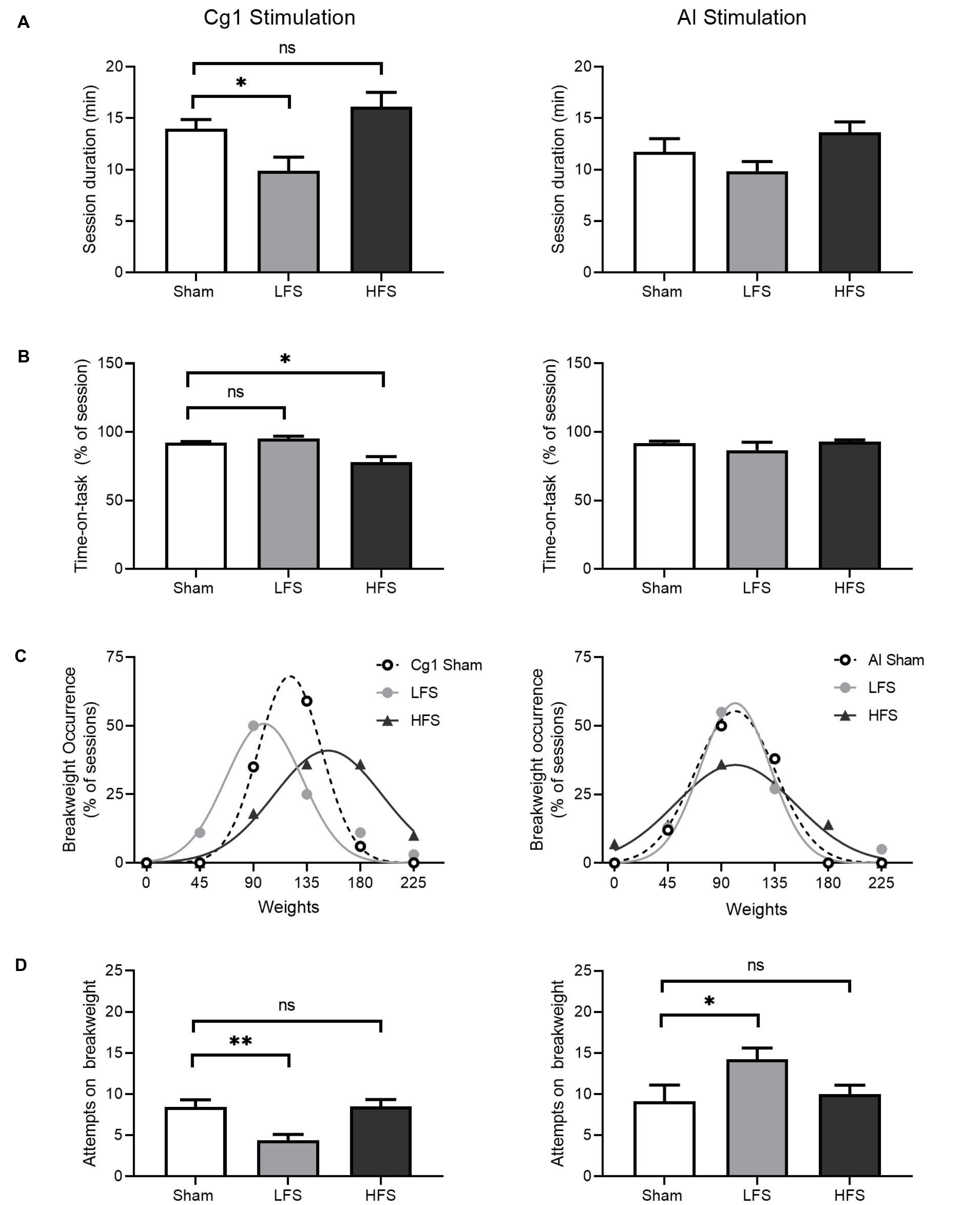

FIGURE 5 | Effects of stimulation on WLT session performance. Cg1 stimulation data are presented on the left, AI stimulation data are presented on the right. **(A)** Session duration. Animals could quit the task at any time point. **(B)** Time-on-task, calculated as the percent of session duration engaged in rope pulling and reward consumption. **(C)** Break weight distribution comparisons between stimulation conditions. Symbols denote observed frequencies, lines denote Gaussian fits. **(D)** Attempts made on the break weight prior to quitting. For all bar graphs, data are shown as mean ± SEM. Asterisks denote significant post hoc comparisons as compared to sham; *$p < 0.05$, **$p < 0.01$.

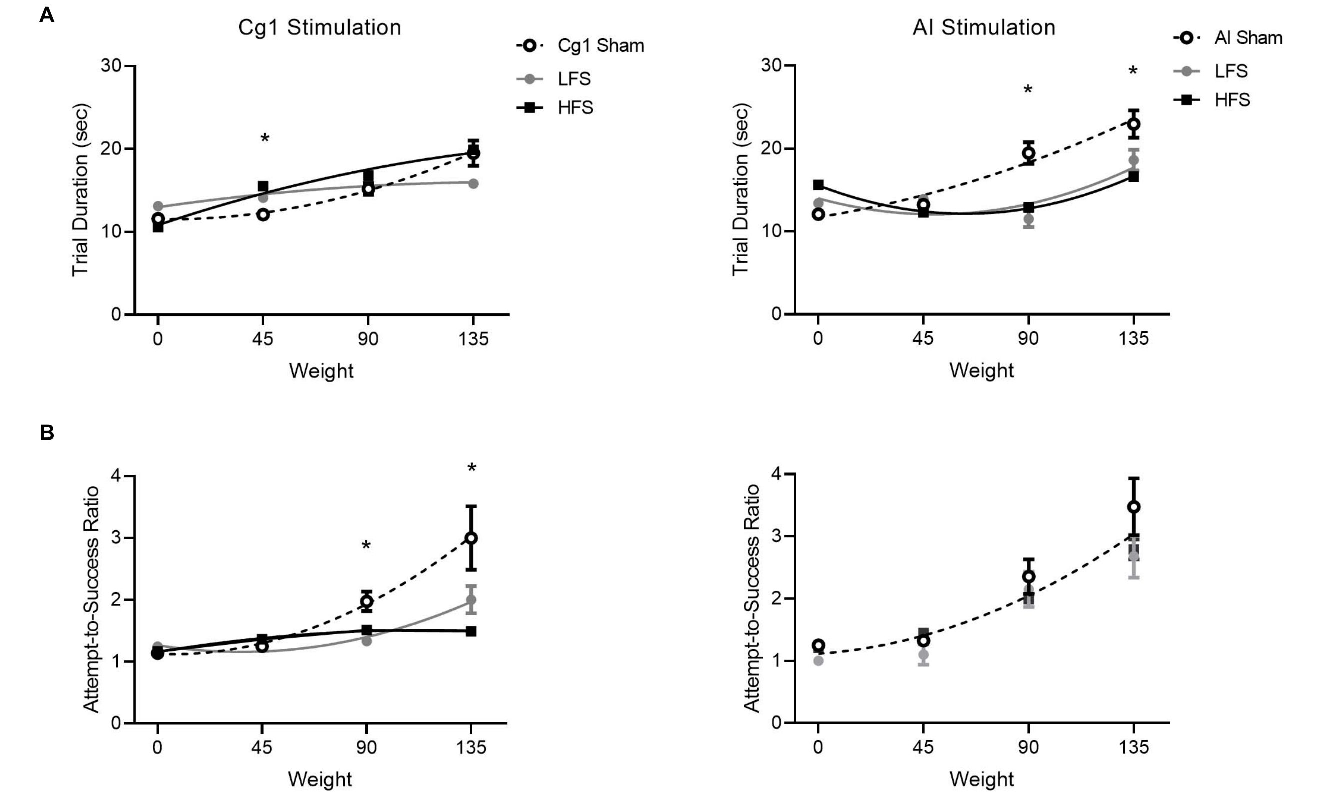

FIGURE 6 | Effect of stimulation on trial performance. Cg1 stimulation data are presented on the left, AI stimulation data are presented on the right. **(A)** Trial duration for stimulations versus sham. **(B)** Attempt-to-success ratios for stimulations versus sham. Data symbols denote mean ± SEM; lines illustrate second order polynomial best fit. For attempts-to-success ratios during insula stimulation there is only one line of best fit illustrated, as all three conditions could be fit with a shared global curve. Asterisks denote significant *post hoc* comparisons where both stimulation conditions (LFS and HFS) differed from sham.

shorter session durations and lower break weights – and the animals also made fewer attempts on the break weight before electing to quit (**Figure 5D**, left). When the break weight trials were examined in isolation to determine how many attempts were made before quitting, attempts differed between conditions [$H(2) = 12.2$, $p = 0.002$], with LFS significantly lower than sham ($p = 0.006$).

To determine if Cg1 stimulation had more subtle effects on performance within the task itself, trial duration and attempts-to-success ratio were examined across the three stimulation conditions (**Figure 6**, left). Trial duration exhibited a significant Condition × Weight interaction [$F(6,396) = 10.3$, $p < 0.001$]. The data were best represented by distinct quadratic fits [$F(6,399) = 10.3$, $p < 0.001$]. When attempts-to-success ratios were examined, a significant Condition × Weight interaction was also observed [$F(6,396) = 5.9$, $p < 0.001$]. Again the different stimulation conditions were best represented by distinct quadratic fits [$F(6,399) = 8.4$, $p < 0.001$].

Effects of AI Stimulation on WLT Performance

We predicted that AI stimulation would reduce effort investment in the WLT, that is, animals would spend more time off-task and/or quit the task sooner. When overall session durations were compared, no effect of AI stimulation was detected [$H(2) = 5.1$, $p = 0.08$; **Figure 5A**, right]. Within each session, percentage of time-on-task also did not differ between conditions [$H(2) = 0.39$, $p = 0.82$; **Figure 5B**, right]. When break weight distributions were fitted with Gaussian curves, mean break weights were no different between conditions [$F(2,9) = 3.2$, $p = 0.08$; **Figure 5C**, right]. When break weight trials were examined in isolation to determine how many attempts were made before quitting, stimulation did have a small, marginally significant effect [$F(2,42) = 3.1$, $p = 0.055$; **Figure 5D**, right]. Low-frequency stimulation of the AI was associated with an increase in the number of attempts on the break weight as compared to sham ($p = 0.046$).

To determine if AI stimulation had more subtle effects on performance within the task itself, trial duration and attempts-to-success ratio were examined across conditions (**Figure 6**, right). Trial duration exhibited a significant Condition × Weight interaction [$F(6,477) = 8.24.1$, $p < 0.001$]. The data were best represented by distinct quadratic fits [$F(6,480) = 8.6$, $p < 0.001$]. When attempts-to-success ratios were examined, there was no Condition × Weight interaction [$F(6,477) = 1.1$, $p = 0.37$]. The same curve fit could be applied to all conditions [$F(6,480) = 1.9$, $p = 0.08$].

DISCUSSION

Given that conjoint activation of the cingulate cortex and insular cortex is observed during effortful decision-making (Engstrom et al., 2014), volitional responding (Medford and Critchley, 2010), and task switching (Menon and Uddin, 2010), we hypothesized that stimulation of these areas in rat would alter performance in an effortful weightlifting task. Despite being used clinically, electrical stimulation *in vivo* is still not well understood in terms of mechanism. Stimulation can cause proximal as well as distal effects and has been linked to neuronal excitation as well as inhibition; stimulation frequency appears to be one important determinant in these differing effects (Bari et al., 2013; Mohan et al., 2020). For this reason we tested two stimulation frequencies – 10 and 130 Hz – frequencies which have been used successfully in prior rat studies (Pushparaj et al., 2013; Rea et al., 2014; Lim et al., 2015; Lindenbach et al., 2019). We initially predicted bidirectional effects based on stimulation site: that Cg1 stimulation would increase effort expenditure and persistence in the task, and that AI stimulation would decrease effort expenditure and prompt earlier quitting.

Contrary to our prediction, low-frequency Cg1 stimulation resulted in shorter task sessions: animals quit the task sooner (lower break weight) and made less attempts on the break weight before quitting. One interpretation of this is that LFS reduced motivation to perform the task, notably as it got more difficult, however performance metrics within the session indicated that there was motivated rope-pulling throughout. At 0 and 45 g LFS was associated with slower trial-by-trial performance (longer trial duration) but the attempts-to-success ratio centered around 1; the latter continued into higher weights indicating that when rats decided to initiate a pull they were generally successful. One interpretation of this is that Cg1 LFS facilitates a "slow and steady" approach, where the time to complete a single trial is slower but the efficiency of the pull is maintained (i.e., an attempt-to-success ratio $\sim$1), even at higher pulling weights. This differs from sham stimulation conditions, where there is an exponential increase in attempts-to-success ratio as the pulling weight increases. Under sham stimulation in this study, and as observed in non-stimulated animal cohorts in previous studies (Porter and Hillman, 2020; Porter et al., 2020), heavier weights result in more failed attempts – i.e., rope pulls fail to reach the 30 cm mark required to trigger reward. Cg1 LFS appears to exert a subtle change in WLT behavior: the animal experiences fewer fails, but also terminates the task earlier.

High-frequency Cg1 stimulation in our animals was associated with more time spent off-task in each session, however this did not equate to poorer overall performance in the task. Rather, HFS was again associated with a "slow and steady" pulling efficiency but also with a higher break weight. Taken together these data suggest that under Cg1 HFS animals work consistently at the task, even into higher weights, but take frequent small breaks which culminate in more time spent off-task. To our knowledge this is one of the first studies to examine rat Cg1 modulation during an effortful task. Hart et al. (2020) also recently published a study examining Cg1 modulation during an effortful task; they demonstrated that chemogenetic excitation and inhibition of the region reduced lever-pressing in a progressive ratio, choice-based task. Similar to our study, the Hart et al. (2020) findings defy simple interpretation: manipulations assumed to be opposing produced similar behavioral shifts. While puzzling, both studies demonstrate that Cg1 manipulation during a task can shift effort expenditure in subtle ways.

Electrical stimulation of the rat cingulate and deeper vmPFC has been examined in other previous studies that were framed toward investigating anxiety- and depressive-like behaviors. However, results of those studies have been mixed. For example, Lim et al. (2015) compared the effects of 10 and 100 Hz Cg1/vmPFC stimulation in naive Sprague-Dawley males as the animals performed a battery of tests. HFS reduced home cage emergence latency and increased food intake, however null effects were reported in the open field, sucrose intake test and forced swim test. Rea et al. (2014) used 130 Hz vmPFC stimulation in rats from the Flinders Sensitive Line, a genetic animal model of depression, and found that HFS improved sucrose consumption and forced swim performance. Our findings that LFS and HFS of Cg1 produce subtle but significant changes in WLT performance in naïve animals suggests that the WLT may be worth investigating in future rodent studies investigating anxiety- and depressive-like behaviors.

Low-frequency insular stimulation in our study was associated with increased attempts on the break weight and faster performance (shorter trial duration) on higher weights. Faster performance on higher weights was also observed in the HFS condition for AI. In both stimulation conditions, there was no difference in attempts-to-success ratio as compared to sham, and no difference in end break weight or time on-task as compared to sham. Taken together, one interpretation is that AI stimulation (LFS or HFS) increases the speed of the animal's trial-by-trial task performance in high-effort circumstances, but this does not equate to improved efficiency or improved performance overall (i.e., higher break weight). In our open field assessment HFS in the AI initially appeared to induce a slight increase in speed (**Figure 3**) however this was not a significant difference compared to sham ($p = 0.38$). Thus the change in speed observed in our animals appears task-specific and not a general change in locomotor activity, a finding that has also been observed from stimulation of the lateral habenula in male Wistar rats (Jakobs et al., 2019).

In a previous insular stimulation study in rat, Pushparaj et al. (2013) demonstrated that 130 Hz HFS decreased nicotine self-administration in fixed-ratio and progressive-ratio operant tasks. These behavioral results mirrored the group's earlier findings using baclofen inactivation of in the insula (Forget et al., 2010) leading the authors to suggest that HFS is producing a regional inactivation effect. In our study, however, we found no significant effect of AI HFS on progressive-ratio performance overall (i.e., break point) for sucrose reward. Likewise, a prior study using quinolinic acid lesioning of the insula also reported no effect on progressive-ratio responding for normal food pellets (Daniel et al., 2017). Thus it is difficult to interpret just how insula manipulation alters effort expenditure – whether it affects motivation, effort exertion, or more generally the decision-making framework.

Lesioning of the rat anterior insula by various methodologies suggests it may be at the more general level of decision-making framework. Optimal choice selection in a slot machine task is reduced following GABAergic inhibition of the insula (Cocker et al., 2016); decision-making in a rodent version of the Iowa Gambling Task is altered following quinolinic lesioning of the insula (Daniel et al., 2017); and strategy shifting in response to sensory specific satiety is reduced following chemogenetic manipulation of the insula (Parkes et al., 2018). Importantly, Daniel et al. (2017) highlighted the impact of the individual rat's baseline behavioral preference pre-insular lesion, and this may be worth considering in regional stimulation studies. Daniel et al. (2017) demonstrated that insular inactivation in baseline "good decision makers" caused a shift toward less-optimal exploitation behavior, whereas insular inactivation in baseline "poor decision makers" caused a shift toward more optimal exploitation. Individual variability in gambling decisions likely explains the discrepant results that other groups reported in insular manipulation gambling studies which analyzed group means (Mizoguchi et al., 2015; Pushparaj et al., 2015). Because of the small sample size in the current study (n = 10), we were reliant on group means and could not perform robust group splits into baseline "low-effort" and "high-effort" rats, however this is an area ripe for future investigation.

Finally, for future studies of Cg1/AI stimulation, we offer two methodological considerations. In this study we utilized unilateral stimulation; it would be interesting to repeat the WLT using bilateral stimulation to determine if the subtle effects we observed here become more prominent with bilateral stimulation. Indeed many of the studies discussed above utilized bilateral stimulation (Pushparaj et al., 2013; Rea et al., 2014; Lim et al., 2015; Lindenbach et al., 2019). In this study we also utilized continuous stimulation for the duration of the task session, similar to the studies discussed above. One innovative stimulation approach has recently been detailed by Lindenbach et al. (2019). Their study examined 20 Hz stimulation of the ventral subiculum during a progressive-ratio task, but rather than continuous stimulation the researchers applied stimulation only after the first fail (± stimulation at the start of the session). This idea of using stimulation to "boost" performance after a failed attempt lends itself well to the WLT and combined with bilateral stimulation could provide interesting results.

DATA AVAILABILITY STATEMENT

The raw data supporting the conclusions of this article will be made available by the authors, without undue reservation.

ETHICS STATEMENT

The animal study was reviewed and approved by the University of Otago Animal Ethics Committee, AUP 91/17.

AUTHOR CONTRIBUTIONS

KLH and BSP conceived the study. CS ran the experiments. BSP wrote analysis routines. All authors participated in data analysis and manuscript preparation.

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A Primer on the Role of Boredom in Self-Controlled Sports and Exercise Behavior

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Self-control is critical for successful participation and performance in sports and therefore has attracted considerable research interest. Yet, knowledge about self-control remains surprisingly incomplete and inconsistent. Here, we draw attention to boredom as an experience that likely plays an important role in sports and exercise (e.g., exercise can be perceived as boring but can also be used to alleviate boredom). Specifically, we argue that studying boredom in the context of sports and exercise will also advance our understanding of self-control as a reward-based choice. We demonstrate this by discussing evidence for links between self-control and boredom and by highlighting the role boredom plays for guiding goal-directed behavior. As such, boredom is likely to interact with self-control in affecting sports performance and exercise participation. We close by highlighting several promising routes for integrating self-control and boredom research in the context of sports performance and exercise behavior.

Keywords: boredom, self-control, effort, reward-based choice, exercise, sports, decision making

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INTRODUCTION

Although self-control is a heavily researched psychological concept, an inconsistent body of literature limits the understanding of self-control's role in orienting goal-directed behavior (e.g., sports and exercise). Recently, it has been argued that boredom might be an overlooked confound in self-control research that has contributed to some of the inconsistencies in this research area. Since exercise can be perceived as boring, but can also be used to alleviate boredom, boredom is likely to play an important role in the context of sports and exercise. However, sports psychological research has rarely turned to boredom as an important factor to examine. This perspective aims to address this gap and to explicate how boredom and self-control are expected to interact in orienting goal-directed behavior. As self-control has received substantial research interest already, we will only briefly explicate the current understanding of self-control as a reward-based choice and devote more space to boredom. Specifically, we will highlight why boredom might have acted as an overlooked confound in self-control research, and how it might have affected self-controlled sports and exercise behavior (directly and via its interaction with self-control). We close by highlighting several promising routes for integrating self-control and boredom research in the context of sports performance and exercise behavior.

Self-Control in Sports and Exercise

> The man who can drive himself further once the effort gets painful is the man who will win.

– Sir Roger Bannister (the first human to run a mile in under 4 min)

The importance of self-control in the context of sports and exercise is widely acknowledged (Englert and Taylor, 2021). A plethora of studies have highlighted the relevance of self-control for regular exercise (Hagger et al., 2010b; de Ridder et al., 2012) and for performing well in a sporting task (Giboin and Wolff, 2019; Brown et al., 2020). The importance of self-control—and its resulting popularity as a topic for research in sports and exercise psychology—is intuitively appealing. After all, self-control is defined as "the efforts people exert to stimulate desirable responses and inhibit undesirable responses" (de Ridder et al., 2012, p. 77) and the notion of physical and/or mental effort is inherent to sports and exercise. In addition, self-control is often also referred to as *willpower* (Ainslie, 2020), a quality that is held in exceptionally high regard in sports (as exemplified in the above quote from Sir Roger Bannister).

It is important to note that applying self-control seems to carry an intrinsic cost (Kool and Botvinick, 2013; Ackerman et al., 2020) whose origin is still subject to debate (Shenhav et al., 2017). Attesting to this costliness, the application of self-control is tightly coupled with the sensation of effort that people normally try to avoid (Shenhav et al., 2017). Accordingly, a large body of research indicates that the application of self-control in one task will lead to impaired performance in a subsequent self-control demanding task (Hagger et al., 2010a; Giboin and Wolff, 2019; Brown et al., 2020). For example, after having applied self-control to complete a challenging work assignment, one might struggle to muster the self-control needed to go out for a late run in the dark. Current theorizing conceptualizes self-control application as a reward-based choice (Kurzban et al., 2013; Shenhav et al., 2013, 2017; Wolff and Martarelli, 2020) and from this perspective, the self-control costs of going out for a late run can outweigh the run's prospective benefits for the aspiring exerciser. Crucially, this theorizing implies that not mustering the self-control that would be required to engage (or continue) with a self-control demanding activity may not necessarily reflect a failure of self-control but might simply reflect an adaptive reward-based choice to switch from exploitation to exploration behavior (Bieleke and Wolff, in press). In line with this idea, empirical and theoretical work indicates that incurred self-control costs make people less willing to apply further self-control (Wolff and Martarelli, 2020), particularly if goal progress is not obvious to them (Osgood, 2018).

In experimental sports psychology research, the effect of prior self-control exertion on subsequent sports performance is typically investigated with a sequential two-task paradigm (Englert, 2016). Here, an experimental group performs a high self-control task (HCT), and a control group performs a low self-control task (LCT) after which both groups perform a self-control demanding sporting task (e.g., dart throwing, sprint starts, or isometric strength endurance). An example of a frequently used HCT in self-control research is the incongruent Stroop task (Wolff et al., 2018). Attesting to its self-control demands, the incongruent Stroop is generally associated with higher error rates and longer response latencies than its congruent counterpart (which is frequently used as the LCT in self-control research) and is perceived as more self-control demanding (Wolff et al., 2019). If the prior application of self-control indeed reduces the willingness to invest further effort (Wolff and Martarelli, 2020), then participants should perform worse in the sporting task after an HCT than after an LCT.

Indeed, recent meta-analytic evidence provides support for this hypothesis (Giboin and Wolff, 2019; Brown et al., 2020). However, the magnitude of performance impairment is not robustly linked to the duration of the prior self-control task (Giboin and Wolff, 2019), which conflicts with the theoretical proposition that the magnitude of the performance decrement should scale linearly with the duration of the prior self-control task (Hagger et al., 2010a). In addition, a recent bias-sensitive meta-analysis of the literature suggests that initial estimates of the effects of prior self-control on subsequent sports performance might be smaller than initially assumed (Holgado et al., 2020). This inconsistent body of literature limits our understanding of the relationship between self-control and sports performance.

Boredom: A Possible Confound

Outside the sporting context, self-control research with the sequential two-task paradigm has yielded similarly heterogeneous findings (Wolff et al., 2018). In fact, null findings (Wolff et al., 2019), evidence for publication bias (Carter and McCullough, 2014), and a large file-drawer of unpublished studies (Wolff et al., 2018) in this field have prompted researchers to conclude that it is still unclear if prior self-control exertion robustly impairs subsequent self-control performance (Friese et al., 2017).

Recently, it has been suggested that boredom might have contributed to these inconsistencies by acting as a confound of self-control effects on performance (Wolff and Martarelli, 2020). Indeed, it is plausible that LCTs and HCTs not only differ with respect to the self-control demands they impose but also in regard to how boring they are perceived to be (Milyavskaya et al., 2019). It seems particularly likely that some LCTs are systematically more boring than their HCT counterparts because they are designed to place minimal demands on self-control (and indeed, cognitive processes), thereby rendering them prototypical boredom inductions (Wolff and Martarelli, 2020). Accordingly, one recent study has shown that a more self-control demanding modified version of the Stroop task was perceived as less boring than a traditional Stroop (Bieleke et al., 2020a). Further attesting to the importance of boredom in self-control research, another study showed that if an HCT created feelings of boredom, performance on a subsequent self-control task was impaired (Osgood, 2015). Conversely, a very recent study has shown that if a primary LCT was perceived as boring, no performance differences between the LCT and HCT conditions could be observed in a subsequent isometric endurance task (Mangin et al., under review). The potential for boredom in LCT is made even more plausible by the fact that tasks that are typically used in self-control research are also frequently used in boredom research as experimental inductions of boredom (Wolff and Martarelli, 2020).

Boredom might also play a role in explaining the absence of a robust correlation between prior self-control task duration and any subsequent drop in performance. Neither boredom nor task-imposed self-control demands should be treated as static experiences (Mills and Christoff, 2018; Wolff and Martarelli, 2020). A task that was initially very demanding in terms of self-control might become progressively easier to perform due to learning or practice effects. To illustrate, while a tennis serve might be a very complex task for a beginner, professional tennis players have practiced them ad nauseam, making the movement execution second nature to them. Thus, an HCT can turn into an LCT as a function of practice. In line with this, one recent high-powered study showed that performance improved over time on an incongruent Stroop task, the longer participants worked on it (Wolff et al., 2019). Analogous arguments can be made with regard to boredom, which is best understood as a highly dynamic feeling state (Mills and Christoff, 2018) that varies greatly as a function of the task characteristics (e.g., different task demands; Bieleke et al., 2020a). Thus, neglecting the temporal dynamics of boredom and self-control demands might contribute to the inconsistent link between prior task duration and subsequent performance decrement (Wolff and Martarelli, 2020).

Exercise Can Be Boring

> Marathon running is a terrible experience: monotonous, heavy, and exhausting.
>
> – Veikko Karvonen (Olympic medalist at the 1956 marathon)

Crucially, boredom is not only a potential confound in research on self-control demanding laboratory tasks. Besides being inherently linked to effort, engaging in sports and exercise can also be plain boring (as exemplified by the quote from Veiko Karvonen above). For instance, boredom has recently been identified as a frequent obstacle during aerobic endurance exercises (Hirsch et al., 2020). On the other hand, sports and exercise can also be used to alleviate boredom (Morris et al., 2003). Thus, boredom clearly plays a multifaceted role in the sports and exercise context. Indeed, the relevance of boredom in sports had already been acknowledged as early as 1926, when the monotony of regular athletic training was introduced as an analogy of industrialized work (Davies, 1926). However, despite an abundance of lay intuition on the detrimental effects boredom has on sports performance and exercise behavior (Orenstein, 2012), research that has specifically assessed boredom in sports and exercise remains scarce. One recent notable exception showed that even professional athletes frequently struggle with boredom, with detrimental consequences for performance (Velasco and Jorda, 2020). In the exercise domain, boredom proneness has been associated with less self-reported vigorous exercise behavior (Wolff et al., 2020a).

One very likely reason for this research gap is that research from sports and exercise has primarily focused on the self-control demands of completing effortful and difficult tasks, like performing a sprint start (Englert et al., 2015), persisting for as long as possible in an endurance task (Boat et al., 2020), or adhering to an exercise regimen (Englert and Rummel, 2016). This focus makes intuitive sense, since self-control is per definition linked to the notion of effort (de Ridder et al., 2012). However, this also neglects self-control demands that do not fit the prototypical mold of high demand and high effort tasks (e.g., practicing basketball free throw technique ad infinitum). In the second part of this perspective, we will explicate that boredom is one such demand and highlight how it is intrinsically related to self-control and how it uniquely affects behavior.

ARGUMENTS FOR BOREDOM AS AN IMPORTANT FACTOR IN SELF-CONTROLLED SPORTS AND EXERCISE BEHAVIOR

Another reason for the scarcity of boredom research in sports is that until very recently boredom had not attracted much scientific interest in general (Mills and Christoff, 2018). This is rapidly changing, however, Recent work has advanced boredom research by providing more definitional clarity for both the state (Bench and Lench, 2013; Elpidorou, 2018; Elpidorou, 2020) and what it means to be boredom prone as a trait (Tam et al., under review). This work has clarified the conditions under which state boredom is likely to occur (Westgate and Wilson, 2018), as well as its functional relevance (Bench and Lench, 2019; Wolff and Martarelli, 2020). Finally, recent work has linked trait boredom proneness and self-control and explicated their joint role in goal-directed behavior (Wolff and Martarelli, 2020). These recent conceptual and empirical advancements provide an excellent starting point for investigating the role of boredom—at the level of both state and trait—in the context of self-controlled sports and exercise behavior.

Boredom and Self-Control Overlap by Definition

Boredom has been defined as the "aversive state that occurs when we are not able to successfully engage attention" and an "awareness of a high degree of mental effort expended in an attempt to engage with the task (Eastwood et al., 2012, p. 481)". Accordingly, boredom differs from seemingly related states (like low interest or amotivation), by being a decidedly aversive sensation where one wants to engage with something but is unable to do so (Mugon et al., 2018; Danckert and Eastwood, 2020). In line with the latter, recent research has shown that performing an easy but boring task creates sensations of fatigue that can even outweigh the fatigue that is experienced by performing a demanding self-control task (Milyavskaya et al., 2019). Thus, inherent to their respective definitions, boredom and self-control share two core features. The capacity to control attention has been identified as the most important function of self-control (Schmeichel and Baumeister, 2010) and failure to engage attention with available activities accompanies the sensation of boredom (Malkovsky et al., 2012; Hunter and Eastwood, 2018). With respect to effort, applying self-control creates the sensation of effort. Likewise, any task that leads to the feeling of boredom requires mental effort if we decide to redouble our efforts to stick with the boring task. Consequently, it has been proposed that boredom affects results of self-control research because staying engaged with a boring task constitutes a self-control demand (Milyavskaya et al., 2019; Wolff and Martarelli, 2020). Thus, although an LCT is expected to be less self-control demanding than an HCT in terms of its task-specific self-control demands, this effect might be offset if the LCT leads to more boredom than the HCT, which would unintentionally increase the self-control demands of continuing to work on the LCT. In the same vein, a slow ten-kilometer run might be self-control demanding not (only) because the runner has to regulate aversive exercise-induced bodily sensations but also because the run itself has become boring to the runner.

Boredom in Sports and Exercise

Another perspective on why boredom likely matters for sports and exercise comes from research on the conditions under which boredom occurs. Research shows that boredom can result from an incompatibility between the demands associated with a current activity and the available attentional resources (Pekrun et al., 2010; Eastwood et al., 2012; Westgate and Wilson, 2018). Critically, this attentional mismatch can occur when an activity is underchallenging (e.g., running at moderate intensities) or overchallenging (e.g., trying to dunk a basketball although one can clearly not jump high enough) (Pekrun et al., 2010). In addition, boredom can occur when an activity is perceived as being void of meaning (van Tilburg and Igou, 2012, van Tilburg and Igou, 2017; van Tilburg et al., 2013). It makes intuitive sense that perceived lack of meaning might be important in the context of exercise behavior. To illustrate, exercisers often do strength-related exercises at the gym or go running in the forest not because they genuinely like it but because they hope this will improve appearance or reduce weight (DiBartolo et al., 2007). However, the gains made by dragging oneself to the gym or the forest accumulate only very slowly, thereby making it easy to lose faith in the meaningfulness of this behavior. Beyond a lack of perceived meaning, athletes might perceive some exercises as boring because they do not yield immediate feedback and rewards. To illustrate, athletes who genuinely like the sport they engage in might not enjoy the ancillary training they have to carry out in order to do well at their main sports. In line with this, unpublished pilot data from our lab provide strong statistical evidence that team sports athletes perceive their ancillary individual training sessions to be more boring than their primary sports-specific training sessions (for further information, please see OSF | Perspective on Boredom and Self-Control in Sports). One possible reason for this could be that ancillary individual training sessions are perceived as less rewarding (e.g., in terms of enjoyment) and are less rich in relevant feedback (e.g., relevance of subjectively improved running efficiency for getting better at scoring goals in soccer) than the training sessions of their primary sports. Taken together, the conditions that are conducive to boredom are likely to occur frequently in various sports and exercise settings.

Critically, while sports and exercise are associated with heightened arousal, boredom has traditionally been associated with low arousal (Mikulas and Vodanovich, 1993). At first glance, this is at odds with the premise of this paper. However, as the examples above indicate, boredom is also likely to occur when arousal is high (e.g., when doing endurance training as a soccer player). In line with this, research shows that boredom can be considered a mixed arousal state (Merrifield and Danckert, 2014) or even a high arousal state (Danckert et al., 2018). One potential response to this mix of results regarding the physiological signature of boredom is to suggest that arousal should not be considered a key feature of the definition of boredom (Elpidorou, 2020). While this debate cannot be resolved here, it does suggest that while arousal is an important factor to consider in the proposed research program, it should not be considered integral to it.

Boredom Is a Signal to Change Behavior

Importantly, although boredom is an aversive sensation, it is still assumed to have high functional relevance (analogous to the experience of pain; Danckert and Eastwood, 2020). It has been proposed that boredom's function is to signal that one is failing to engage with an ongoing activity and/or that one could be deploying one's attention toward more rewarding activities. Thus, by tracking diminishing returns of an ongoing activity (Berlyne, 1970) and by increasing sensitivity to more rewarding alternatives (Milyavskaya et al., 2019) boredom acts as a catalyst for behavioral change (Bieleke and Wolff, in press; Martarelli and Wolff, 2020; Wolff and Martarelli, 2020). This understanding of boredom's function has been supported by recent computational (Gomez-Ramirez and Costa, 2017) and empirical work (Geana et al., 2016). This has the important implication that despite its negative connotation, the state of boredom is neither good nor bad *per se*, its aversiveness merely acts as a signal to do *something* else. What this *something* will be seems to depend on situational constraints. More specifically, recent work has shown that getting bored during a hedonically negative experience instigates a switch to hedonically positive experiences, and, interestingly, the opposite is also true: boredom during hedonically positive experiences appears to instigate a switch to hedonically negative experiences (Bench and Lench, 2019). Thus, boredom urges one to seek out an experience that is different to one's current experience. For example, a runner might get bored because of the monotony of the exercise and feels the urge to do something more exciting. Likewise, getting bored by a tiresome work assignment could also drive one to go to the gym and engage in a more energizing activity.

Self-Control and Boredom Are Tightly Associated

Boredom and the sense of effort that accompanies the application of self-control seem to affect goal-directed behavior in close tandem, but with clearly differentiable functions (**Figure 1**). As we have outlined above, applying self-control creates the

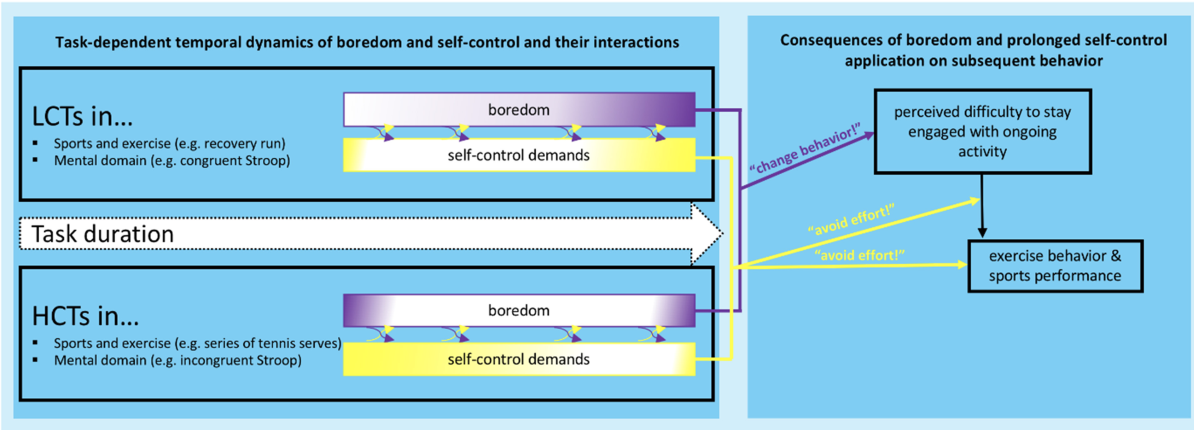

FIGURE 1 | Working model on the proposed interplay of boredom and self-control in modulating exercise behavior and sports performance (model adapted from Martarelli and Wolff, 2020; Wolff and Martarelli, 2020). The left panel visualizes the temporal dynamics of task-induced boredom and self-control demands as a function of the type of task [low control task (LCT); vs. high control task (HCT)] and the duration of the task. More specifically, the model proposes that task characteristics change as a function of task duration which in turn leads to changes in task-imposed self-control demands and task-induced boredom. The color gradients reflect the potential changes in respective signal strength as a function of time on task. For example, an LCT (e.g., recovery run) that becomes monotonous and under-stimulating over time, might become more boring over time, *and* to keep going despite being bored increases the run's self-control demands. On the other hand, HCTs can lead to the experience of boredom at the beginning due to over-stimulation (e.g., tennis serves executed by a novice) and at the end due to under-stimulation (e.g., as a result of overlearning). The left panel is a schematic representation of the assumed temporal dynamics and the arrows that connect boredom and self-control demands indicate that both signals are expected to affect each other. Importantly, the temporal dynamics of each sensation must not be linear and are assumed to depend on task characteristics and individual differences (for a comprehensive version of the model and an in-depth discussion, please see Wolff and Martarelli, 2020). The right panel visualizes the behavioral relevance of boredom and self-control exertion in affecting sports performance and exercise behavior. Boredom signals whether one should explore more rewarding alternative activities ("change behavior!") and self-control demands signal whether one should avoid investing further effort ("avoid effort!"). Thus, at any given time during the task (visualized by the arrow on the left panel), those signals are expected to vary in strength. The model proposes that boredom has a direct effect on the perceived difficulty to stay engaged with an ongoing activity, which in turn impacts ongoing exercise behavior and sports performance. Successful participation and performance in sports relies on self-control; therefore, the model proposes that self-control moderates the effect of perceived difficulty on exercise behavior and sports performance. Crucially, other variables are likely to influence the proposed relationships, such as differences in perceived meaning and trait boredom or trait self-control might moderate the velocity of gradient changes over time.

sensation of effort and empirical evidence indicates that this reduces the willingness to invest further effort (Sjåstad and Baumeister, 2018; Lin et al., 2020). Recent theorizing postulates that self-control allocation reflects the reward-based choice that the expected value of applying control outweighs the resulting self-control costs (Shenhav et al., 2013, 2017). In this conceptualization, the sensation of effort assumes the role of tracking the ongoing control costs and biasing behavior away from further application of self-control processes (Kurzban et al., 2013; Shenhav et al., 2013; Wolff and Martarelli, 2020).

Similarly, boredom's function is to instigate a change in behavior that is driven by a reduced valuation of current task value (Berlyne, 1970) and a greater sensitivity for rewards (Milyavskaya et al., 2019). Thus, boredom uniquely affects goal pursuit by instigating a change in behavior. Crucially, boredom can also make goal pursuit more self-control demanding. As a task becomes boring, signaling the urge to do something else, we must choose whether to engage a different task or increase our efforts to persist with the current one. Choosing the latter course of action (which is likely prevalent in the context of sports and exercise where athletes choose to persist on monotonous training regimes), directly contributes to rising self-control costs that, according to recent reward-based models of self-control (Kurzban et al., 2013; Shenhav et al., 2013, 2017; Wolff and Martarelli, 2020), people would normally strive to minimize. Thus, from a conceptual point of view, boredom and self-control appear to be tightly coupled in their guiding function for goal-directed behavior (Wolff and Martarelli, 2020; Bieleke and Wolff, in press).

In line with this, empirical evidence points toward a strong inverse relationship between trait self-control and boredom proneness (Mugon et al., 2018). This implies that individuals who experience the state of being bored frequently and intensely (Tam et al., under review) (e.g., in the face of repetitive gym work) should also exhibit lower levels of self-control, making it difficult for them to cope with the boredom-induced urges to disengage and instead apply the required self-control to persist with the boring task. Attesting to the existence of such a link in the exercise context, one recent study showed that high trait self-control and low boredom proneness form part of a latent personality profile that was linked to more regular exercise, whereas a profile with lower self-control and higher boredom proneness was linked with considerably lower exercise levels (Wolff et al., 2020a). Further evidence on the proposed interplay between boredom proneness and self-control comes from recent research on adherence to the social distancing guidelines amidst the ongoing COVID-19 pandemic (Bieleke et al., 2020b; Boylan et al., 2020; Wolff et al., 2020b; Martarelli et al., 2021). In line with the above propositions, boredom proneness was linked to less adherence and this effect was mediated by the perceived difficulty to comply with social distancing guidelines (Wolff et al., 2020b). In addition, high self-control was linked to better adherence and moderated the link between the perceived difficulties and adherence behavior.

The close relationship between boredom and self-control is further exemplified by both concepts' link to changes in the perception of the passage of time (Vohs and Schmeichel, 2003; Danckert and Allman, 2005). For example, while boredom proneness has been linked with the tendency to perceive time as running slowly, the opposite was found for high trait self-control (Witowska et al., 2020). On the state level, experiencing boredom and applying self-control (along with the sensation of effort this creates) has been linked to overestimating the time spent on a boring and/or self-control demanding task (Vohs and Schmeichel, 2003; Danckert and Allman, 2005). Importantly, biases in time perception might directly affect the cost-benefit analysis that underlies goal-directed behavior (Kurzban et al., 2013; Ainslie, 2020).

DISCUSSION

Up to now, we have made the case for the overlooked importance of boredom in the context of sports and exercise. In the conclusion of this paper, we outline some key implications and avenues for integrating boredom into research on self-control in sports and exercise behavior more generally.

Experimental research on the role of prior mental exertion on subsequent sports performance, as well as research on the role of self-control for exercise adherence should systematically assess state and trait boredom. As outlined above, the experience of boredom might alter sports performance in its own right (e.g., making it feel aversive) and could alter task-induced self-control demands (e.g., making it feel more demanding). Considering boredom would potentially help to understand and dissolve inconsistent research findings. With respect to the latter, boredom prone individuals might find (certain) exercises harder to adhere to. This seems more likely for some kinds of sport or activities than for others (e.g., repetitions in the gym may be more prone to ratings of boredom, whereas adventure sports may be chosen as an *escape* from boredom; Kerr and Houge Mackenzie, 2012). Likewise, it is plausible that the specific settings in which a sport or exercise is embedded in (e.g., collective vs. individual) might affect boredom and as a consequence the self-control that is needed to adhere to the activity. Finally, the feedback and reward structure of a sport or exercise (e.g., the availability and immediacy of a success like scoring a goal) is likely to influence whether it gives rise to the experience of boredom.

These propositions raise the question of how to measure boredom within the various exercise contexts. There are well-established self-report measures of domain-unspecific trait (Struk et al., 2017) and state boredom (Fahlman et al., 2013, for an overview, see Vodanovich and Watt, 2016). However, as boredom is a highly contextualized experience (Chin et al., 2017) it makes sense to assess it with reference to the specific context (Vodanovich and Watt, 2016). In the exercise domain, the recently developed *Bored of Sports Scale* (BOSS; Wolff et al., 2020a) already allows researchers to assess individual differences in exercise-related boredom (example item: "Exercising is dull and monotonous") (Wolff et al., 2020a). Overall, there is a need to adapt and develop further sport specific questionnaires to measure boredom experienced in specific sport settings (e.g., boredom during long-distance runs), during specific exercise activities (e.g., jogging), and in the distinct settings of individual vs. collective activities (e.g., gym workouts vs. team sports).

Boredom has additional substantive implications for research in sports and exercise that only peripherally affect task-induced self-control demands. For instance, when running there may be several moments when the mind is engaged with unrelated thoughts—a phenomenon referred to as mind-wandering (Smallwood and Schooler, 2006; Christoff et al., 2018). Mind-wandering has become a highly researched topic in the past few decades (Callard et al., 2013) and has been shown to be related to boredom (Isacescu et al., 2017; Martarelli et al., 2020). Indeed, both experiences signal that a current task is not engaging one's attentional resources fully. Mind-wandering might occur when the actual experience is boring. When one cannot change overt behavior, an alternative that is always available is to explore inner worlds (Mills and Christoff, 2018). Like boredom, mind-wandering has only scarcely been addressed in sports science (Latinjak, 2018). However, it is a relevant concept, because especially deliberate forms of mind-wandering might be used as a strategy to counteract boredom. On the other hand, spontaneous mind-wandering might derail attentional engagement with an exercise and thereby further exacerbate boredom and the challenge of continuing the activity. Moreover, as is the case with the dynamics of boredom and self-control, mind-wandering likely also changes over time and should not be considered as a static experience (Christoff et al., 2016). Investigating boredom and related constructs in a sports context is relevant not only for understanding their impact on sports engagement and performance but also as a potential avenue to improve participation rates in sports activities by helping individuals to better regulate their engagement with these healthful activities. In the same vein, as some people utilize sport and exercise to alleviate boredom (Morris et al., 2003), it is crucial to understand the social (e.g., exercising in a group as opposed to exercising alone) and contextual (e.g., participating in virtual bike racing, as opposed to simply pedaling alone on the hometrainer) conditions that make sports and exercise a powerful remedy for boredom.

To conclude, boredom is omnipresent in everyday life, and the sports and exercise context is no exception. We call for investigating *when* and *why* boredom occurs in self-control research in sports and exercise, and *how* it affects goal-directed behavior in these settings.

DATA AVAILABILITY STATEMENT

The datasets presented in this study can be found in online repositories. The names of the repository/repositories and accession number(s) can be found below: https://osf.io/6uc2k/.

ETHICS STATEMENT

Ethical review and approval was not required for the study on human participants in accordance with the local legislation and institutional requirements. The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

WW, MB, CM, and JD developed the ideas presented here. WW wrote the first draft of the manuscript. WW, MB, CD, and JD revised the manuscript. All authors contributed to the article and approved the submitted version.

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BRIEF RESEARCH REPORT
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Testing the Effects of a Preceding Self-Control Task on Decision-Making in Soccer Refereeing

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The present study tested the assumption that the momentary level of self-control strength affects the accuracy rates in a sports-related judgment and decision-making task. A total of $N = 27$ participants rated the veracity of 28 video-taped statements of soccer players who were interviewed by a non-visible referee after a critical game-related situation. In half of the videos, the players were lying, and in the other half, they were telling the truth. Participants were tested twice: once with temporarily depleted self-control strength and once with temporarily available self-control strength (order counterbalanced; measurements separated by exactly 7 days). Self-control strength was experimentally manipulated with the Stroop task. In line with two-process models of information processing, we hypothesized that under ego depletion, information is processed in a rather heuristic manner, leading to lower accuracy rates. Contrary to our expectations, the level of temporarily available self-control strength did not have an effect on accuracy rates. Limitations and implications for future research endeavors are discussed.

Keywords: decision-making, ego depletion, self-control, cognitive fatigue, sports, effort, Stroop, refereeing

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INTRODUCTION

Deception in sports is a critical issue as it might decisively change the outcome of a match (Güldenpenning et al., 2017). According to Hsu (1997), deception means "making someone believe something that is not true in order to get what you want" (p. 167). For instance, a wrongfully granted penalty kick during overtime in a tied soccer match will likely determine which team wins the game (Sabag et al., 2018). In sports, lying to the referee can be considered a special form of deception. While research on deception has a long tradition in sports (for an overview see Güldenpenning et al., 2017), and the ability to detect deceit and, especially, lies has been center stage in the criminal justice system (e.g., Akehurst et al., 1996) as well as in educational settings (e.g., Marksteiner et al., 2013) for many years, only recently has the topic of lie detection been addressed in sports-related contexts. This seems rather surprising, given the high potential impact of "successfully" lying to a referee.

Given the impending influence of deceit on the results of a sporting competition, it seems highly important that a referee's judgment and decision-making take place as accurately as possible.

However, as far as we know, there have been very few systematic, experimental studies on referee accuracy rates regarding deception (e.g., Morris and Lewis, 2010; Renden et al., 2014; Aragão e Pina et al., 2018), as most studies on deception in sports have been correlational and, for instance, asked their participants how they would possibly behave in a certain hypothetical situation (e.g., Kavussanu and Ntoumanis, 2003). A notable exception is a study series by Morris and Lewis (2010), in which they first generated a sequence of video clips in which soccer players were instructed to overstate the effects of a tackle by an opposing player. In a subsequent study, neutral observers rated each video clip whether the respective video-taped player had actually been fouled or not. The results revealed that the neutral observers judged the video-clips very accurately. Another experimental study on lie detection was conducted by Englert and Schweizer (2020). Taking a similar approach, the authors first created 28 video clips in which soccer players were either telling the truth or lying regarding two simulated critical game situations. The veracity of each of the 28 video clips was later rated by neutral observers in a series of three studies. The results were rather mixed, as the statements of some of the interviewed players were rather easy to classify, while other players were fairly good at lying. When looking at the accuracy rates of correctly classifying truths and lies in other domains (e.g., the criminal justice system), recent meta-analyses indicate that, overall, individuals are not very accurate at detecting lies, or more precisely, they are only slightly better than the chance level (i.e., accuracy rate of 54%) (e.g., Bond and DePaulo, 2006).

It remains largely unknown which factors influence the accuracy rates of referees. Previous meta-analyses found no empirical evidence that gender, age, expertise, or certain personality traits significantly impacted the accuracy rates (e.g., Aamodt and Custer, 2006; Bond and DePaulo, 2006). In order to identify potential factors, we must first take a closer look at the actual judgment and decision-making process. Dual-process models of information processing assume that there are two different types of information processing when making a judgment (e.g., Chaiken and Maheswaran, 1994; Chaiken and Trope, 1999; Petty et al., 2005) (for an application of dual-process theorizing to the domain of sports see Furley et al., 2015): *Heuristically* (also called *peripheral route*) or *systematically* (also called *central route*). When processing information and making a judgment in a heuristic manner, individuals focus less carefully on the content of a statement and more so on peripheral cues, such as the likability or trustworthiness of the source or simply the number of arguments presented by the source (Petty et al., 2005). On the contrary, systematic information processing allows a person to carefully pay attention and evaluate the quality of the arguments presented (e.g., Chaiken and Trope, 1999). The importance of dual-process models has also been shown in other sport- and exercise-related settings (Furley et al., 2015): for instance, a physically inactive person might have the intention to work out in the evening, but has a negative attitude toward physical exercise and tends to avoid straining physical activities (e.g., Bluemke et al., 2010). In the evening, his/her favorite TV program is on and the person has to make a decision on whether to exercise or not. When making the decision heuristically, the person is less likely to exercise as he/she pays less attention to the positive aspects of physical activity. However, when making the decision systematically, he/she weighs the positive and negative aspects of exercising against one another and is more likely to work out (see also Englert and Rummel, 2016). Taken together, heuristic information processing is less reflective and requires less effort than systematic information processing (Petty et al., 2009; Petty et al., 2005). Previous research from the criminal justice system has reliably shown that judgments are more accurate when taking the systematic information processing route (e.g., Feeley and DeTurck, 1995; Masip et al., 2009; Vrij et al., 2010). This leads to the question: Which factors determine which type of information processing dominates in a given situation? One potential candidate is the level of temporarily available self-control strength, which we will describe in more detail in the following sections (e.g., Davis and Leo, 2012).

According to the strength model, all self-control acts are based on a global metaphorical resource with a limited capacity (e.g., Baumeister et al., 1998; see also Audiffren and André, 2015; André et al., 2019). In this context, self-control means inhibiting certain impulses or response tendencies in order to keep striving for desirable outcomes and to perform at the highest possible level (e.g., Englert, 2017, 2019). Self-control acts include, amongst others, emotion regulation, attention regulation, and most importantly for the present investigation, judgment, and decision-making (Hagger et al., 2010; Samuel et al., 2018) (for an overview, see also Englert, 2017, 2019). It is assumed that after individuals have worked on a self-control task their self-control resources become temporarily depleted for a certain amount of time. During this so-called state of *ego depletion*, following self-control tasks are executed less efficiently as less cognitive effort is likely to be invested (e.g., Baumeister et al., 1998). Given that self-control strength needs to be exerted in order to process information via the cognitively demanding systematic route, previous empirical research has shown that ego depleted individuals tend to process information in a heuristic manner (e.g., Wheeler et al., 2007; Baumeister et al., 2008; Unger and Stahlberg, 2011). In two studies, Reinhard et al. (2013) manipulated ego depletion and found out that ego-depleted participants were more likely to process information heuristically and displayed lower lie detection accuracy rates than non-depleted participants (for similar findings, see also Wheeler et al., 2007; Davis and Leo, 2012).

Based on these empirical findings and theoretical assumptions, we assumed that individuals are more likely to process information heuristically if they had been working on a straining self-control task before (i.e., under ego depletion). As systematic information processing is associated with higher accuracy rates during judgment and decision-making, we tested the hypothesis that depleted individuals are less accurate in correctly classifying ambiguous situations during a soccer match than non-depleted participants (see also Reinhard et al., 2013). In order to test these assumptions, we adopted Englert and Schweizer's (2020) approach and asked participants at two separate times of measurement to rate the truth of a series of 28 video-taped statements of soccer players, in which they either lied to a referee or told him the truth. At one time of measurement, participants' self-control strength was experimentally depleted, while it remained intact at the other time of measurement (order counterbalanced).

MATERIALS AND METHODS

Participants

A G*Power (Faul et al., 2007) analysis showed that a sample of $N = 27$ was necessary for detecting at least a medium effect (parameters: $f = 0.30$, $\alpha = 0.05$, $1 - \beta = 0.85$, $r_{\text{repeated measures}} = 0.50$, $\varepsilon = 1$). Based on this estimate, a total of $N = 27$ university students from a German university volunteered to partake in the present investigation (16 females, 11 males; $M_{Age} = 27.74$ years, $SD_{Age} = 7.17$). Three participants had soccer refereeing experience ($M = 3.67$ years, $SD = 3.79$). The study was approved by the local ethics committee, and all participants delivered written informed consent.

FIGURE 1 | Illustration of the experimental setup for the generation of the stimulus material. The player wearing the jacket is a confederate acting as an attacking player, the player wearing the white jersey is a confederate acting as the teammate of the attacking player, and the player wearing the black jersey is the target player acting as the defender. The referee is standing on the right, observing the scene.

Design, Procedure, and Measures

The participants were tested at two times of measurement exactly 7 days apart under standardized conditions in single sessions on a regular computer in a university lab room. All instructions, video clips, and questionnaires were delivered via an online survey program (Unipark). Each participant was wearing regular stereo headphones, and the sound was played at a constant volume. At one time of measurement, participants' self-control strength was experimentally depleted (depletion condition), while it remained intact at the other time of measurement (control condition; order counterbalanced). First, participants reported demographic information (i.e., age, sex, and refereeing experience).

Then, self-control strength was experimentally manipulated using the Stroop test, which has been frequently applied in self-control research (e.g., Bray et al., 2012; Englert and Bertrams, 2014). The Stroop test consists of color words which are displayed either in the same font color as the color word (congruent Stroop trial; e.g., the word "red" written in red font color) or in a different font color (incongruent Stroop trial; e.g., the word "red" written in yellow font color); participants need to always name the font color instead of the written color word. It has been reliably shown that in order to ignore the color word and to read the font color instead, self-control needs to be invested, which is why this task has been regularly applied to manipulate self-control strength. In the present study, at both times of measurement, participants first performed a series of 32 practice trials and then worked on 300 incongruent Stroop trials in the depletion condition and on 300 congruent Stroop trials in the control condition. The number of falsely identified Stroop trials and the average response latencies were measured as manipulation checks, assuming that in the depletion condition, participants would make more mistakes and would need longer to answer each trial (in milliseconds) (e.g., Bray et al., 2012; see also Pageaux et al., 2014).

At both times of measurement, following the Stroop task, the participants were informed that they would be watching a series of video clips. These video clips were taken from Englert and Schweizer's (2020) study, in which the authors created 28 video clips in which male soccer players from a club from the sixth highest league in Germany (out of 11 leagues) were either telling the truth or lying regarding two simulated critical game situations. These simulated game situations took place immediately before an interview with a professional soccer referee (see **Figure 1**). In both situations, the player acted as a defender as another player played a long pass toward the goal line for his teammate. Once, the defender was asked to not allow the other player to get to the ball and to instead let the ball cross the goal line, which would lead to a goal kick for his team. In the other situation, the instructions were similar with the only difference being that the defender did actually touch the ball last before it passed the goal line. In this latter case, the correct decision would have been a corner kick. However, in both situations, the defender was asked to tell the referee, who had not seen the critical situation, in the subsequent video interview, that the offensive player had touched the ball last and the correct decision was supposedly a goal kick, meaning that the defender was telling the truth in one interview and was lying in the other. The referee asked each player exactly the same questions and was not seen in the video. The participants in the current study did not watch the critical situation, but only the subsequent interview. The participants were also told that each player was in a similar critical situation twice during the same game and would thus be interviewed by the same referee at two separate times. However, the participants were not made aware of the fact that each player was lying in one interview and speaking the truth in the other interview, leading to a total of 14 true statements and 14 lies. On average, each video clip lasted roughly 28 seconds ($M = 27.5$, $SD = 6.27$), and the player's upper torso, face, and legs could be seen in each clip. The sound quality was the same in all video clips. Participants were further instructed that they would have to rate the veracity of each interview on a continuous scale ranging from 1 (*not at all true*) to 10 (*totally true*) immediately following each video clip (for this procedure, see also Marksteiner et al., 2013). The video clips were displayed in a randomized order immediately after finishing the Stroop task in both conditions. In total, participants rated the veracity of 28 video statements while being ego depleted and the veracity of the same 28 video statements with fully available self-control strength. In order to reduce the likelihood of a learning effect, the two times of measurement were separated by exactly 7 days, and the order of the video presentation was randomized.

Finally, after the second time of measurement, the participants were debriefed and thanked for their participation.

Data Analysis

Data were analyzed using SPSS (version 27; SPSS Inc., Chicago, IL, United States). We ran paired samples t-tests to investigate the assumptions that the depletion condition would perform worse in the Stroop task (i.e., longer response latencies in milliseconds; higher number of Stroop errors) and would be less adept in correctly distinguishing between true and false statements than the control condition. All effect sizes were calculated as Cohen's d (i.e., small effect: $d = 0.2$; medium effect: $d = 0.5$; large effect: $d = 0.8$; Cohen, 1988). For all analyses, statistical significance was accepted as $p < 0.05$.

RESULTS

Preliminary Analyses

As expected, the Stroop response latencies in the depletion condition (M = 839.08 ms, SD = 179.68) were significantly longer than in the control condition (M = 717.55 ms, SD = 156.80), $t(26) = 7.02$, $p < 0.0001$, $d = 1.35$. Additionally, there was the expected tendency in the number of Stroop errors between the depletion condition ($M = 7.96$, $SD = 6.00$) and the control condition ($M = 6.59$, $SD = 5.80$), which however failed to reach statistical significance, $t(26) = 1.86$, $p = 0.075$, $d = 0.36$. On average, the depletion condition (M = 331633.70 ms, SD = 69442.08) needed significantly longer to finish the 300 Stroop trials than the control condition (M = 295259.67 ms, SD = 53785.51), $t(26) = 3.89$, $p < 0.0001$, $d = 0.75$.

Primary Analyses

In line with Englert and Schweizer's (2020) approach, for both conditions, we first compared the veracity ratings of the true statements to the veracity ratings of the lies in order to investigate the question of whether participants in both conditions were able to distinguish (on average) between true and false statements (for descriptive statistics see **Table 1**). In both groups, false statements were rated significantly lower than true statements, indicating that participants in both conditions were able to distinguish between true and false statements (control: $t(26) = 2.15$, $p = 0.041$, $d = 0.41$; depletion: $t(26) = 4.34$, $p < 0.001$, $d = 0.83$).

Next, in order to investigate potential differences between the depletion and the control conditions, we compared the ratings of the true statements between the two times of measurement (control vs. depletion). Contrary to our hypothesis, the veracity ratings did not differ statistically significantly between the depletion condition ($M = 5.99$, $SD = 0.81$) and the control condition ($M = 5.83$, $SD = 0.98$), $t(26) = 0.81$, $p = 0.426$, $d = 0.16$. There were also no significant differences between the depletion condition ($M = 5.39$, $SD = 0.73$) and the control condition ($M = 5.32$, $SD = 0.98$) in the veracity ratings of the false statements, $t(26) = 0.39$, $p = 0.703$, $d = 0.07$ (see also **Table 1**).

Complementary Bayesian Hypothesis Testing

We ran additional Bayesian paired samples t-tests, to further investigate whether the differences in the veracity ratings of true and false statements between the depletion and the control condition do not exist (i.e., that the null hypotheses are more likely to be true; for this approach, see also Dienes, 2014; Wagenmakers et al., 2018a,b). For the true statements, a two-sided analysis revealed a Bayes factor (BF01) suggesting that the data were 3.64 times more likely under the null (i.e., the two conditions do not differ in their veracity statements of the true statements) than the alternative hypothesis (i.e., the two conditions differ) with a median effect size of 0.14, which indicates moderate evidence in favor of the null hypothesis. For the false statements, the results indicate that the observed data are 4.58 times more likely under the null (i.e., the two conditions do not differ in their veracity statements of the false statements) than the alternative hypothesis (i.e., the two conditions differ) with a median effect size of 0.07, which indicates moderate evidence in favor of the null hypothesis.

DISCUSSION

In the present study, we tested the assumption that individuals would be less adept in correctly identifying the veracity of a player's statement following a critical game situation during a soccer match if they had been working on a straining self-control task beforehand. For that reason, participants rated a series of video statements at two times of measurement, once with fully available self-control strength and once in a state of ego depletion (order counterbalanced). According to two-process models, there are two types of information processing, namely a heuristic and a systematic mode. When judging the veracity of a statement in a heuristic manner, individuals tend to focus on rather invalid cues to deception (e.g., number of statements), while a systematic mode is related to an increased focus on valid cues (e.g., actual content of the statement) and a higher likelihood of classifying a statement correctly (DePaulo et al., 2003; Forrest et al., 2004). But, systematic information processing is effortful and, according to several authors, requires self-control strength (e.g., Wheeler et al., 2007; Baumeister et al., 2008; Unger and Stahlberg, 2011; Davis and Leo, 2012; Reinhard et al., 2013). If one's self-control

TABLE 1 | Mean veracity ratings for the true and false statements, separated by condition (depletion vs. control).

Statement	Depletion condition		Control condition	
	M	*SD*	*M*	*SD*
True	5.99	0.81	5.83	0.98
False	5.39	0.73	5.32	0.98

N = 27. Each video was rated on a continuous scale ranging from 1 (not at all true) to 10 (totally true).

resources had been taxed in a previous task, he/she is less likely to have the necessary self-control strength to process information systematically and will tend to process heuristically instead. However, the results did not support our hypothesis as there were no statistically significant differences in the accuracy rates between the control and the depletion condition.

When investigating why the control and the depletion condition did not differ regarding their veracity ratings, it is important to emphasize that in both conditions, participants actually could differentiate between true and false statements (although not very strongly). This can be considered a necessary prerequisite for testing our main hypothesis: If participants in the control condition cannot distinguish between true and false statements, then they cannot get worse in the depletion condition. Given that this prerequisite was met, how can we then explain that participants in the depletion and the control conditions did not differ, considering that the study was adequately powered and that the depletion manipulation was effective? One potential explanation for this pattern is that participants in the control condition did rely on heuristic processing as well. This would both explain why participants were not able to distinguish more strongly between false and true statements (because doing so would require more systematic processing) and why they did not get worse in the depletion condition. To address this issue, further research might want to employ not only a condition that is supposed to decrease systematic and to increase heuristic processing (such as the depletion condition in the present research), but furthermore a condition that is supposed to increase systematic processing. This might be accomplished by incentivizing participants, for example (see also Beckmann, 2020).

Another potential explanation might be the low level of expertise/experience of the participants in our study (only three participants had soccer refereeing experience), as one might reason that participants with soccer refereeing experience are better at correctly judging player statements (e.g., MacMahon et al., 2007; Moore et al., 2019). Even though several large-scale studies from the criminal justice system and educational psychology have reliably demonstrated that the raters' expertise does not affect their accuracy rates (e.g., Aamodt and Custer, 2006; Bond and DePaulo, 2006), future studies should investigate whether the same is true in sports-related judgment and decision-making situations.

We would also like to address the fact that the depletion condition took significantly longer to finish the Stroop task than the control condition. This matter seems especially important, as a recent study by Boat et al. (2020) revealed that longer Stroop task durations were related to lower performance in a subsequent self-control task. However, in the current study we did not find an effect of the different Stroop task durations on the veracity ratings. Future studies should continue to dig deeper into the effects of different self-control task durations on performance (see also Wolff et al., 2021).

Individuals do not only differ in their levels of temporarily available self-control, but also in their general self-control abilities, meaning that some are simply better at regulating themselves than others (i.e., trait self-control; Tangney et al., 2004). In general, individuals with higher levels of trait self-control are more adept at volitionally controlling their impulses and focusing on the task at hand (e.g., De Ridder et al., 2012). In the current study, we did not measure trait self-control strength; however, given the fact that we applied a repeated measures design, we assume that trait self-control strength did not play a major part in our study. It has to be noted that the validity of the ego depletion effect itself has been questioned on theoretical and empirical grounds. On an empirical level, some recent large-scale replication studies did not find reliable statistical evidence for the ego depletion effect (e.g., Hagger et al., 2016; Blázquez et al., 2017). For instance, Vohs et al. (2021) conducted a preregistered replication report with over 3,500 participants from 36 labs worldwide. While participants with depleted self-control did not differ significantly from the non-depleted participants in terms of their performance, depleted participants did feel more fatigued than control participants. So, why did depleted participants feel fatigued while their actual performance did not suffer from their depletion? It might be reasonable to assume that the dependent variable in the Vohs et al.'s study (Cognitive Estimation Test) (Bullard et al., 2004) was not self-control demanding *enough*. If the dependent measure only requires minimal effort, it is highly unlikely to be affected by a straining preceding self-control task (see also Loschelder and Friese, 2016). In a similar fashion, in our study rating the videos systematically might not place sufficiently high self-control demands on one's self-control resources, thus making it more difficult to find statistically significant differences between the depleted and the non-depleted conditions. Furthermore, while the results of the Stroop test revealed the expected differences between the depletion and the control condition, we did not apply an additional manipulation check measuring the level of perceived depletion following the Stroop task. This notion seems especially important, as for instance Clarkson et al. (2010) have demonstrated that participants who perceived themselves as being more depleted performed worse in following self-control acts than participants who perceived themselves as being less depleted (see also Wright and Mlynski, 2019). Even though previous studies have reliably shown that participants reported significantly higher levels of perceived depletion after the incongruent Stroop task compared to the congruent one (e.g., Hagger et al., 2010), future studies should apply additional manipulation checks to test the effectiveness of the respective ego depletion manipulation.

On a theoretical level, several researchers argue that the assumption of a limited metaphorical self-control resource is not appropriate and cannot be adequately tested empirically (for a discussion, see also Eronen and Bringmann, 2021). For instance, the process model by Inzlicht and Schmeichel (2012, 2016) postulates that a primary self-control act does not deplete limited resources but rather instigates shifts in motivation (i.e., the person does not want to work on another straining task), emotions (i.e., the person perceives other straining tasks as rather negative), and attention (i.e., impaired attention regulation), which ultimately affects performance in subsequent self-control tasks. In a similar fashion, according to the behavioral restraint extension of the general fatigue analysis (e.g., Wright and Agtarap, 2015; Wright and Mlynski, 2019), the amount of self-control (i.e., restraint intensity) one can or, more precisely, is willing to invest in a given task is not dependent on temporarily available self-control resources. Rather, it is a function of perceived fatigue, task difficulty (i.e., the magnitude of an unwanted urge), and success importance (i.e., the importance of resisting the urge), with associated cardiovascular responses following (i.e., changes in systolic and diastolic blood pressure as well as mean arterial pressure; Wright et al., 2012). Therefore, fatigue does not automatically lead to less effort or impaired self-control performance (e.g., Wright et al., 2013). For instance, if a fatigued person thinks that success in an upcoming task is highly unlikely and that success is not especially important, he or she is unlikely to invest high amounts of effort which will eventually lead to impaired performance. However, if the same person views success in the upcoming task as likely and important, he or she will be willing to invest more effort and perform at a higher level. Assessing these additional psychological and physiological parameters specified in the process model as well as the behavioral restraint extension of the general fatigue analysis might shed some light on the actual mechanisms contributing to our present pattern of results.

Taken together, even though we did not find statistically significant differences between the control and the depletion condition in accuracy rates, we do consider the present findings to be highly informative. First, they suggest that participants are not necessarily worse at detecting lies in sports when in a state of ego-depletion. Second, the present findings suggest fruitful avenues for further research (e.g., different manipulations for systematic and heuristic processing). Third, it adds to the recent discussion surrounding the ego depletion effect, indicating that systematic information processing might be less prone to be affected by states of ego depletion. Fourth, it highlights the necessity to dig deeper into the psychological and physiological mechanisms potentially affecting self-control performance.

DATA AVAILABILITY STATEMENT

The raw data supporting the conclusions of this article will be made available by the authors, without undue reservation.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by the Ethics Board of the University of Bern. The patients/participants provided their written informed consent to participate in this study. Written informed consent was obtained from the individual(s) for the publication of any potentially identifiable images or data included in this article.

AUTHOR CONTRIBUTIONS

CE, AD, and GS equally contributed to the conceptualization of the study and review of relevant related work. CE, GS, and AD analyzed and interpreted the data. CE and GS prepared the draft manuscript. AD provided the critical revisions. All authors approved the final version of the manuscript and agreed with the order of presentation of the authors.

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OPINION
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Again, No Evidence for or Against the Existence of Ego Depletion: Opinion on "A Multi-Site Preregistered Paradigmatic Test of the Ego Depletion Effect"

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INTRODUCTION

The ego depletion effect has been one of the most cited psychological phenomena since Baumeister et al. first introduced the term in 1998. The authors assume that individuals only possess a limited (metaphorical) self-control resource or strength that can become temporarily depleted after having engaged in a self-control demanding task (i.e., *ego depletion*). In a typical experimental setup (i.e., the sequential two-task paradigm), participants first work on a task that either does or does not require self-control exertion (e.g., an incongruent vs. congruent Stroop task), which should therefore lead to ego depletion in the former case, while self-control strength should remain relatively stable in the latter case (e.g., Webb and Sheeran, 2003). Afterwards, all participants work on another self-control task to measure their momentary self-control strength. The assumption that self-control performance suffers in the state of ego depletion (i.e., the second task performance is lower in the depleted compared to the non-depleted control condition) has been supported in hundreds of studies (e.g., Dang et al., 2021) and two meta-analyses (Hagger et al., 2010; Dang, 2018).

THE BEGINNING OF THE "REPLICATION CRISIS"

The ego depletion effect has come under scrutiny in recent years; for instance, Carter and McCullough (2013, 2014) argued that publication bias might have inflated the estimated size of the ego depletion effect. In 2016, Hagger and colleagues conducted a large-scale replication study (i.e., Registered Replication Report; RRR) with more than 2,000 participants from 23 laboratories worldwide, also adopting the sequential two-task paradigm. The e-crossing procedure (e.g., Baumeister et al., 1998) served as the initial task to manipulate ego depletion: In the control condition, participants saw a series of words on a computer screen and had to press a certain button on the keyboard whenever the respective word contained the letter "e." In the depletion condition, participants were asked to only press the button when the word had an "e" that was not adjacent to another vowel. Contrary to the hypotheses, the study did not find any reliable evidence supporting the ego depletion effect as performance in a subsequent secondary self-control task did not differ between the two conditions.

In the aftermath of the RRR, Baumeister and Vohs (2016) questioned the appropriateness of the e-crossing procedure, arguing that "in retrospect, the decision to use new, mostly untested procedures for a large replication project was foolish" (p. 574). The authors suggested other ego depletion tasks, which were rejected by the lead authors of the RRR as Hagger et al. (2016) wanted to apply computerized tasks that were culturally and linguistically neutral [for a response, see also

Hagger and Chatzisarantis (2016)]. We agree with Baumeister and Vohs (2016) that the e-crossing procedure might not have been an ideal choice to manipulate ego depletion as "self-regulation is typically understood as altering and overriding responses" (p. 574). In the e-crossing task as applied by Hagger et al. (2016), participants did not have to override any response tendencies, habits or impulses as they had never worked on the e-crossing task before and did not have the opportunity to first build up a response habit. To make matters even more interesting, a recent study by Wimmer et al. (2019) in which the authors manipulated the difficulty of the e-crossing task by modifying the text from semantically meaningful to non-meaningful sentences and by increasing ego-depletion rule complexity did not find any effect on a subsequent Stroop task, raising the question of whether the e-crossing task is useful to induce ego depletion. Consequently, if ego depletion had not been successfully manipulated in the RRR, it does not seem surprising that the control and experimental conditions did not differ in their performance in the second self-control task.

THE MULTI-SITE PREREGISTERED PARADIGMATIC TEST OF THE EGO DEPLETION EFFECT

In their recently published multi-site project, Vohs et al. (2021) made another attempt to assess the size and robustness of ego depletion effects. For this reason, the authors also adopted the sequential two-task paradigm in a study with more than 3,500 participants from 36 laboratories. The laboratories had the choice between applying the *e-task protocol condition* ($n = 20$ laboratories) or the *writing task protocol condition* ($n = 16$ laboratories). The results were inconclusive; that is, overall, the data neither clearly support nor debunk the existence of the ego depletion effect. Interestingly, higher self-reported fatigue after the initial self-control demanding task was associated with lower subsequent self-control performance—a pattern largely in line with previous findings (e.g., Clarkson et al., 2010; Englert et al., 2021) and recent theorizing (Bertrams, 2020).

In the e-task protocol condition, the e-crossing procedure was used as the initial task to manipulate ego depletion. In contrast to the RRR (Hagger et al., 2016), participants from both conditions first built a habit by crossing off all instances of the letter "e" on a sheet of text. Afterwards, they worked on another text and, as was the case in Hagger et al.'s RRR, the control condition again crossed out each instance of the letter "e," while the experimental condition received the more difficult crossing instructions (i.e., only cross out the letter "e" if there was a vowel before or after the letter). In total the e-crossing task lasted up to 15 min. We would like to point out that repetitively working on a simple task for 15 min or close to that in the control condition might lead to increased levels of boredom. Coping with boredom is a self-control demand of its own (Wolff and Martarelli, 2020); thus, in both the depletion and the control conditions, participants' self-control resources could have been strained after the e-crossing task, undermining the likelihood of detecting a possible ego depletion effect.

Afterwards, as dependent variable the degree of persistence the participants demonstrated when working on a set of figure tracing tasks was measured (i.e., time spent on the figure tracing task and the number of figures participants worked on). To master the figure tracing task, participants had to trace series of figures in their entirety with a highlighter marker and were neither allowed to pick up the marker at any time nor to cross the same line segment twice (Vohs et al., 2008). Participants were unaware that some of the figures were actually unsolvable. Depending on the type of analysis, there was a small ego depletion effect on how long the participants tried to solve the puzzles for. While this result must not be overstated as evidence supporting the existence of the ego depletion effect, it equally fuels doubts about the assumption that the ego depletion effect is nothing but pure fantasy.

While our main criticism focuses on the writing task protocol condition, we would like to briefly discuss the validity of the figure tracing task as well. First, there are some degrees of freedom how to analyze performance in the figure tracing task, namely analyzing the time spent on the task and the number of tasks participants worked on separately, or analyzing a combination of these two outcome measures. Second, the amount of effort one is willing to invest in the task largely depends on one's believe that the tasks are actually solvable or not. If a person realizes that the respective figure cannot be traced perfectly, stopping the task is actually the better option than going on. While Vohs et al. controlled for this possibility by excluding participants who were aware of the fact that some figures were unsolvable, we at least question whether spending more time on an unsolvable task is indeed indicative of "better" performance.

As said, our main criticism refers to the writing task protocol condition. In this condition, self-control strength was experimentally manipulated with a writing task that required the inhibition of certain letters [see also Bertrams et al. (2010)]. In our view, this writing task does indeed require self-control as individuals needed to inhibit their well-developed writing habits. However, we take issue with the use of the Cognitive Estimation Test (CET, Bullard et al., 2004), which was applied as the subsequent second task (i.e., the dependent variable). The CET requires participants to *guess* the answers to a series of 20 questions (19 questions in the Vohs et al. study) that have unclear answers, meaning that participants needed to generate novel responses (e.g., "How many seeds are there in a watermelon?," "What is the age of the oldest living person in the United States?," "How long does it take to iron a shirt?," and "How long does it take for fresh milk to go sour in the refrigerator?"). According to Vohs et al. (2021), the CET requires self-control because the answers cannot be determined algorithmically or with declarative knowledge. This is an overly succinct rationale from which it does not logically follow that the CET does require self-control. In previous research (Schmeichel et al., 2003), it was claimed that each CET question can be appropriately answered by reasoning and consideration of related knowledge—or more precisely *via* fluid cognitive processing, which is enabled by the central executive of the working memory system [see also Shallice and Evans (1978)]. Based on the

CET performance, Vohs et al. (2021) did not observe any evidence of the ego depletion effect. This makes sense to us as we cannot see that the CET measures self-control or any other executive functioning that should be impaired by recent self-control demands.

First, it seems obvious that some items of the CET may well-depend on prior knowledge, which shrinks the variance that could be explained by the ego depletion manipulation. For instance, people who iron their shirts regularly will be more accurate in the CET than someone who always thought ironing is a waste of time. Second, if the use of the CET as a self-control measure would be justified by its (potential) reliance on executive working memory processes, recent research which has found that working memory tasks possibly do not rely on self-control strength (Dang, 2018) should be taken into account. Third and most important, the CET was not designed to measure fluctuating within-individual variables, such as self-control strength, but primarily to help distinguish between healthy individuals and those with certain clinical conditions (e.g., dementia or ADHD; Bullard et al., 2004). Therefore, the CET may be seen as a measure of "abnormality" (Bullard et al., 2004, p. 835), which becomes clearer by paying closer attention to how CET scores are determined. There are no correct solutions in this test in the objective sense; that is, it does not matter, for example, how many seeds actually are in a watermelon and how far the participants' answers diverge from this *true* value. Rather, the scoring system is either based on the answers of a small unrepresentative sample ($N = 113$; Bullard et al., 2004) or an unknown sample reported in unpublished gray literature [Fein et al., 1998; see Schmeichel et al. (2003)]. In Vohs et al.'s study, estimations that were within the 25–75th percentile interval of this norm sample received two points, answers within the 5–24th or the 76–95th percentiles received one point, and answers outside these intervals received zero points. How arbitrary this scoring system is, becomes even more apparent given the fact that in another ego depletion study, the participants within the 90% response range (rather than in the 95% response range; Vohs et al., 2021) of the norm sample were awarded one point (Schmeichel et al., 2003). From all this, it follows that, at best, the CET can identify the (maybe clinically relevant) tendency to give more or less untypical estimations, whereby the reasons for such deviations are unknown. Given the concerns about the internal consistencies of cognitive estimation tests, the items of these tests may even measure different constructs (Scarpina et al., 2015). Vohs et al. (2021) did not report the internal consistency of the CET in their study, which is typically rather low [e.g., Cronbach's $\alpha = 0.60$ in Schultz and Ryan (2019)]. Taken together, in our opinion, the CET is neither a reliable nor a valid measure of self-control. Thus, Vohs et al.'s (2021) writing task protocol condition does not offer any insights into whether ego depletion is real or not, independent of their results.

Our final concern with Vohs et al.'s study regards the overall study design, namely that the ego depletion manipulations were potentially confounded with the outcome measures. More precisely, based on the present findings it is unclear whether the writing task would have affected performance in the figure tracing task differently than the e-crossing task. Likewise, it might be possible that the e-crossing task had a stronger effect on the CET than the writing task. Therefore, future studies might consider to fully cross the independent and dependent variables of the two protocols.

REPLICATION REQUIRES APPROPRIATE OPERATIONALIZATION

Just as in the RRR, we are puzzled why the authors organized such a complex and highly important research project choosing a task as the dependent variable that by no means meets the definition of a self-control task (i.e., overriding habits; Baumeister and Vohs, 2016) and has not been demonstrated to psychometrically soundly measure the construct of interest. The authors explain that their task choice was based on the so-called *paradigmatic replication approach* as they asked ego depletion experts to generate "possible tasks for the study's procedures, focusing on their paradigmatic fit with the construct" (p. 4). It seems odd to us that the experts chose the CET, which is not paradigmatic at all for reliably and validly measuring momentary self-control. According to Lishner (2015), replication efforts can be assigned to a replication continuum ranging from "exact" to "maximally divergent," and "consistent but false findings are more likely to occur in the process of replication when one moves farther away from the 'exact' side of the replication continuum toward the maximally divergent side" (p. 57). To us, the current replication effort is closer to the divergent side of this continuum given the—in our eyes—inappropriateness of the CET.

In general, it has to be acknowledged that there is no broad consensus which tasks are *valid* self-control tasks and how long self-control needs to be invested in a given task in order to actually induce ego depletion [see also Englert (2017), e.g., Boat et al. (2020)]. For instance, it remains unclear how long a Stroop task should ideally last or how many trials it should contain (e.g., Wolff et al., 2021). Based on these inconsistencies in experimental methodology, researchers have high levels of degrees of freedom when planning ego depletion experiments.

CONCLUDING REMARKS

We would like to point out that we are not picking a side as to whether ego depletion exists or not; that is not the aim of this opinion article. The goal is to outline the necessity to properly operationalize the central constructs of a theoretical model in order to test its validity, and we strongly believe that this was not achieved in Vohs et al.'s (2021) multi-site study. In a recent meta-analysis, Dang (2018) reported the effect sizes for the most commonly used ego depletion tasks, and we would encourage future replication efforts to choose appropriate self-control tasks based on empirical evidence. We agree with Nelson et al. (2018) that a critical methodological reflection of traditional and current research practices can lead to "psychology's renaissance" (p. 511). We also agree with Popper (1963) that "the criterion of the scientific status of a theory is its falsifiability, or refutability, or testability" (p. 33), meaning that as researchers, it is our obligation to test the validity of theoretical models over and over again in order to increase trust in their robustness, especially given the recent replication crisis in psychological science. However, in order to test a model's validity, valid procedures need to be applied. In our eyes, this was not the case in Vohs et al.'s (2021) new multi-site project.

AUTHOR CONTRIBUTIONS

CE and AB equally contributed to the writing of the manuscript and the review of relevant related work. Both authors approved the final version of the manuscript and agreed with the order of presentation of the authors.

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The Neural Correlates of Effortful Cognitive Processing Deficits in Schizophrenia: An ERP Study

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Background: Individuals' information processing includes automatic and effortful processes and the latter require sustained concentration or attention and larger amounts of cognitive "capacity." Event-related potentials (ERPs) reflect all neural activities that are related to a certain stimulus. Investigating ERP characteristics of effortful cognitive processing in people with schizophrenia would be helpful in further understanding the neural mechanism of schizophrenia.

Methods: Both schizophrenia patients (SCZ, n = 33) and health controls (HC, n = 33) completed ERP measurements during the performance of the basic facial emotion identification test (BFEIT) and the face-vignette task (FVT). Data of ERP components (N100, P200, and N250), BFEIT and FVT performances were analyzed.

Results: Schizophrenia patients' accuracies of face emotion detection in the BFEIT and vignette emotion detection in the FVT were both significantly worse than the performance of the HC group. Repeated-measures ANOVAs performed on mean amplitudes and latencies revealed that the interaction effect for group × experiment × site (prefrontal, frontal, central, parietal, and occipital site) was significant for N250 amplitude. In FVT experiment, N250 amplitudes at prefrontal and frontal sites in schizophrenia group were larger than those of HC group; the maximum N250 amplitude was present at the prefrontal site in both the groups. For N250 latency, the interaction effect for group × experiment was significant; N250 latencies in the schizophrenia group were longer than those of the HC group.

Conclusion: Schizophrenia patients present effortful cognitive processing dysfunctions which reflect in abnormal ERP components, especially N250 at prefrontal cortex and frontal cortex sites. These findings have important implications for further clarifying the neural mechanism of effortful cognitive processing deficits in schizophrenia.

Keywords: schizophrenia, the face-vignette task, effortful cognitive processing, event-related potential, frontal cortex site, prefrontal cortex site

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INTRODUCTION

Schizophrenia protect is a severe mental disorder characterized by destruction of thinking, sense of self, emotional response, logical reasoning, cognition, perceptions and volitional behavior. Among all the major symptoms of schizophrenia, cognitive dysfunction authentically contributes to the disabling nature of the disorder (Bowie et al., 2006; Stern, 2012). Systematic studies have confirmed that cognition is an individual's process of obtaining and applying knowledge, that is, a process of processing information (Tomasello and Call, 2011). Individuals' information processing includes automatic processes and effortful processes. Automatic cognition processing requires almost no attention to be allocated to the task at hand and in many instances is executed in response to a specific stimulus. Correspondingly, effortful cognition, namely as effortful information processing, depends on attentional capacity and usually can be tested by an "effort-demanding" cognitive task (i.e., tasks that require sustained concentration or attention or require larger amounts of cognitive "capacity") (Tancer et al., 1990; Hammar and Ardal, 2012; Rodriguez-Larios et al., 2020).

The vulnerability model of schizophrenia was derived from the integration of heredity, abnormal brain structure, impaired brain functioning, physiological and psychological development and early learning (Yank et al., 1993). Reduced availability of processing resources is the critical feature of the vulnerability model of schizophrenia (Rund and Landrø, 1990). Studies have indicated that schizophrenia patients also experience severe deficiencies in resources availability for effortful controlled processing (i.e., effortful cognitive dysfunction) (Granholm et al., 2007; Patrick et al., 2015). Previously a study investigated the relationship between early visual information processing deficits and effortful processing resource allocation (Granholm et al., 2007). In this study, both chronic schizophrenia patients and healthy controls (HCs) performed a span of apprehensive task, and pupillary responses were recorded as an index of resource allocation or mental effort during the task. Their results revealed that patients displayed reduced effortful resource allocation and impaired detection accuracy. Another study compared the effortful decision making of schizophrenia patients versus HCs by focusing on the effort expended when completing a rewards task; the findings also revealed a pattern of inefficient effortful decision making in schizophrenia patients relative to HCs (McCarthy et al., 2016).

Effortful cognitive processing was involved in both social cognition and neurocognition. For example, effortful emotional cognitive processing is a kind of effortful cognition that may supply top-down as well as goal-directed reappraisal of emotion-laden stimuli to contextually modulate affect-driven responding (Phillips et al., 2003; Ochsner and Gross, 2005). The essential characteristic of effortful emotional cognition is attributable to either social cognition or neurocognition. Effortful emotional cognition can be measured by a face-vignette task (FVT), which is an information-processing task with a high processing load, and individuals' performances on FVT represent their available processing resources (Patrick et al., 2015). Previous studies have confirmed that patients with schizophrenia exhibit impairments in effortful emotional processing (Rowland et al., 2012; Patrick et al., 2015). Cognitive deficits, one of the major symptom dimensions occurring early in the stage of schizophrenia and relatively stable over time, have been considered as a potential treatment target (Rowland et al., 2012; MacKenzie et al., 2018).

Event-related potentials (ERPs), based on electroencephalogram (EEG) recordings, are electrophysiological responses reflecting cognition-related neural activities that can be evoked by certain stimuli during an experiment. ERPs have become a prominent approach for studying the neural mechanisms involved in cognitive function. To date, although previous studies have suggested that schizophrenia patients have impaired effortful cognitive processing, the ERP characteristics of effortful cognitive processing in schizophrenia have not been reported yet, although it would be of help to understand the neural mechanism of this disease. Furthermore, clarifying such ERP characteristics would have implications for better understanding the etiology, clinical features and treatment strategies in schizophrenia.

On the basis of the foregoing, it is supposed that people with schizophrenia would have abnormal ERP responses due to effortful emotional processing deficits. To test this hypothesis and investigate the neural mechanism of effortful cognitive processing in schizophrenia, both schizophrenia patients and HCs performed a Basic facial emotion identification test (BFEIT) and a FVT, to reflect their automatic cognitive function and effortful cognitive function, respectively. ERPs evoked by BFEIT and FVT were synchronously measured.

MATERIALS AND METHODS

Time and Setting

This study was conducted from January 01, 2017, to July 31, 2020 in the affiliated Wuxi Mental Health Center of Nanjing Medical University. The study protocol was approved by the Ethics Committee of Wuxi Mental Health Center and was conducted in accordance with the Declaration of Helsinki (Reference No. 2017LLKY007).

Participants

All participants with schizophrenia were recruited from inpatients of Wuxi Mental Health Center. The inclusion criteria were: (a) met the criteria of schizophrenia according to the Diagnostic and Statistical Manual of Mental Disorders, Fifth edition (DSM-5), (b) Chinese Han, aged 18 to 65 years, (c) had no current or history of neurological illness or any other kind of severe physical illness that would affect his/her cognitive function, (d) educational level no less than junior middle school (normally 9 years), (e) volunteer to participate in this study, and (f) clinical stable enough to fulfill this study. The exclusion criteria were: (a) met criteria of any other mental disorder according to DSM-5, (b) treated by electroconvulsive therapy

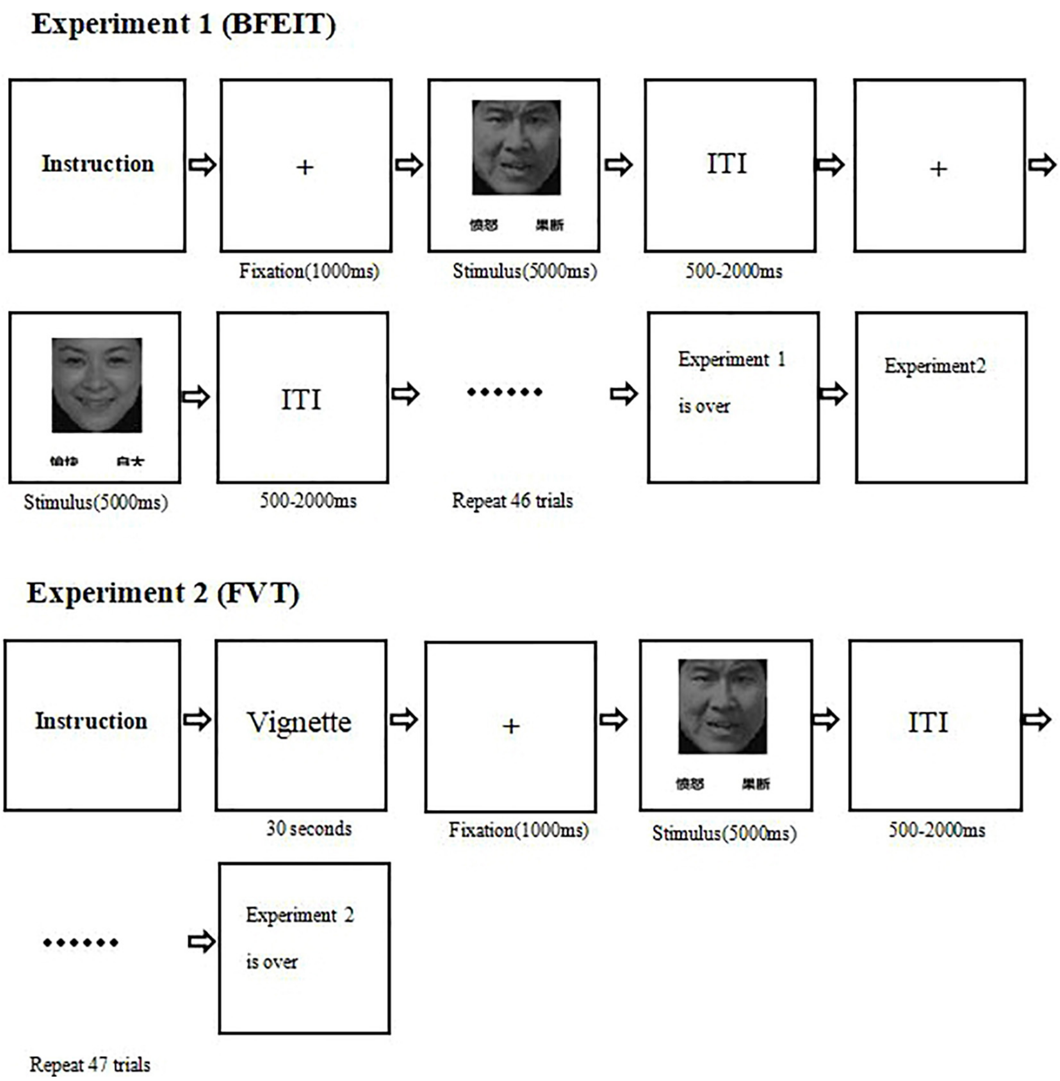

FIGURE 1 | A diagram illustrating the experimental procedures (experiment 1: the measurement of the basic facial emotion identification test; experiment 2: the measurement of the face-vignette task). 愤怒, angry; 果断: determined; 愉快, happy; 自大, smugness. ITI, inter-trial interval.

(ECT) or modified ECT within 6 months before recruitment, (c) had nicotine/other substance misuse or dependence within the latest 6 months, and (d) visual impairment that cannot be corrected to satisfy the demand of the current study. Healthy controls were recruited from local citizen through advertisement. The inclusion criteria were: (a) met no criteria of any kind of mental disorder according to DSM-5, and (b) to (e) criteria as the schizophrenia patients group. The exclusion criteria of HCs were the same as (c) and (d) items as the patients group.

All participants provided written informed consent. Considering that some schizophrenia patients might have impaired capacity to provide consent even if they are clinically stable, we also got consent from patients' guardians or close relatives. All participants were compensated 300.00 Chinese Yuan (equals to about 45 US dollars) for their time taking part in this study.

Measurements

Basic Facial Emotion Identification Test

In the BFEIT task, there were six categories of basic emotions (i.e., happy, angry, sad, fear, surprise, and disgust) which were selected from the Chinese facial affective picture system (Gong et al., 2011). All pictures were black-and-white photographs with hair removed. Each category of facial emotion types included 8 pictures and the number of male and female face pictures were balanced across each emotion category. The procedure of BFEIT is sketched in **Figure 1**. Briefly, in each trial, following a centrally presented fixation (" + ", 1.0 × 1.0 cm, last for 1000 ms), there was a facial picture with two choices below. One choice represented the basic emotion matched to the face picture and the other one conveyed a non-basic emotion (i.e., guilty, smugness, painful, determined, hopeful and insulted). The position (bottom left or bottom right) of the two choices were pseudo-random. Participants were required to make a choice to classify the emotion represented by the picture as quickly and accurately as possible, with a response deadline of 5000 ms, by pressing a labeled keypad. The next trial started following an inter-trial interval (ITI) with a range varied randomly from 500 to 2000 ms.

Face-Vignette Task

The procedure of FVT was designed similarly as described in a previous study (Patrick et al., 2015). A succinct situational vignette was constructed, for each face picture, to convey an above mentioned non-basic emotion. Before the test in participants of this study, the intended emotion for each vignette was validated by seven Chinese undergraduates. The mean accuracy was 0.91 [standard deviation (SD) = 0.03], and the observed inter-rater reliability was 0.75. According to the specific emotional category (e.g., a fearful facial expression paired with a painful vignette), the face-vignette pairs were matched such that each vignette was inconsistent with the facial expression (see **Figure 2**). The specific face-vignette pairs included sadness-guilty, happy-smug, fear-painful, angry-determined, disgusted-insulted and surprised-hopeful. During the FVT trial, the vignette was presented first and the participants were required to read it aloud. After the participants well understood the vignette, he/she could press the "SPACE" of a keyboard to continue. Then a fixation presented in the center of the screen for 1000 ms followed by a face picture. Be different from the BFEIT, participants were required to choose the emotion that best matching the vignette by pressing a labeled keypad (include three items, i.e., face responses, vignette responses and random responses). The procedure of FVT is also sketched in **Figure 1**.

Both BFEIT and FVT were designed and performed via E-Prime software version 3.0 (Psychology Software Tools Inc, Sharpsburg, PA, United States). All stimuli were presented on a 19-inch computer screen with 1280 × 1024 resolution and a 60 hertz (Hz) refresh rate. Participants were seated in a moderate light and sound attenuated room in front of the screen at a distance of around 60 cm. Participants were asked to complete BFEIT (experiment 1) and then FVT (experiment 2). Before the formal trial, there was a practice procedure (12 trials) to ensure that participants understood the task.

Electroencephalogram Recordings and ERP Analysis

The BioSemi Active Two system was employed to record EEGs in continuous mode at a 500 Hz digitization rate. According to the international 10/20 system, a customized BrainCap (EasyCap, Herrsching, Germany) containing 64 Ag/AgCl ring-type electrodes was arranged for EEG recordings. Vertical and horizontal electrooculography (EOG) was monitored by additional electrodes placed below and on the external canthi of the left eye. Electrode impedances were below 5 kiloohm (kΩ). The EEG and EOG were filtered by 0.05–100 Hz bandpass filter. The left and right mastoids served as references, and the ground electrodes were placed under the left clavicle site. ERPs were only derived from participants' correct responses.

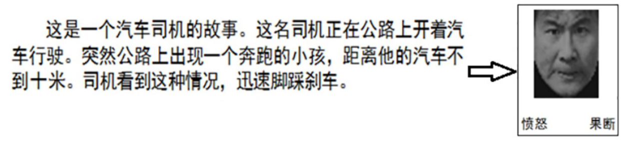

FIGURE 2 | An example of a trial of the FVT. The situational vignette is displayed as follows (in Chinese): This is a story about a bus driver. The driver is driving a bus on the road. Suddenly, he sees a running child on the road no more than 10 m ahead of his bus. He quickly steps on the brakes. Followed by the vignette, there appears a picture of a face on the upper and two choices (angry vs. determined) on the lower position. Subjects are required to choose the better one that reflects the given scenario. 愤怒, angry; 果断: determined.

The EEG data were processed offline using Brain Vision Analyzer 2.0 (Brain Products GmbH, Munich, Germany) and re-referenced offline to the averaged left and right mastoids. A bandpass filtered between 0.1 and 30 Hz using a zero-phase shift Butterworth filter was used. In each single trial, the EOG was eliminated by the independent component analysis (ICA) algorithm. Data were segmented by stimulus marker from −200 to 1000 ms. Segments were baseline corrected using −200 to 0 ms prestimulus time and were eyeblink corrected using established measures (Monaghan et al., 2019). Artifact rejection for individual channels was performed, and a given segment was rejected if the voltage gradient exceeded 50 microvolts (μV)/ms, the amplitude was ±75 μV, or the signal was flat (<0.5 μV for more than 100 ms). Segments were averaged across stimulus markers. The time-windows locked in each peak were selected.

Grand average ERP responses to target stimuli in experiment 1 and experiment 2 were computed across all participants, and three distinct ERP components, i.e., N100, P200, and N250, were identified and used for statistical analyses based on their distinctive polarities, latencies and topographic maps. In this study, following the stimulus onset, N100 data were measured in a time window between 80 and 150 ms; P200 data were measured in a time window between 150 and 230 ms; and N250 data were measured in a time window between 200 and 350 ms.

According to the scalp topographical distribution of grand-averaged ERP activity in this study, a set of available electrodes was used for statistical analyses. Nineteen electrode sites (AF3, AFz, AF4, F1, F2, Fz, F3, F4, C1, C2, Cz, C3, C4, P1, Pz, P2, PO3, PO4, and POz) were selected and classified into the following five regions of sites: prefrontal site (AF3, AFz, and AF4), frontal site (F1, F2, Fz, F3, and F4), central site (C1, C2, Cz, C3, and C4), parietal site (P1, Pz, and P2), and occipital site (PO3, POz, and PO4). The peak amplitude and corresponding latency of each ERP component and electrode site were measured. And then the average amplitude and latency for each of the five brain regions were calculated.

Statistical Analysis

All data were analyzed using Statistical Program for Social Sciences software version 22.0 (SPSS, IBM Corp., United States). Quantitative data were compared between the two groups

TABLE 1 | Demographic characteristics and clinical information of participants [mean (SD)].

Variable	Schizophrenia patients (n = 33)	HCs (n = 33)	Test statistic
Age (year)	33.8 (7.7)	32.6 (5.8)	t = 0.742, p = 0.461
Age range	19–44	22–45	–
Sex (M/F)	20/13	21/12	χ^2 = 0.064, p = 0.800
Education (years)	13.3 (2.8)	14.3 (1.9)	t = 1.712, p = 0. 920
PANSS-Tot scores	63.8 (16.3)	–	–
PANSS-Pos	15.8 (5.8)	–	–
PANSS-Neg	15.1 (5.0)	–	–
PANSS-Gen	32.8 (8.2)	–	–
Handedness (R/M/L)	12/10/11	13/10/10	χ^2 = 0.195, p = 0.901
Duration of illness	11.2 (7.3)	–	–
Medicine(A/C/O/R)	6/10/9/8		

HC, healthy control; SD, standard deviation; R, right, M, mixed, L, left; PANSS, Positive and Negative Syndrome Scale; Tot, total scores; Pos, positive symptoms factor scores; Neg, negative symptoms factor scores; Gen, general symptoms factor scores. Medicine (A: amisulpride C: clozapine O: olanzapine, R: risperidone).

by independent t tests (two-tailed) and quantitative data by Pearson chi-square test. The amplitudes and the latencies of ERP components (N100, P200, and N250) in each brain regions (prefrontal, frontal, central, parietal and occipital sites) were compared between the schizophrenia group and the HC group using a 2 (group, schizophrenia vs. HCs) × 2 (experiment, BFEIT vs. FVT) × 5 (brain region, prefrontal vs. frontal vs. central vs. parietal vs. occipital) repeated-measures analysis of variance (ANOVA). Effect sizes were estimated using η^2 and Cohen's d. The degrees of freedom of the F ratio were corrected using the Greenhouse-Geisser method. Least square difference (LSD) tests were performed as *post hoc* analyses if needed. The Pearson's correlation analysis was conducted between the amplitudes and latencies of N250 and the PANSS scores respectively. Alpha values of 0.05 were considered significant.

RESULTS

Demographic Characteristics of Participants

In line with the inclusion and exclusion criteria, data of 33 schizophrenia patients and 33 HCs were retained for analysis after ruling out those incomplete or low-quality data. The demographic characteristics and clinical information of them are shown in **Table 1**. There were no significant differences in mean age, educational level, duration of illness, handedness and male-female ratio between the two groups. For patients, the mean lurasidone-equivalent dose was 75.8 ± 4.1 mg/d as calculated according to the previous report (Ng-Mak et al., 2019).

Comparisons of BFEIT and FVT Performance Accuracy Between the Two Groups

For BFEIT, independent sample t test results indicated that there was significant difference in emotion identification accuracy between the schizophrenia group (Mean = 69.3%, SD = 11.8%) and the HC group (Mean = 77.1%, SD = 12.4%) (t = 2.629, p = 0.011). For FVT, significant difference was found in correct vignette response proportions between the schizophrenia patients (face response proportions: Mean = 29.9% SD = 13.8%; vignette response proportions: Mean = 67.9% SD = 14.1%) and the HCs (face response proportions: Mean = 22.6%, SD = 8.9%; vignette response proportions: Mean = 75.3%, SD = 8.9%) (t = 2.546, 2.659; p = 0.013, 0.008). However, there were no significant differences in random response proportions between the schizophrenia group (Mean = 2.2%, SD = 0.1%) and the HC group (Mean = 2.1%, SD = 0.1%) (t = 1.231, p = 0.168). The performance of the HC group was much better than that of the schizophrenia group.

Comparisons of RTs Between the Schizophrenia Group and the HC Group

For BFEIT, there were significant differences in RTs for target stimuli between the schizophrenia group (Mean = 750.3 ms, SD = 146.2 ms) and the HC group (Mean = 609.5 ms, SD = 102.8 ms) (t = 4.531, p = 0.000). For FVT, similar differences were found between the two groups. The average RTs for target stimuli of schizophrenia patients (Mean = 808.7 ms, SD = 97.7 ms) was much longer than that of the HC group (Mean = 621.1 ms, SD = 66.1 ms) (t = 9.138, p = 0.000).

ERP Data Analysis

For BFEIT, the average number of trials for ERP analyzing were 32.34 ± 5.44 in the schizophrenia group and 36.02 ± 5.87 in the HC group. For FVT, the average number of trials for ERP analyzing were 31.75 ± 6.59 in the schizophrenia group and 35.73 ± 4.46 in the HC group.

Using N100, P200 and N250 as dependent variables, a 2 × 2 × 5 repeated-measures ANOVA was performed on mean amplitudes and mean latencies, respectively, with the group (schizophrenia group vs. HC group) as the between-subjects factor and the experiment (experiment 1 vs. experiment 2) and site (prefrontal, frontal, central, parietal, and occipital) as the within-subjects factor.

N100 Component

As shown in **Figures 3**, **6A–D**, for N100 amplitude, the three following interaction effects involving the factor "group" failed to reach significance: group × experiment × site ($F_{1,\ 78}$ = 0.025, p = 0.914, $\eta 2$ = 0.000); group × experiment ($F_{1,64}$ = 0.025, p = 0.174, $\eta 2$ = 0.029); and group × site($F_{1,83}$ = 0.475, p = 0.542, $\eta 2$ = 0.007).

For N100 latency, the three following interaction effects involving the factor "group" also failed to reach significance: group × experiment × site ($F_{2,135}$ = 2.215, p = 0.110, $\eta 2$ = 0.033);

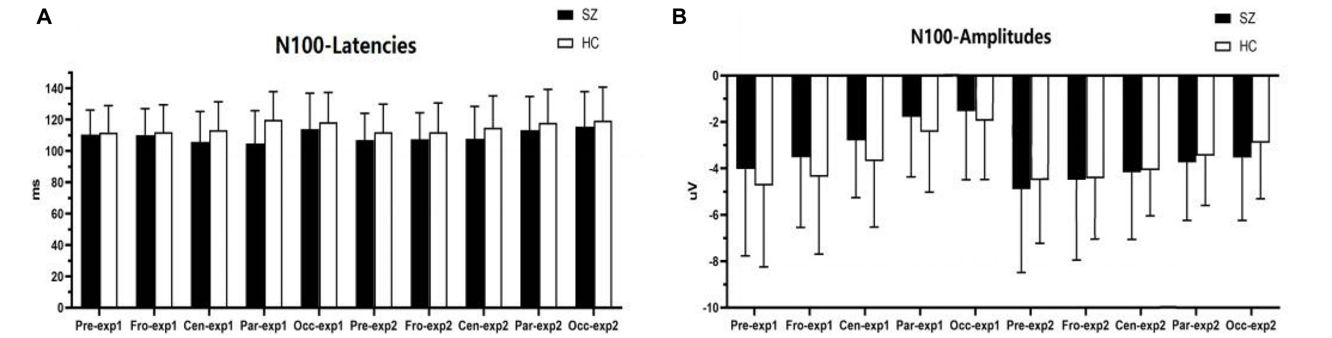

FIGURE 3 | Comparisons of N100 latencies **(A)** and amplitudes **(B)** between the schizophrenia group and HC group within ROIs and experiments. SZ, schizophrenia; HC, healthy control; exp, experiment; Pre, prefrontal, Fro, frontal, Cen, central, Par, parietal, Occ, occipital.

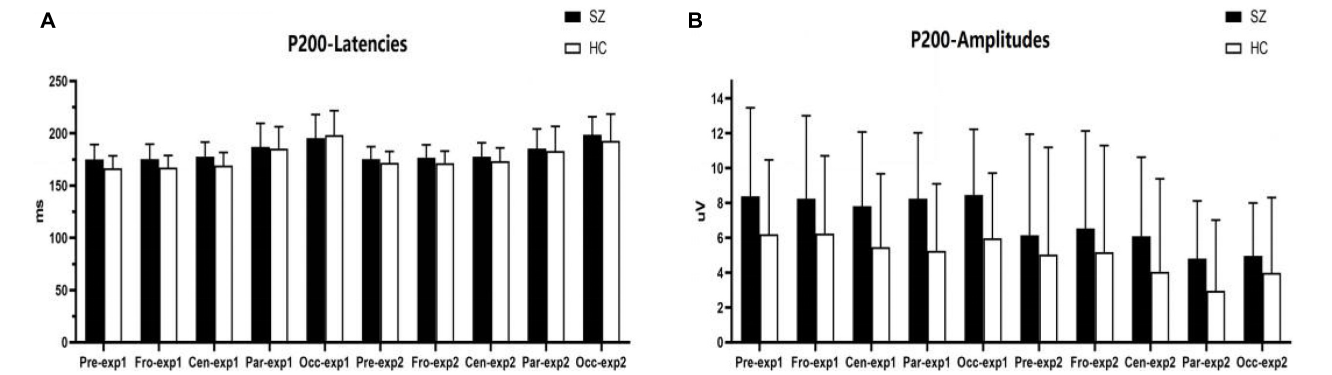

FIGURE 4 | Comparisons of P200 latencies **(A)** and amplitudes **(B)** between the schizophrenia group and HC group within sites and experiments. SZ, schizophrenia; HC, healthy control; exp, experiment; Pre, prefrontal, Fro, frontal, Cen, central, Par, parietal, Occ, occipital.

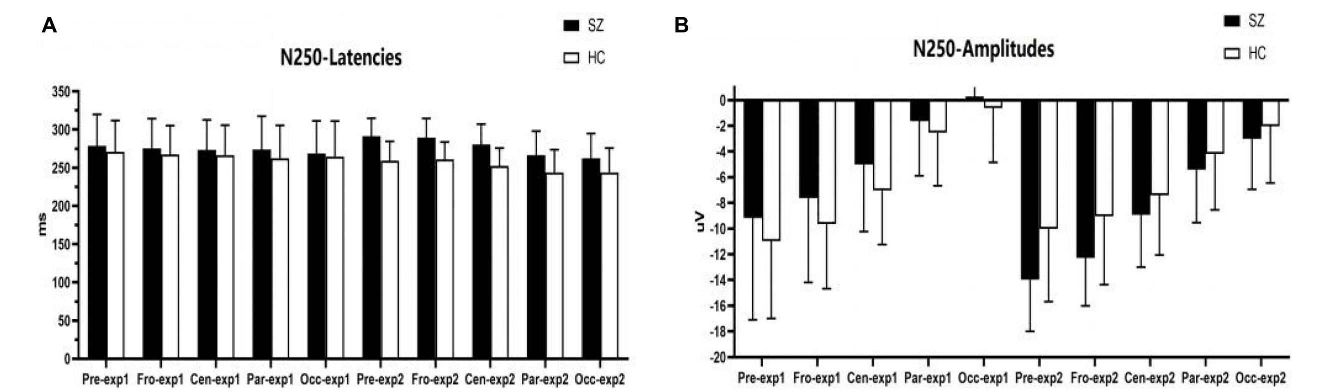

FIGURE 5 | Comparisons of N250 latencies **(A)** and amplitudes **(B)** between the schizophrenia group and HC group within sites and experiments. SZ, schizophrenia; HC, healthy control; exp, experiment; Pre, prefrontal, Fro, frontal, Cen, central, Par, parietal, Occ, occipital.

group × experiment ($F_{1,64} = 0.053$, $p = 0.818$, $\eta 2 = 0.001$); and group × site ($F_{2,108} = 1.113$, $p = 0.324$, $\eta 2 = 0.017$).

P200 Component

As shown in **Figures 4**, **6A–C,E**, for P200 amplitudes, the three following interaction effects involving the factor "group" failed to reach significance: group × experiment × site ($F_{1,95} = 0.791$, $p = 0.422$, $\eta 2 = 0.012$), group × experiment ($F_{1,64} = 0.925$, $p = 0.340$, $\eta 2 = 0.014$), and group × site ($F_{1,82} = 0.437$, $p = 0.558$, $\eta 2 = 0.007$).

For P200 latency, the three following interaction effects involving the factor "group" failed to reach significance: group × experiment × site ($F_{2,128} = 2.666$, $p = 0.073$, $\eta 2 = 0.040$), the interaction effect for group × experiment ($F_{1,64} = 0.016$,

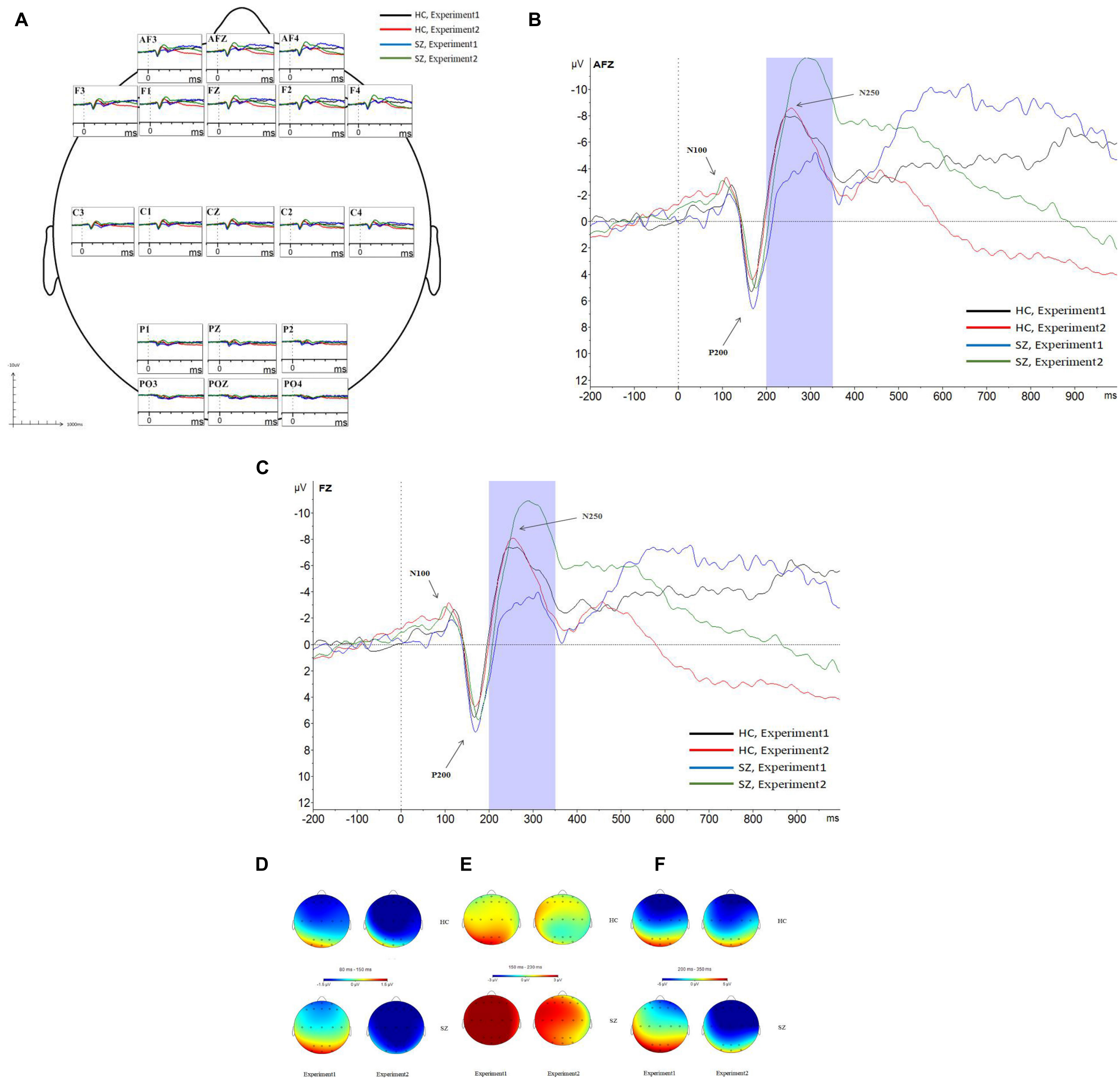

FIGURE 6 | Grand average event-related potentials (ERPs) at the AF3, AFz, AF4, F1, F2, Fz, F3, F4, C1, C2, Cz, C3, C4, P1, Pz, P2, PO3, PO4, and POz sites for all participants during experiment 1 and experiment 2 **(A)**. Following stimulus onset, the N100 component was measured in a time window within 80–150 ms, the P200 component was measured in a time window within 150–230 ms, and the N250 component was measured in a time window within 200–350 ms. In experiment 2, N250 amplitudes at the prefrontal site and frontal site in the SZ group were larger than those of the HC group; N250 latencies in the SZ group were longer than those of the HC group **(B,C)**. Topographic distributions of the N100 components **(D)**, P200 components **(E)**, and N250 components **(F)** in the SZ group and HC group. SZ, schizophrenia; HC, healthy control.

$p = 0.901$, $\eta 2 = 0.000$), and group × site was not significant ($F_{1,95} = 0.717$, $p = 0.451$, $\eta 2 = 0.011$).

N250 Component

As shown in **Figures 5**, **6A–C,F**, for N250 amplitudes, the interaction effect for group × experiment × site was significant ($F_{1,87} = 5.992$, $p = 0.009$, $\eta 2 = 0.086$).

In experiment 1, the interaction effect for group × site was not significant ($F_{1,83} = 0.612$, $p = 0.476$, $\eta 2 = 0.009$), and the main effect for site was significant ($F_{1,83} = 127.404$, $p = 0.000$, $\eta 2 = 0.666$); however, the main effect for group was not significant ($F_{1,64} = 1.910$, $p = 0.172$, $\eta 2 = 0.029$). There were no significant differences in N250 amplitudes between the schizophrenia group and the HC group in experiment 1, and the maximum N250 amplitude was observed in the prefrontal site.

In experiment 2, the interaction effect for group × site was significant ($F_{1,83} = 4.018$, $p = 0.038$, $\eta 2 = 0.059$). The simple effect for the group within sites was significant ($F_{1,64} = 10.674$, $p = 0.002$, $\eta 2 = 0.143$). N250 amplitudes at the prefrontal site and frontal site in the schizophrenia group were larger than

TABLE 2 | Correlations between N250 and PANSS.

	N250 amplitudes		N250 latencies	
	AFZ	FZ	AFZ	FZ
PANSS-Tot	*r* 0.061	0.020	0.199	0.123
	p 0.737	0.910	0.267	0.494
PANSS-Pos	*r* 0.193	0.145	0.242	0.196
	p 0.281	0.422	0.174	0.275
PANSS-Neg	*r* –0.080	–0.119	–0.038	–0.148
	p 0.657	0.508	0.833	0.410
PANSS-Gen	*r* 0.033	0.011	0.247	0.198
	p 0.583	0.950	0.165	0.270

PANSS, Positive and Negative Syndrome Scale; Tot, total scores; Pos, positive symptoms factor scores; Neg, negative symptoms factor scores; Gen, general symptoms factor scores.

those of the HC group ($F_{1,64}$ = 10.647, 8.064; p = 0.002, 0.006; η^2 = 0.143, 0.112), and no differences were observed in other sites. The simple effect for site within groups was significant (for schizophrenia, $F_{3,62}$ = 42.867, p = 0.000, η^2 = 0.675; for HC group, $F_{3,62}$ = 23.664, p = 0.000, $\eta2$ = 0.534). The maximum N250 amplitude was noted at the prefrontal site in both groups.

For N250 latency, the interaction effect for group × experiment × site was not significant ($F_{2,114}$ = 0.613, p = 0.053, $\eta2$ = 0.090), and the interaction effect for group × site was not significant ($F_{2,97}$ = 0.570, p = 0.521, $\eta2$ = 0.009); however, the interaction effect for group × experiment was significant ($F_{1,64}$ = 4.228, p = 0.044, $\eta2$ = 0.062). In experiment 1, the simple effect for the group within sites was not significant ($F_{1,64}$ = 0.798, p = 0.375, $\eta2$ = 0.012). In experiment 2, the simple effect for the group within sites was significant ($F_{1,64}$ = 23.582, p = 0.000, $\eta2$ = 0.269); N250 latencies in the schizophrenia group were longer than those in the HC group.

Correlation Analysis Between N250 and PANSS Scores

The Pearson's correlation analysis was conducted between the amplitudes and latencies of N250 at AFz and Fz electrode sites and the PANSS scores, respectively. As shown in **Table 2**, no significant correlation was found between any two parameters.

DISCUSSION

This study is the first to investigate the ERP characteristics of effortful cognitive processing in schizophrenia using a face-vignette task. In this study, the capacity of effortful cognitive processing was determined by the ability of schizophrenia patients to apply contextual information when judging the meaning of facial expressions, while the capacity of automatic cognitive processing was determined by the sample emotion identification task. Our findings showed that the emotion identification accuracy of normal controls was higher than that of the schizophrenia patients in sample emotion identification task; however, schizophrenia patients exhibited poor face-vignette task performance, i.e., the face response proportions of the normal controls were lower than those of the schizophrenia patients, and the vignette response proportions of the normal controls were higher than those of the schizophrenia patients. Most importantly, we found that N250, which was evoked by target stimuli in the face-vignette task and basic facial emotion identification test, was responsible for the ERP characteristics of effortful cognitive processing in participants. N250 amplitudes at the prefrontal site and frontal site in the schizophrenia group were larger than those in the HC group, and N250 latencies in the schizophrenia group were longer than those in the HC group.

This study mainly focused on the investigations of effortful information processing using the effortful cognitive task, and we did not explore the effect of the emotional on individual's cognition. The BFEIT and FVT only reflected automatic information processing and effortful information processing respectively. In our study, the procedure of the FVT mainly included vignettes describing situational information that was discrepant in affective valence prior to target facial expressions; this resulted in appraisals of emotional attributes reflecting the dominance of either the facial expression or the emotional context. Many studies on effortful cognitive processing have reported that schizophrenia patients present impairments in effortful emotional processing (Rowland et al., 2012; Patrick et al., 2015). Additionally, results of a study suggested that the impaired motivational drive in patients with schizophrenia may be at least partly due to a decreased effort-expenditure for greater rewards (Barch et al., 2014). Consistent with the findings of the above studies, this study confirmed that schizophrenia patients cannot perfectly utilize contextual information for specific story-face pairs, whereas normal controls more commonly made good judgments on the contextual information, which showed that schizophrenia patients present effortful cognitive impairment.

Many studies have indicated that ERP amplitudes, which are evoked by different complexity levels of tasks, reflect electrophysiological measurements of the mental workload (Horat et al., 2016; Shaw et al., 2018), whereas ERP latencies reflect the brain cognitive processing speed for the target stimulus onset (Mcarthur and Bishop, 2002; Tsai et al., 2012). N250 is an ERP component that has been studied in relation to face emotion processing in schizophrenia. Previous studies showed that abnormal N250 is related to the impaired face emotion processing in schizophrenia (Lee et al., 2010; Wynn et al., 2013; McCleery et al., 2015). Additionally, individuals at risk for schizophrenia showed significant impairments in facial affect recognition and reduced amplitudes in the N250 component (Wolfgang et al., 2012), and Positive and Negative Syndrome Scale (PANSS) scores of schizophrenia patients are related to N250 component (Kim et al., 2013). However, studies which investigated the characteristics of ERPs induced by facial emotion recognition displayed that schizophrenia patients did not show N250 abnormalities (Wynn et al., 2013; Yang et al., 2017). Our results showed that N250 amplitudes in schizophrenia patients were larger than those of HCs and N250 latencies in schizophrenia patients were longer than those of HCs, which might suggest that schizophrenia patients experienced an increased mental workload and slowed processing speed due to effortful information processing deficits. The characteristic

ERP component N250 triggered by the target stimulus may be a valuable marker for studying schizophrenia.

In our study, the PANSS scores were not correlated with N250 amplitudes and latencies, which might indicate that abnormal N250 are not state dependent in schizophrenia. We assumed that larger N250 amplitude reflected more substantial increases in the cognitive resources allocated to the processing of the effortful cognitive task. Our findings also confirm that neural correlates of effortful cognitive processing deficits in Schizophrenia because of the abnormal N250 components at the prefrontal and frontal sites. Namely, a high load on "prefrontal and frontal site" cognitive processes led to a larger N250 amplitude.

Because no differences were observed between schizophrenia patients and HCs in the ERP component N250, which was triggered by the target stimulus in BFEIT (i.e., the picture of a face in the BFEIT) onset, all participants used a similar amount of cognitive resources. However, our findings for ERP characteristics in the BFEIT and FVT might be due to differences in task demands, i.e., automatic cognition processing requires near zero attention for the task, whereas effortful processes use attentional capacity; the mental workload was higher in the FVT than in the BFEIT.

CONCLUSION

Schizophrenia patients present effortful cognitive processing dysfunctions, and effortful cognitive processing deficits show abnormal ERP responses at prefrontal cortex and frontal cortex sites.

LIMITATIONS

There are several limitations in the present study. Firstly, participants' effortful cognitive processing might be influenced by the working memory since the vignettes and face pictures were presented sequentially. It's worthwhile to establish better experimental paradigm that can address this problem and further explore the effortful cognitive processing characteristics. Secondly, the schizophrenia patients in this study were clinical stable ones; therefore, whether the results can reflect situations of patients in other disease status remains unclear. Thirdly, because of the small sample sizes, the findings must be considered as preliminary. Further studies with larger sample sizes and with the same ERP parameters are needed to further verify the findings of the study. Finally, because of the deficient spatial resolution of ERPs, further studies with functional magnetic resonance imaging or magnetoencephalography should be conducted to confirm the underlying brain generators, as presented by an abnormality in ERP response, which may further clarify the neural mechanism of effortful cognitive processing deficits in schizophrenia.

DATA AVAILABILITY STATEMENT

The datasets generated for this study are available on request to the corresponding author.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by the Ethics Committee on Human Studies, the Affiliated Wuxi Mental Health Center of Nanjing Medical University, Wuxi, Jiangsu Province, China. The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

C-GJ and Z-HZ wrote the manuscript. C-GJ and JW performed the BFEIT and FVT data analysis and statistics. C-GJ, X-HL, Y-LX, and JW oversaw the ERP data/demographic data collection. C-GJ and Z-HZ analyzed the ERP data. Z-HZ was in charge of the design and implementation of the study and contributed to the data interpretation. All authors contributed to the article and approved the submitted version.

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Elites Do Not Deplete – No Effect of Prior Mental Exertion on Subsequent Shooting Performance in Elite Shooters

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In order to perform at the highest level, elite shooters have to remain focused during the whole course of a tournament, which regularly lasts multiple hours. Investing self-control over extended time periods is often associated with lower levels of perceived self-control strength (i.e., the subjective estimation of how much mental effort one is capable of investing in a given task) and impaired performance in several sports-related domains. However, previous findings on the effects of prior self-control efforts on shooting performance have been mixed, as elite shooters seem to be less affected by preceding self-control demanding tasks than sub-elite athletes. Therefore, the aim of the present study was to investigate the effects of self-control on shooting performance in elite shooters. Hence, we randomly assigned elite shooters to an experimental (n = 12) or a control condition (n = 11) and asked them to perform a series of 40 shots at baseline (T1) and again after a task which either did or did not require self-control (T2). Additionally, we continuously measured the shooters' level of perceived self-control strength. We assumed that in elite athletes, shooting accuracy as well as the perceived level of self-control strength would not be significantly affected over time from T1 to T2 in both conditions. In line with our assumptions, Bayesian linear mixed effect models revealed that shooting performance remained relatively stable in both conditions over time and the conditions also did not differ significantly in their perceived levels of self-control strength. Contrary to resource-based theories of self-control, these results speak against the idea of a limited self-control resource as previous acts of self-control did not impair subsequent shooting performance in elite athletes.

Keywords: self-control, self-regulation, ego depletion, fatigue, sports, mental effort

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INTRODUCTION

In professional shooting, elite athletes must perform at their highest levels during the whole course of a competition in order to be successful (e.g., Di Fronso et al., 2016). Shooting tournaments are often divided into preliminary, main, and finals, with each part lasting up to 3 h (Chen and Mordus, 2018). In order to win, athletes must consistently shoot with high accuracy as single outliers might result in a bad overall performance or even in missing the final round. Therefore, not only selectively controlling but also sustaining attention is highly relevant in shooting competitions (e.g., Ihalainen et al., 2016). Sustained attention is defined as "the ability to maintain attentional focus on relevant stimuli with repeated presentation over extended periods" (Williams and Saunders, 1997, p. 174). Research has shown that tasks that necessitate sustained attention (e.g., archery) are usually accompanied by a steady decrement in performance (e.g., Milstein et al., 2005). This decrement can also be observed in shooting (e.g., Tremayne and Barry, 2001; Kim et al., 2019). For this reason, it is crucial to understand the underlying psychological processes that influence performance (e.g., Laaksonen et al., 2018).

Volitionally controlling attention over extended periods of time requires self-control, as individuals have to inhibit themselves of paying attention to distracting stimuli and instead have to force their attention to the relevant stimuli (e.g., Pageaux and Lepers, 2018; André et al., 2019). For instance, during a shooting competition, an athlete has to shield his/her attention, ignore the crowd or internal thoughts, and focus on the task at hand in order to succeed (e.g., Abernethy et al., 2007; for an overview, see Englert, 2017, 2019). However, exerting control over the self does not always work (e.g., Head et al., 2017). Sticking to the previous example, it has been shown that basketball players are less adept at controlling their attention in high-pressure situations, leading to performance impairments (e.g., Wilson et al., 2009).

But why does something as crucial as self-control appear to fail sometimes and what determines whether or not it is applied effectively? A large body of theoretical and empirical work suggests that applying self-control is an effortful process (see, for example, Westbrook and Braver, 2015; Shenhav et al., 2017; Wolff et al., 2019; Wolff and Martarelli, 2020). Recent theorizing suggests that this sense of effort (Kurzban, 2016) serves as a signal to bias behavior away from further self-control demanding tasks (e.g., Wolff and Martarelli, 2020). Accordingly, self-control allocation can be understood as a subjective reward-based choice where an individual tries to maximize the expected value of applying self-control (Shenhav et al., 2013, 2016). This expected value of control (EVC) is computed by comparing control costs (e.g., getting fatigued while trying to stay focused on a lengthy shooting task) with the expected rewards (e.g., winning an Olympic medal in shooting) of a control-demanding action. If the costs outweigh the expected benefits, no (or not enough) self-control is applied, leading to task disengagement (or reduced performance). On the other hand, even with rising costs (e.g., due to a prior self-control task), people seem to be able to maintain a high level of performance if the task is rewarding enough for the attendant costs to not outweigh its value (Muraven and Slessareva, 2003). Indeed, a recent meta-analysis indicates that the detrimental effects of prior mental exertion are less severe when the subsequent exercise is one, the participants are used to voluntarily engage in any way (Giboin and Wolff, 2019). To illustrate, a recent study with shooters showed that self-reported self-control strength decreased over time in sub-elite athletes, whereas it did not meaningfully change in elite athletes over the course of a long series of shooting trials (Englert et al., 2020). Thus, across a series of self-control demanding shooting trials, the perceived costs of applying control were markedly lower in elite athletes. In addition, elite athletes did not display a drop in performance, whereas sub-elites' performance substantially deteriorated over the course of the shooting task. Attesting to the crucial role of self-control for shooting performance, lower levels of perceived self-control strength (i.e., the subjective estimation of how much mental effort one is capable of investing in a given task) prior to a shooting block were robustly linked to worse subsequent shooting performance for elites and sub-elites.

Taken together, these findings indicate that over the course of a self-control demanding shooting task, elites do not perceive their self-control to wane and their performance does not deteriorate. As a limitation to these findings, Englert et al. (2020) did not experimentally manipulate prior self-control exertion with a separate task that was performed before the shooting task, but simply monitored the temporal dynamics of shooting performance and self-control over the course of the shooting task. However, such a sequential two-task paradigm approach is the established design of choice to investigate causal effects of prior self-control exertion on a subsequent self-control demanding task (e.g., Englert, 2019). Here, we address this shortcoming by investigating the role of prior mental exertion in an unrelated primary self-control demanding task on subsequent shooting performance in a sample of elite shooters. Building on recent theorizing of self-control as a reward-based choice (Kurzban et al., 2013; Shenhav et al., 2013; Wolff and Martarelli, 2020) and empirical evidence suggesting that task-specific self-control costs (i.e., the self-control demands of shooting) do not cause performance to deteriorate over the course of this task (Englert et al., 2020), we expected neither elite athletes' perceived self-control levels nor their shooting performance to be affected by prior mental exertion.

One reason for this lack of impairments in performance and perceived self-control might be that elite athletes process self-control demands more efficiently (Wolff et al., in press) or are better at applying self-control in general (Martin et al., 2016; Wolff et al., 2019). So, a prior self-control task would not be perceived as self-control demanding and in turn would not affect subsequent shooting performance. Another explanation would be that elite athletes experience prior mental exertion in an unrelated primary task as costly. However, these costs must not necessarily carry over into the EVC calculation of the secondary task. After all, this is the task they enjoy to do (high value) and are extremely proficient at doing (low task-specific self-control costs). The first explanation would be supported if primary self-control tasks of different difficulty are not perceived to differ in the self-control demands they impose. The second explanation would be supported if the primary task creates differences in perceived self-control costs, which do not translate into altered perceptions of self-control strength (and as a consequence, altered shooting performance) in the secondary task. This second research question is tested exploratively in this paper.

The current study aims at extending Englert et al.'s (2020) findings, by investigating the causal effects of a self-control demanding task on subsequent shooting performance and perceived self-control strength.

MATERIALS AND METHODS

Participants

A total of 23 elite shooters volunteered to participate in the present study (11 women and 12 men; $M_{age} = 19.43$, $SD_{age} = 4.11$; shooting experience: $M = 6.03$ years, $SD = 3.69$; training per week: $M = 164.32$ min, $SD = 63.63$; all participants were native German speakers; see also **Table 1**). Each participant was a member of the National Training Centre of Baden-Württemberg, Germany. Only the best shooters of Germany are recruited as members of the National Training Centre, delivering evidence for their high levels of expertise. Nine participants were primarily air gun shooters (10 m standard distance), and 14 participants were primarily small-caliber rifle shooters (50 m standard distance). In regard to sample size and the data analytic strategy, we followed the approach and the power simulations that had been found sufficient in Englert et al. (2020). Here, as per definition, a power analysis was performed as such: We simulated plausible data samples with different numbers of subjects according to prior assumptions regarding the effect size in the shooting scenario. On each of these samples, we fitted a model that could answer our question of interest. We then calculated the frequency of detection of group difference by the model. If the detection rate was above 80% (i.e., power of 0.8), the number of subjects was deemed adequate. Participants were asked to not consume caffeine, alcohol, or nicotine up to 24 h before the testing session and to eat a healthy meal up to 1 h before taking part in the study. The participants were informed about the basic aims of the study but were blinded with respect to the specific study hypotheses. Before beginning the assessments, each participant gave written informed consent based on APA's ethics code. The full data set can be found at https://osf.io/7bdh6/.

TABLE 1 | Descriptive statistics for both groups.

Variables	Experimental group *n* = 12	Control group *n* = 11
Male sex, *N* (%)	6 (50)	6 (55)
Air gun shooters	4	5
Small-caliber rifle shooters	8	6
Age in years, *M* (*SD*)	18.25 (2.38)	20.73 (5.24)
Shooting experience in years, *M* (*SD*)	4.83 (1.67)	7.34 (4.83)
Training per week in min, *M* (*SD*)	166.36 (57.67)	162.27 (71.88)

Design, Procedure, and Measures

In the current study, we investigated the effects of a self-control demanding task on elite shooters' perceived levels of self-control strength and their shooting performance over time. In order to do so, we asked elite shooters to perform a shooting task (i.e., four blocks of 10 shots each) at two times of measurement (T1 and T2) and continuously measured their levels of perceived self-control strength after 10 shots each. After T1, the participants were randomly assigned to work on a task which required high levels of self-control (experimental condition) or on a task which was less effortful (control condition).

The study was conducted in single sessions at the National Training Centre of Baden-Württemberg, Germany. First, the participants delivered written informed consent, reported demographic information (age, gender, shooting experience, and training per week), confirmed that they did not consume caffeine, alcohol, or nicotine up to 24 h before the testing session, had a healthy meal up to 1 h before taking part in the study, and performed an individual warm up session for approximately 5 min.

Then, the participants were informed that they had to perform two shooting series with a transcription task in between, starting with the first series of four shooting blocks of 10 shots each on standard regulation shooting boards (i.e., 40 shots in total; T1). On the shooting board, there were 10 concentric rings, with each ring representing a certain score (i.e., 10 points for the center of the target to 0 when the board was not hit at all). In line with the official regulations of the International Shooting Sport Federation (ISSF, 2020), the shooting boards were setup at a distance of 10 m for air gun shooters and at a distance of 50 m for small-caliber rifle shooters (i.e., the dimensions of the shooting boards were identical for gun shooters and small-caliber rifle shooters).

All instructions and questionnaires were delivered as paper-pencil versions in German. To control the possibility that the primary self-control task unintentionally affected subsequent performance *via* mechanisms that were unrelated to the incurred self-control costs, we assessed task motivation as well as positive affect and negative affect in regard to the upcoming shooting task (e.g., Englert, 2019). Task motivation was measured with the subscale Effort and Importance from the Intrinsic Motivation Inventory (IMI; Ryan, 1982; German version: Stocker et al., 2019). All five items started with the phrasing "In the following shooting task…" (sample item: "…I will do my best") and were rated on scales ranging from one (*completely inaccurate*) to seven (*completely accurate*). For the IMI as well as for the other questionnaires included in this study, we computed overall scores by averaging each participant's answers on the specific measure so that higher scores on the respective measure always indicated higher values of the respective variable. The IMI has been frequently adopted in sport and exercise setting and has proven to be a valid measure of motivation (e.g., Goudas et al., 2000).

Positive affect and negative affect related to the upcoming shooting task were measured using the German version of the Positive and Negative Affect Schedule (PANAS; Krohne et al., 1996). The questionnaire includes 10 items for negative

TABLE 2 | Means (*M*), standard deviations (*SD*), and internal consistencies (Cronbach's α) of the German 5-Item Brief State Self-Control Capacity Scale (SMS-5; Lindner et al., 2019) for each shooting block during the first and second shooting rounds.

Shooting block	First shooting round				Second shooting round			
	Experimental group *n* = 12		Control group *n* = 11		Experimental group *n* = 12		Control group *n* = 11	
	M (*SD*)	α	*M* (*SD*)	α	*M* (*SD*)	α	*M* (*SD*)	α
Baseline	5.33 (1.26)	0.877	5.38 (1.14)	0.866	5.25 (1.22)	0.852	5.20 (0.98)	0.832
1	5.27 (1.23)	0.827	5.25 (0.95)	0.760	5.23 (1.28)	0.764	5.11 (0.94)	0.782
2	5.20 (1.39)	0.872	5.25 (0.88)	0.671	4.90 (1.80)	0.914	4.91 (1.34)	0.862
3	5.18 (1.26)	0.838	5.13 (0.93)	0.674	4.77 (1.70)	0.921	4.64 (0.98)	0.683
4	5.05 (1.48)	0.901	4.82 (1.08)	0.595	4.65 (1.74)	0.893	4.38 (0.93)	0.652

Each item of the SMS-5 was answered on a scale from 1 ("not true") to 7 ("very true").

affect (PANASNA; sample item: "angry") and 10 items for positive affect (PANASPA; sample item: "interested") which had to be answered on five-point Likert-type scales (1 – *not at all* to 5 – *very much*). The validity and reliability of the PANAS have been empirically supported in several studies (e.g., Crawford and Henry, 2004).

Finally, before starting the shooting task, shooters completed the German 5-Item Brief State Self-Control Capacity Scale (SMS-5; Lindner et al., 2019), which served as our measure of perceived self-control in the given situation. The SMS-5 is a validated short version of the State Self-Control Capacity Scale (Ciarocco et al., 2007, unpublished).[1] The five items ("I feel drained"; "I feel calm and rational"*; "I feel lazy"; "I feel sharp and focused"*; and "I feel like my willpower is gone"; *inverted item) were answered on scales from 1 (*not true*) to 7 (*very true*) in regard to the athlete's current state (Instruction: "Please reply spontaneously to the following statements about how you feel at the moment"). We calculated overall scores by averaging each participant's answers, with higher scores indicating higher levels of perceived state self-control strength. The validity and reliability of the SMS-5 have been supported in previous studies (e.g., Lindner and Retelsdorf, 2020).

After filling out the SMS-5, participants performed the four shooting blocks of 10 shots each on standard regulation shooting boards and were asked to always aim for the highest score. The scores were measured *via* electronic shooting systems from the company "Meyton"[2] consisting of the software "ShootMasterII" and the electronic scoring targets "BLACK MAGIC" using LED infrared light barriers. After each block, participants reported their perceived level of self-control strength *via* the SMS-5. In total, the SMS-5 was completed at five times during this shooting session at T1 (the internal consistencies for each time of measurement are depicted in **Table 1**).

After a five-minute break, participants were randomly assigned to an experimental (*n* = 12) or a control condition (*n* = 11) and transcribed a neutral text on a separate sheet of paper (for this procedure, Bertrams et al., 2010) for 6 min (as this is a typical duration in this kind of research; Giboin and Wolff, 2019). In the experimental condition, participants were asked to always omit the letters "e" and "n" while transcribing the text, whereas participants from the control condition transcribed the text conventionally. Both conditions were instructed to transcribe as many words as possible in the given time while avoiding transcription mistakes. This task has been successfully applied in numerous studies to manipulate perceived levels of self-control strength (for an overview, Englert, 2017, 2019; for two recent meta-analyses, see Giboin and Wolff, 2019; Brown et al., 2020). The number of transcribed words as well as the number of transcription errors were recorded, assuming that participants from the experimental condition would transcribe fewer words and commit more mistakes due to the more challenging instructions (see also Englert et al., 2015).

Next, participants worked on a three-item manipulation check ("How effortful did you find the transcription task?", "How difficult did you find the transcription task?", and "How strongly did you have to regulate your writing habits?"; Cronbach's α = 0.73; Bertrams et al., 2010), which had to be answered on five-point Likert-type scales ranging from 1 (*not at all*) to 5 (*very much*), assuming that the experimental condition would experience higher self-control costs.

After that, participants immediately started their second series of four shooting blocks of 10 shots each on standard regulation shooting boards (i.e., 40 shots in total; T2). As at T1, we assessed participants' task motivation (Stocker et al., 2019), affect (Krohne et al., 1996), and their perceived levels of self-control strength (SMS-5; Lindner et al., 2019) before executing another series of four shooting blocks of 10 shots each on standard regulation shooting boards. The SMS-5 was filled out after 10 shots each. Shooting performance was again assessed *via* the electronic shooting systems. To match the data from the two shooting rounds, each participant was assigned a unique anonymous code. For both shooting rounds, we set a time limit of 40 min, which was chosen based on the regular competition time (ISSF, 2020).

[1]Ciarocco, N. J., Twenge, J. M., Muraven, M., & Tice, D. M. (2007). *Measuring State Self-Control: Reliability, Validity, and Correlations with Physical and Psychological Stress*. Unpublished manuscript. NJ, USA: Monmouth University.
[2]https://www.meyton.info

Finally, we assessed participants' trait self-control strength with the German short version of the Self-Control Scale (SCS-K-D; Bertrams and Dickhäuser, 2009; Cronbach's α = 0.82), which contains 13 items (e.g., "I am good at resisting temptations") that had to be answered on 5-point Likert-type scales (1 = *Not at all* to 5 = *Very much*). After finishing the SCS-K-D, all participants were debriefed and thanked for their participation.

RESULTS

Data Analytic Strategy

As in our previous work (Englert et al., 2020), we have estimated perceived levels of self-control strength at baseline and after each shooting block. To take into account the baseline differences in the perceived levels of self-control strength between subjects and track its change over time with a better precision, we have expressed self-control measured after shooting blocks in percentage of baseline self-control (SCpercentage).

In line with calls that have been made by fellow researchers, we refrained from using traditional ANOVA to analyze the data and to rather apply linear mixed models instead (Boisgontier and Cheval, 2016). In a nutshell, linear mixed models can be understood as linear regressions within a linear regression. They incorporate the error from clusters of non-independent data points into the total error of the statistical model. This approach has substantial advantages over more traditional statistical approaches. For example, with linear mixed models, measurements that are nested within one subject can be taken into account, unbalanced or missing data can be handled, loss of information which occurs when data are simply averaged is avoided, and the partial pooling strategy allows for better parameter estimation (Boisgontier and Cheval, 2016; Nalborczyk et al., 2019).

It is advised to fully maximize the error structure of linear mixed models to reduce type I errors (Barr et al., 2013). However, it is frequently observed that such models do not converge within a frequentist paradigm, while their Bayesian equivalents tend to converge. Thus, instead of frequentist null hypothesis significance testing (NHST), we opted to employ Bayesian analyses instead. In studies with multiple groups and/or measurements, multiple comparisons represent another issue where a Bayesian framework is better suited than traditional NHST and allows the researchers to assess whether or not an effect credibly differs from a null value (Kruschke and Liddell, 2018; Nalborczyk et al., 2019). Finally, from the perspective of communicating research findings, a Bayesian framework allows for a much more intuitive interpretation of results, as, for example, a 95% credible interval indicates that the estimate has 95% of chance of being within the interval boundaries (Kruschke and Liddell, 2018).

Data cleaning and formatting were performed with Python (3.7). Statistics were performed with R (version 3.5.3). We investigated data distribution with quantile-quantile plots using the *qqplotr* R package with confidence intervals based on an inversion of the Kolmogorov-Smirnov test (Almeida et al., 2018). For all data sets, inspection of Q-Q plots did not lead us to reject the assumption of normal distribution, and thus, all statistical models were set with a normal response distribution. We performed Bayesian statistical analysis with the R package *brms* (Bürkner, 2017, 2018) and used linear mixed models to test our hypotheses (specifics of each model are described in more detail below). For all tests, we used the default priors of the package since they are non-informative and "let the data speak" (Gelman, 2009, p. 176). For each model, we used four Markov Chain Monte Carlo with 4,000 iterations per chain (2,000 for warm-up). We checked that the models converged correctly and fit adequately the data. Unless stated otherwise, we "maximized" the error structure of each model to limit type I errors according to Barr et al. (2013). To get further information from our models, we used the build-in function *hypothesis()* from the package *brms* to calculate contrasts (see the *brms* manual). This function allows the comparison of estimates distributions by subtracting one to another.

To assess, if the experimental condition was more self-control demanding than the control condition, we compared the aggregate of the three manipulation check items (MC-M), the number of transcribed words (T-words), and transcription errors (T-errors) between groups with a simple Bayesian between group comparison. To rule out differences in trait self-control between groups, we performed another simple Bayesian between group comparison with SCS-K-D as the dependent variable. Further, we wanted to compare IMI, PANASPA, and PANASNA between groups across time. For this, we used a model with an interaction between the constant effects time and group and with random intercepts by subjects: DV ~ group × time + (1 | subject). We did not add random slopes by subject across time since we had not enough data points and models could not converge.

Then, we investigated whether shooting performance was affected by the manipulation of perceived self-control strength, and if the effect of the manipulation was exacerbated by the number of shooting blocks performed. For this, we used a linear mixed model with constant effects of group, time and blocks, and interactions between these effects. We used random intercepts by subjects and random slopes across block, time, and their interactions [shooting performance ~ group × block × time + (block × time | subject)]. Block levels were considered as numeric, while time levels were considered as factor. We used the same model to assess whether SCpercentage was affected by the manipulation of perceived self-control strength and if this effect was accentuated by the number of shooting blocks.

Results are presented as such: posterior estimate mean (posterior estimate lower and upper boundaries of the 95% credible interval). The 95% credible interval represents the area of the distribution that contains 95% of the probability distribution. Here, we consider an estimate credibly different from zero if the 95% credible interval does not contain zero.

Preliminary Analyses

We found that the experimental condition had a higher MC-M value (beta coefficient from experimental condition to control

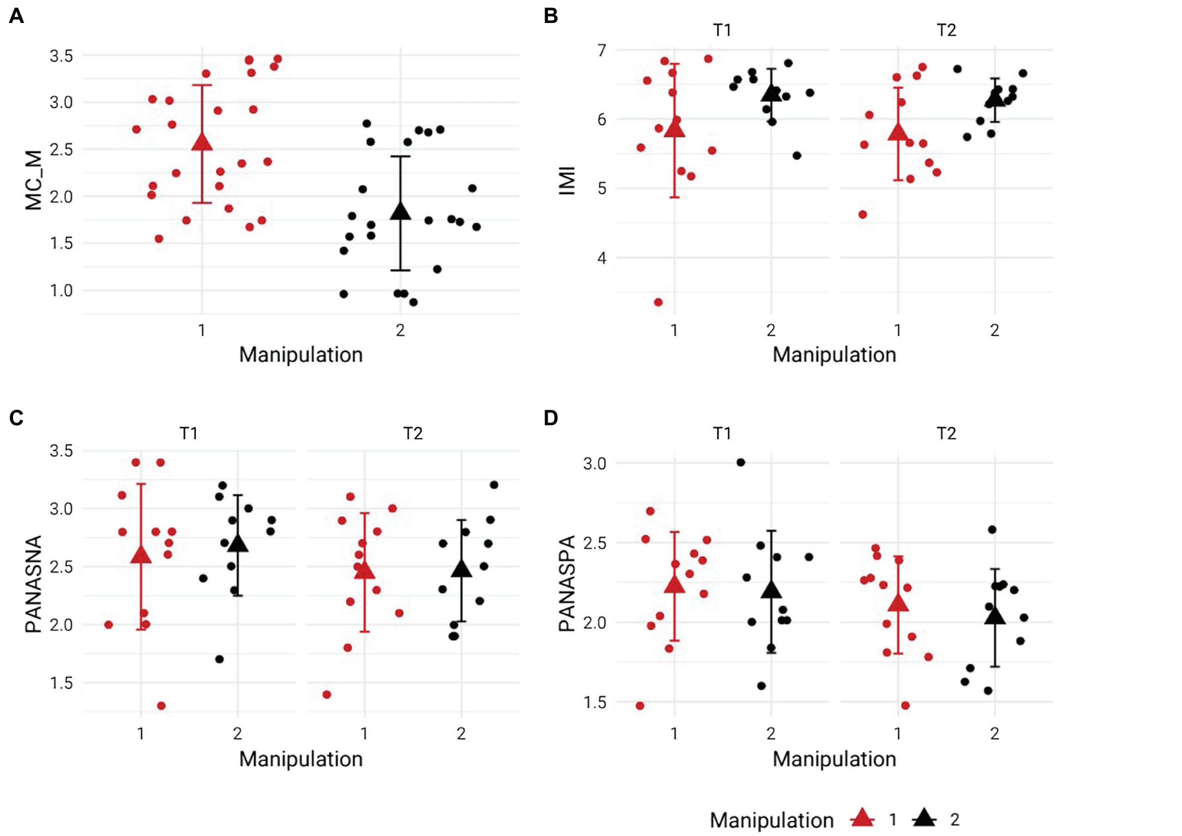

FIGURE 1 | The experimental manipulation (depicted in red) was perceived as more self-control demanding than the control condition (depicted in black) **(A)** but did neither affect the motivation to complete the shooting task **(B)**, nor did it lead to changes in negative **(C)** and positive **(D)** affect. Error bars represent standard deviations.

condition = −0.74 [−1.11, −0.37], **Figure 1A**), indicating that the manipulation of perceived self-control strength was effective. Further, the control group transcribed more words (beta coefficient from experimental condition to control condition = 25.85 [7.33, 44.71]) and committed less errors (beta coefficient from experimental condition to control condition = −3.95 [−6.05, −1.92]). Importantly, there were no credible group, time, and interaction effect for IMI, PANASNA, and PANASPA (**Figures 1B–D**, respectively, statistical results not displayed). Additionally, contrast comparisons between groups at T1 and T2 or within groups contrast comparison between T1 and T2 showed no differences. Finally, groups did not differ in regard to trait self-control, −0.10 [−0.75, 0.56].

Main Analyses

As displayed in **Figure 2**, there was no effect of the manipulation of perceived self-control strength or shooting blocks and no interaction between these factors that affected shooting performance see also **Table 2**. Similarly, SCpercentage was not affected by the manipulation of perceived self-control strength, number of shooting blocks, or an interaction between these factors (**Figure 3**).

DISCUSSION

Investing high levels of self-control over extended periods of time can lead to lower levels of perceived self-control strength (e.g., Englert et al., 2020). During shooting tournaments, athletes need to remain focused over several hours suggesting that perceived self-control strength seems to play a pivotal role for top-level performance (e.g., Di Fronso et al., 2016; Chen and Mordus, 2018). A recent correlational study by Englert et al. (2020) revealed that the level of perceived self-control strength significantly decreased over the course of two one-hour shooting tasks. These decreases in perceived self-control strength were significantly related to actual shooting performance, meaning that the athletes performed worse the lower their levels of perceived self-control strength were. Interestingly, these significant drops in shooting performance and perceived self-control strength were only found in sub-elite shooters. In the present study, we built on these correlational findings reported by Englert et al. (2020) and conducted an experiment in order to dig deeper into the causal effects of a self-control demanding task on performance and the level of perceived self-control strength in elite shooters. As expected,

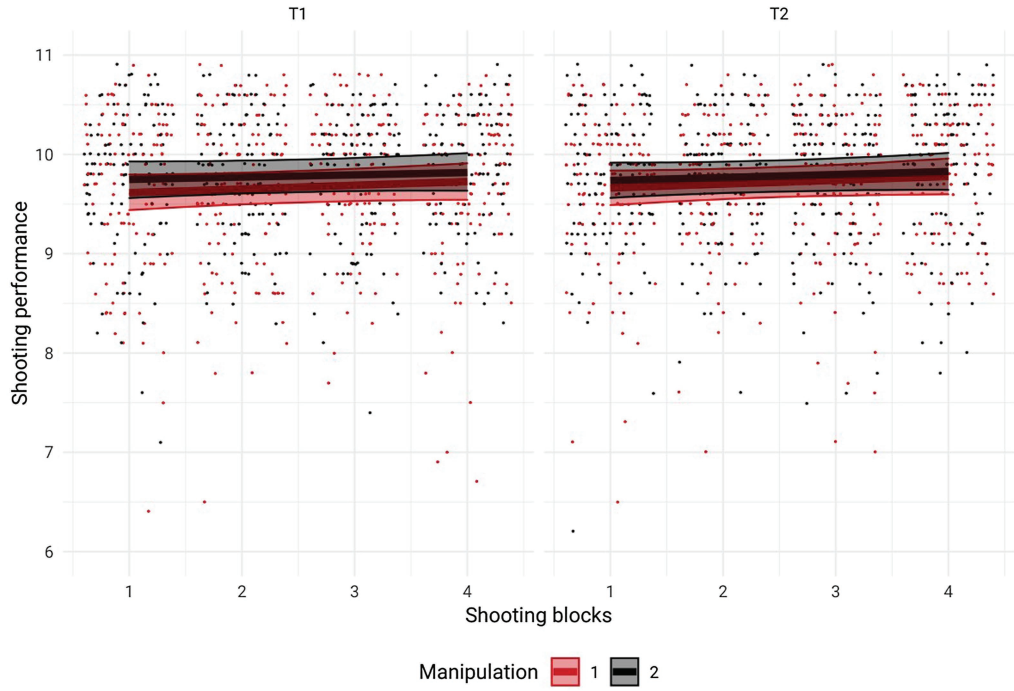

FIGURE 2 | Visualization of shooting performance before (T1) and after (T2) the experimental manipulation as a function of experimental condition. Shooting performance did not change between T1 and T2 and was not affected by prior mental exertion. Data from the experimental group are depicted in red, and data from the control group are depicted in black. Error bars represent 95% credible intervals.

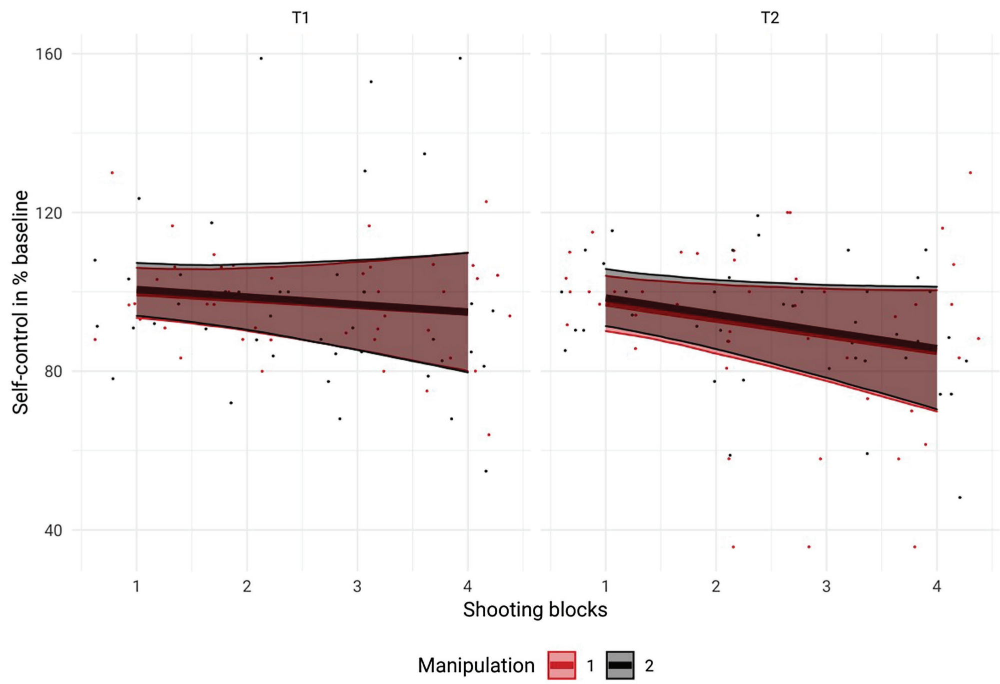

FIGURE 3 | Visualization of perceived self-control strength relative to perceived self-control strength at baseline before (T1) and after (T2) the experimental manipulation as a function of experimental condition. Perceived self-control strength was not affected by prior mental exertion. Data from the experimental group are depicted in red, and data from the control group are depicted in black. Error bars represent 95% credible intervals.

we did not find any empirical evidence for a negative carry-over effect of the self-control demanding task on performance or the level of perceived self-control strength. How can these pattern of results be explained and how might they shed light on the mechanisms of how self-control exerts its influence?

According to one of the most prominent resource-based self-control theories – the strength model (e.g., Baumeister et al., 2007) – an individual's self-control resources are limited: After an initial task which required high levels of self-control, one's self-control resources should diminish over time, ultimately leading to a state of ego depletion, during which subsequent self-control demanding tasks are executed less efficiently (Englert, 2017, 2019). The current findings do not fit the assumption of a limited self-control resource, as we adopted a reliable task designed to manipulate perceived self-control strength, but shooting performance and the level of perceived self-control strength did not differ between the control and the experimental condition (Bertrams et al., 2010). Our study adds to the results of recent studies which did not find any reliable evidence for this ego depletion effect (e.g., Hagger et al., 2016). We would like to tackle four potential reasons why elite shooters did not suffer from the foregoing self-control demanding task.

First, on a methodological level, one might argue that the transcription task we adopted in our study is not an appropriate task to manipulate perceived self-control strength (Bertrams et al., 2010), as previous studies have applied other tasks with longer durations (e.g., see also Van Cutsem et al., 2017). However, we would like to point out that participants in the experimental condition did actually judge the transcription task as being more difficult, more self-control demanding, and more effortful than the control condition, indicating that the task was indeed suited to manipulate perceived levels of self-control strength (see also Englert et al., 2015). Despite the self-control demanding features of the transcription task, the elite shooters were able to remain focused in the subsequent shooting task and did not feel mentally exhausted. Nonetheless, future studies might want to apply alternative self-control demanding tasks to manipulate self-control strength, in order to replicate and extend our findings. However, this endeavor is not as easy as it might seem at first, as there are several flaws regarding the most frequently applied mentally demanding tasks (e.g., Englert et al., 2019). It is also important to mention that thus far there is no general agreement among researchers how long a self-control demanding task should ideally last, to reliably manipulate the level of perceived self-control strength (e.g., Giboin and Wolff, 2019; Englert and Bertrams, 2021; Wolff et al., 2021a). The validity of the most popular mentally fatiguing tasks should therefore be rigorously tested in future studies (e.g., Dang, 2018). In this context, we would also like to point out that future studies might want to adopt repeated measures designs when investigating the effects of effort on performance, in order to reduce between-subject variability (Charness et al., 2012). However, in the current study, it was not possible to analyze shooting performance at multiple times of measurement given the limited training time of the elite athletes.

Second, in line with recent reward-based conceptualizations of self-control, elite athletes might simply be able to perform the shooting task with less self-control costs (for a discussion, please see Wolff et al., in press). One mechanism by which task execution can become less costly is by a higher degree of automatization of the task-specific processing demands. Interestingly, this hypothesis can be supported by many neurophysiological studies indicating task- or training-specific neural adaptations following motor training (Karni et al., 1998; Doyon et al., 2002; Giboin et al., 2019b). Therefore, elite athletes are likely to have motor commands that are extremely optimized and specific for their highly trained tasks (i.e., shooting). Such specific optimizations of the motor command could be one explanation for why elite athletes incur less self-control costs for tasks that are extremely self-control demanding for non-elite athletes. Importantly, this relationship between motor costs of a physical task and the self-control costs it produces does not only apply to elite athletes. Recent psychoneurophysiological evidence shows that more efficient movement execution can improve performance in a self-control demanding physical task while being performed with less activity in brain areas that are relevant for self-control (Giboin et al., 2019a). In terms of optimizing the EVC, a reduction in task-specific control costs could skew the EVC toward applying enough control for performance to not deteriorate, although one had already applied effort toward an unrelated previous self-control task. This is in line with recent meta-analytic evidence that performance drops after self-control application are smaller if the subjects have experience in engaging in the subsequent physical self-control task (Giboin and Wolff, 2019). In regard to our second research question, this indicates that elite athletes are not immune to self-control demands in general (as evidenced by the between group difference in perceived self-control strength after the transcription task), but they seem to be able to efficiently perform the task they are experts in (i.e., shooting) regardless of prior self-control costs.

Third, a recent meta-analysis (Giboin and Wolff, 2019) showed that detrimental effects of prior mental exertion were smaller when the subsequent sporting task had a high person-situation fit (i.e., when participants were asked to do a sporting task they were proficient in as opposed to doing a task they did not regularly engage in). This effect could be even more pronounced in our sample of elite athletes, where the person-situation fit was particularly large (elites doing something they are elite at). In addition, the meta-analysis by Brown et al. (2020) showed that effects of prior mental exertion depended on the type of subsequent physical task. Observing, for example, that performance was not reliably impaired in tasks that required maximum power, whereas in other tasks that supposedly hinged more on self-control performance, was more robustly impaired. In this vein, it is possible that for highly trained shooters the sporting task did not hinge sufficiently on self-control to be robustly impaired by the prior mental exertion.

Lastly, recent work points toward a strong link between boredom and self-control (Wolff et al., 2021b), suggesting that task-induced boredom might act as a confounding self-control demand (Wolff et al., 2021c). Indeed, there is preliminary evidence showing that tasks that are designed to be less self-control demanding might be perceived as being boring (compared to tasks that are designed to be self-control demanding) and that task-induced boredom affects performance in self-control demanding tasks (Bieleke et al., 2021). Thus, another explanation as to why we observed no detrimental effects on shooting performance might be due to systematic differences in how boring the control condition was observed compared to the experimental condition. This last explanation certainly warrants further dedicated research.

Finally, we would like to offer suggestions on how to improve attention regulation and shooting performance. As mentioned in the previous paragraph, reducing the task-specific control costs could skew the EVC toward applying enough control for performance to not deteriorate. For instance, implementation intentions should help to decrease the task-specific control costs: Implementation intentions are predefined action plans which are automatically triggered if a certain situation occurs, meaning that less effort needs to be invested to execute the respective behavior (e.g., Sheeran et al., 2005). Future studies should focus on how implementation intentions can counteract the potential carry-over effects of low levels of perceived self-control strength.

In a similar fashion, the strength model argues that regular self-control training should improve self-control performance in the long run. Bray et al. (2015) asked participants to perform a tiring maximal graded cycling task at two times of measurement separated by 2 weeks. During these 2 weeks, the experimental condition had to regularly squeeze a handgrip multiple times a day (i.e., a self-control demanding task), while the control condition did not receive any additional instructions. After the two-week period, cycling performance in the experimental condition significantly improved, while participants' performance in the control condition did not change significantly.

Taken together, the current study is in line with the correlational findings reported by Englert et al. (2020), as elite shooters seem to be less affected by a previous self-control demanding task. Future studies should continue to dig deeper into the exact mechanisms how expertise exerts its effects on self-control.

DATA AVAILABILITY STATEMENT

The data sets presented in this study can be found in online repositories. The names of the repository/repositories and accession number(s) can be found at https://osf.io/7bdh6/.

ETHICS STATEMENT

Ethical approval was not provided for this study on human participants because ethical review and approval was not required for the study on human participants in accordance with the local legislation and institutional requirements. The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

CE and AD equally contributed to the conceptualization of the study and review of relevant related work. CE, WW, and LS-G analyzed and interpreted the data. CE and WW prepared the draft manuscript, while AD and L-SG provided the critical revisions. All authors approved the final version of the manuscript and agreed with the order of presentation of the authors.

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Does Intensive Training of Attention Influence Cognitive Fatigability in Patients With Acquired Brain Injury?

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Research Objectives: Impairments in attention and the speed of information processing are central to the experience of cognitive fatigue in patients with acquired brain injury (ABI). Attention may be improved through intensive training in a rehabilitation setting. The aim of the study was to investigate the feasibility of reducing cognitive fatigability (CF) using attention training and to explore the effect of two different approaches to attention training.

Design: Randomised controlled study in a rehabilitation setting.

Participants: 59 patients (age 19–59 years) with mild to moderate stroke or traumatic brain injury in the early (<4 month) phase.

Interventions: Patients were randomly assigned to intensive specific training with Attention Process Training (APT) or Activity-Based Attention Training (ABAT) for 3–5 days per week for a period of 5–6 weeks with a total of 20 h, in addition to traditional interdisciplinary rehabilitation.

Main Outcome Measure: CF was conceptualised as performance decline in terms of an increased number of incorrect responses between the first and the last quintiles of the Paced Auditory Serial Addition Test (PASAT). A negative result was defined as fatigability. The evaluator of fatigability was blinded to treatment.

Results: At baseline, there were no differences between the groups in age, education, reasoning, anxiety or depression. After training, a significant treatment effect was found ($p = 0.020$), as the APT-group, but not the ABAT-group, had improved. However, after controlling for baseline differences regarding CF on the PASAT-f, the difference was no longer significant.

Conclusion: The results indicate that cognitive training might be a feasible method for reducing CF through attention training and that patients with high levels of CF benefit most from attention training. The type of intervention provided, whether specific or activity-based attention training, appears to be of less importance, as there was no treatment effect after controlling for the baseline level of CF. Future studies are required to confirm the validity of the findings.

Keywords: acquired brain injury, attention, cognitive fatigability, paced auditory serial addition test (PASAT), intraindividual variability

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INTRODUCTION

Fatigue is one of the most prominent symptoms after brain injury. A substantial number of patients experience prolonged subjective problems that prevent them from returning to work and having an active leisure time (Olver et al., 1996; Glader et al., 2002), but treatment recommendations are still unsatisfactory and the evidence for different treatment approaches is weak (Wu et al., 2015).

There are two general approaches to treatment of the experience of fatigue – pharmacological treatment with methylphenidate (Johansson et al., 2017) and behavioural treatment, based on the assumption that mental fatigue is a condition reflecting an insufficient balance between the internal resources of mental energy and the ability to cope with the demands on the system that has been inflicted by cognitive impairments (Ashman et al., 2008). Thus, earlier behavioural studies attempted to decrease mental load through relaxation and mindfulness training (Johansson et al., 2012) or strengthen mental "capacity" using computerised working memory training (Björkdahl et al., 2013).

Inconclusive results from treatment studies could mirror the problem that fatigue is still poorly defined, and its measurement is limited by methodological and conceptual shortcomings (Kluger et al., 2013), as most studies rely on self-assessment questionnaires (DeLuca, 2005; Walker et al., 2019) that assess self-experienced mental fatigue. However, mental fatigue is a broad concept that does not capture the underlying causes of fatigue, nor is it precise enough to generate specific treatment hypotheses. Also, subjective ratings of fatigue are frequently influenced by other emotional states, such as depression (Arnold, 2008). These factors contribute to a low concordance between subjective self-assessed and objective performance-based fatigue measures and constitute major shortcomings in the evaluation of the effects of fatigue reducing interventions. Therefore, Kluger et al. (2013), have emphasised the importance distinguishing between subjective fatigue, as opposed to objectively measured fatigue.

Cognitive fatigue is a more stringently defined term that is used to show that mental fatigue is associated with thought-demanding tasks (Wylie and Flashman, 2017). To some extent, this term excludes the emotional fatigue that is common in depression (Wylie and Flashman, 2017) but the concept is not specific enough to be able to demonstrate that there is a *fatigability* associated with cognitively demanding tasks. One approach to create an even more narrowly objective assessment of fatigue is to conceptualise it as cognitive fatigability (CF), which is defined as a decline in performance on attention-demanding tasks by comparing performance at the beginning of a cognitively demanding test with performance at the end of the test (Kohl et al., 2009; Kluger et al., 2013), either in terms of a decrease in task accuracy (Walker et al., 2012; Morrow et al., 2015) or an increased response time (Berard et al., 2019). Also, increased intraindividual performance variability has been recommended as a metric for CF (Wang et al., 2014).

Holtzer et al. (2011) have successfully demonstrated that CF is triggered by tasks of executive attention, referring to the capacity to monitor and resolve conflicting information, which is subserved by the frontal cortico-striatal circuitry (Chaudhuri and Behan, 2000, 2004) as opposed to the alerting and orienting parts of attention (Holtzer et al., 2011). In line with this, previous studies (Lorist et al., 2005; Möller et al., 2014) have shown that the tests best suited to the assessment of CF are those which require controlled information processing or coordination of several cognitive domains.

Investigating the options for alleviation of CF in acquired brain injury is of particular interest, since treatment recommendations for CF are insufficient and the evidence for different treatment approaches is weak (Walker et al., 2019).

There are several systematic ways to strengthen the different aspects of information processing using systematic cognitive training (Cicerone et al., 2019). One of them is Attention Process Training, (APT). Attention Process Training is a theoretically anchored, evidence-based attention training method recommended after brain injury (Cicerone et al., 2019). APT includes targeted attention training based on hierarchical repetition to strengthen the attentional and executive functions at a functional level, but it also includes metacognitive aspects that promote a generalisation of strategies (Sohlberg and Mateer, 1987).

In a randomised controlled study (Bartfai et al., 2014) two methods to reduce the impact of attention dysfunction after acquired brain injury (ABI) were compared; a systematic cognitive training approach, Attention Process Training (APT), and Activity-Based Training of Attention (ABAT), focussing on adjustment and the use of strategies with the aim of improving occupational performance (Markovic, 2017).

Our group has previously demonstrated a performance decrement in attention-demanding tests, along with increased self-rated fatigue, in patients with mild traumatic brain injury (mTBI) (Möller et al., 2014). Furthermore, in an fMRI study (Möller et al., 2017) we have shown that mTBI patients did exhibit a decrease in performance on a psychomotor vigilance test (PVT) and an altered regional cerebral blood flow (rCBF) in several regions, including the left thalamus and superior frontal gyri, right precuneus and insula, together with the left/right medial frontal gyri and ACC, when compared to the healthy controls. Parts of these regions have been found to be active in tasks involving executive attention (Raz, 2004).

There is no gold standard for which test measures fatigability best, but the Paced Auditory Serial Addition Test (PASAT) is a multifactorial attention-demanding task measuring information processing speed, sustained attention, working memory and multitasking capacity (Gronwall, 1977) that has been used in several studies to capture CF in patients with multiple sclerosis (Walker et al., 2019), where fatigue is a major problem (Cantor, 2010). Though used in slightly different ways across studies as to interstimulus intervals (ISI) and cut off points for impairment, the performance on PASAT in the 3-second version of the test has shown a decline in MS-patients, based on the slope of correct responses throughout the test (Schwid et al., 2003) or by comparing the number of correct responses in the first and the last thirds of the test (Morrow et al., 2015).

Since CF has been associated with functional alterations in attentional networks in the brain (Möller et al., 2017) and since CF is, by its conceptual definition, closely associated with difficulties in sustaining attention and has been shown to be sensitive to executively demanding attention tasks (Holtzer et al., 2011), our hypothesis was that attention training could reduce CF after brain injury and that systematic attention training with metacognitive components (APT) might outperform ABAT by targeting the executive aspects of attention to a greater extent. Thus, the present study had two research aims: firstly, to investigate the feasibility of reducing CF using attention training and, secondly, to explore the effect of two different approaches to attention training. The present study is the first attempt to alleviate CF using systematic attention training.

MATERIALS AND METHODS

All of the data was collected from a large clinical trial investigating the effects of intensive cognitive rehabilitation of attention, and its impact on function and activity, after acquired brain injury. The specific details can be obtained from the study protocol (Bartfai et al., 2014).

Participants

60 consecutive patients, 19–59 years, 40 men and 20 women in an early phase (<4 months) after mild to moderate stroke or traumatic brain injury with verified attentional impairment, were admitted to either inpatient or outpatient rehabilitation. The Glasgow Coma Scale (GCS) for the TBI patients was 13–15. The degree of stroke impairment at the point of impact was assessed based on medical journals in collaboration between a neuropsychologist and a rehabilitation medicine specialist. Patients included in the study were at a level corresponding to 13–15 GCS. The exclusion criteria indicate that patients with more severe cognitive impairment were not included. One of the patients did not complete the treatment, thus the final sample consisted of 59 subjects.

Inclusion Criteria

Impairment in attention defined by the APT test (cut off scores of 70% or less on at least two of five subtests), scores in the lower average range and above for reasoning skills and abstract thinking (WAIS-III Matrix reasoning Scaled score ≥ 7) (Wechsler, 2003), age 18–60 years and a good understanding of Swedish. The presence of cognitive fatigability was not an inclusion criterion, since the data was collected from a clinical trial not focussing on CF (Bartfai et al., 2014).

Exclusion Criteria

Moderate to severe aphasia, ongoing psychiatric illness, a history of anoxic episodes, substance abuse and severe pain. Severe memory impairment, neglect, an impaired visual field or motor impairment also led to exclusion. For more detailed information, see the previously published study protocol (Bartfai et al., 2014).

Patients who scored ≥ 10 in HADS (Zigmond and Snaith, 1983) were offered antidepressant treatment and were included three weeks after the initiation of pharmacological treatment, according to clinical praxis. These patients were reassessed before inclusion to ensure that their HADS scores met the inclusion criteria.

Procedure

All of the patients were consecutively included in the study within the first four months of injury. They underwent an extensive neuropsychological assessment. In the present study, pre and post-intervention data (within two weeks before beginning the training and after the training) was used. The patients participated in an interdisciplinary brain injury rehabilitation programme (in and outpatient care) with an added 20 h of attention training, either APT ($n = 31$) or ABAT (n = 28), based on randomisation (**Figure 1**). The intensity of the training was 45–90 min, 2–3 times per week for 5–6 weeks. Since rehabilitation cannot be blinded, neither patients nor therapists were blinded to the intervention. However, the assessment was blinded as to the form of treatment (Bartfai et al., 2014).

Assessment

Paced Auditory Serial Addition Test

The PASAT is a cognitively demanding test measuring mental processing speed and various aspects of attention and working memory functions (Gronwall, 1977). We conceptualised CF as a performance decline in terms of an increased number of incorrect responses between the first and the last quintiles of PASAT. The partitioning into quintiles, formerly applied in studies where cognitive fatigability was assessed with psychomotor vigilance tasks (Möller et al., 2017; Berard et al., 2019), was chosen with the intent to optimise the sensitivity to performance decrement.

The Swedish version of the test includes 60 pre-recorded numbers at a standardised pace of 2.4 s between numbers. The task is to sum each new number with the previous one and provide the correct answer before the next number is given. Higher scores indicate better performance. Performance was evaluated according to the manual (Gronwall, 1977).

Cognitive fatigability was measured as declining performance in terms of an increased number of incorrect responses on PASAT (2.4-second version); PASAT fatigability (PASAT-f). The PASAT-f was used as the primary outcome measure.

PASAT fatigability (PASAT-f) was calculated as follows: the material was divided into five sections of 12 numbers each, where the number of correct answers in the first section was subtracted from the number of correct answers in the last section. Fatigability was defined as a lower result at the end of the test compared to the beginning, which gives a negative value, and was reported as the percentage of correct answers in the first quintile [(number of correct answers in the first quintile/total number of correct answers for the entire task) * 100] subtracted from the percentage of correct answers in the last quintile [(number of correct answers in the last quintile/total number of correct answers for the entire task) * 100]. For example: [(4/40)*100] – [(10/40) * 100] = –15%. Intraindividual variability was estimated

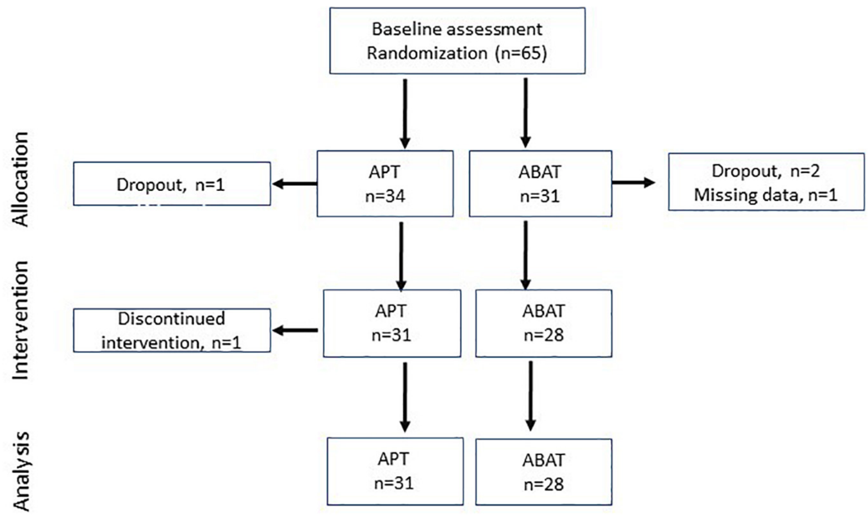

FIGURE 1 | Flowchart of participants in each stage of the trial. APT, Attention Process Training; ABAT, Activity-Based Attention Training.

as the standard deviation of the number of correct responses for each quintile.

Ruff 2 & 7

The Ruff 2 & 7 Selective Attention Test (Ruff and Allen, 1996) is a continuous performance test that measures cognitive speed and sustained and selective attention. In this study, the test was used to investigate the correlation between visual attention as measured with Ruff 2 & 7 and fatigability. The Ruff 2 & 7 consists of twenty, fifteen-second trials, where the task is to identify and cancel the target digits 2 and 7. The digits are embedded among distractors. The distraction consists of alphabetical letters (automatic selective attention) and other numbers (controlled selective attention) for ten trials each. Sustained attention is measured as the total number of correctly identified targets. Higher scores indicate better performance. Performance was evaluated according to the manual (Ruff and Allen, 1996).

Digit Span

Verbal attention span and working memory was assessed with Digit span forward according to the Wechsler Adult Intelligence Scale procedures (Wechsler, 2003). The test was used to investigate the relation between attention span, unrelated to processing speed, and fatigability. The participant is asked to repeat a series of numbers in order of length, (between 2 and 9 numbers), two trials per length.

Hospital Anxiety and Depression Scale

Hospital Anxiety and Depression Scale (HADS) (Zigmond and Snaith, 1983) is a self-assessment questionnaire that was used to control for the effects of anxiety and depression, pre and post-intervention. The questionnaire consists of 14 items divided into the two subcategories of anxiety and depression. A score > 10 on either subscale indicates pathology.

Interventions

Attention Process Training

Attention Process Training (APT) (Sohlberg and Mateer, 1987) is an intensive, function-specific and individualised cognitive training method targeting five attention levels; focussed, sustained, selective, divided, and alternating attention. The training program is comprised of structured visual and auditory exercises administered in a hierarchical manner, supplemented by metacognitive training, education about ABI related attentional deficits and training in generalising acquired strategies into daily life. The APT programme includes a screening instrument to assess attention dysfunction. The result of the test indicates the type and number of attention problems at hand and the suitable starting level for the training program. The APT was performed by a neuropsychologist.

Activity-Based Attention Training

Activity-Based Attention Training (ABAT) consists of standard occupational training that focusses on activity limitations due to attention dysfunction (Markovic, 2017). Activity-Based Attention Training was considered to be treatment as usual. The training includes compensatory strategy training in attention-demanding tasks in the domain of ADL, computerised tasks and group activities. The aim of the training is to improve occupational performance by building on adjustment and the use of these strategies. Examples of the compensatory strategies generally used were taking frequent breaks, using notebooks and verbal self-guidance. Activity-Based Attention Training was performed by an occupational therapist, either individually or in a group depending on the aim.

Statistics/Data Analysis

Variables were summarised using standard descriptive statistical methods. The difference between pre-training and post-training (d-values) was calculated for the neuropsychological outcome measures that were administered pre-treatment and post-treatment. As to inferential statistics, non-parametric methods were used for variables that were not normally distributed. For continuous non-parametric data, the Mann-Whitney *U* test was used for comparison between groups and the Wilcoxon rank sum test was used for comparison within groups. For group comparisons based on categorical data, the Chi2 method was applied. Parametric methods were used for normally distributed data on the interval level. For independent samples, a *t*-test was used to compare treatment groups and a paired samples *t*-test was used for comparison within treatment groups. Depending on the data type, either a Pearson correlation or a Spearman's rank was used for analysis of the associations between variables. To control for baseline differences, a univariate analysis of covariance was performed with d-values of fatigability as the dependent variable, group as the fixed factor and baseline fatigability as a covariate.

The significance level was set to $p < 0.05$ (2-tailed). Power was set at 0.85 (Bartfai et al., 2014). Data was analysed in IBM SPSS, version 23.

RESULTS

Demographics

At baseline there were no differences in age, education, reasoning, digit span, anxiety/depression, type of injury or latency. In the APT group there was a trend toward more women than in the ABAT group (*p = 0.054*) (**Table 1**). However, fatigability rates were comparable between males and females *(t* = –0.181*, df* = 57 *p = 0.857).*

Baseline Descriptive Data

At baseline, the APT group showed significantly more fatigability (PASAT- f) than the ABAT group, as well as a higher degree of performance variability. There were no significant baseline differences between the groups on PASAT total, Ruff 2 & 7 ADS or Ruff 2 & 7 CSS (**Table 2**). The baseline results on Ruff 2 & 7 ADS and CSS for the total group were in the lower normal T-score range ($M = 45$, $SD = 10.53$; $M = 41$, $SD = 9.93$). Thus, for further baseline statistics the two groups were merged.

There were small but significant correlations between PASAT and Ruff 2 & 7 ADS ($r = 0.298$, $p = 0.023$), though not for Ruff 2 & 7 CSS ($r = 0.257$, $p = 0.052$), and between PASAT and Digit Span ($r = 0.341$, $p = 0.012$). Also, there were small but significant correlations between PASAT-f and Ruff 2 & 7 ADS ($r = 0.315$, $p = 0.016$) and between PASAT-f and Digit Span (number of digits forward) n = 59 ($r = 0.290$, $p = 0.033$). However, we found no significant correlations between the PASAT-f variability and Ruff ADS ($r = –0.112$, $p = 0.405$), Ruff 2 & 7 CSS ($r = –0.049$, $p = 0.715$) or Digit Span ($r = –0.241$, $p = 0.079$).

No significant correlations were found between depression (HADS) and fatigability (PASAT-f) ($r = 0.070$, $p = 0.597$) or between anxiety (HADS) and fatigability ($r = –0.076$, $p = 0.567$).

Intervention Effect

Both groups improved on the PASAT total score after training compared to baseline, indicating improved processing speed but there was no significant difference between the groups (**Table 2**).

There was no significant improvement in fatigability for the total group of patients ($t = –1.579$, $df = 57$, $p = 0.120$). However, as indicated in **Figure 2**, a significant treatment effect (d-value; $t = –2.389$, $df = 56$, $p = 0.020$) was observed, as the APT group, which started from a lower level, reduced their fatigability (PASAT f) more than the ABAT group ($p = 0.020$). Furthermore, intraindividual variability was significantly reduced in the APT-group ($t = 2.399$, $df = 30$, $p = 0.023$) but not in the ABAT-group ($t = 1.724$, $df = 26$, $p = 0.097$).

Both groups improved on Ruff 2 & 7 ADS and CSS after training (**Table 2**), however, no significant correlation was found between the change in fatigability pre/post intervention (d-value) and the d-values for Ruff 2 & 7 ADS ($r = 0.129$, $p = 0.339$) and CSS ($r = 0.034$, $p = 0.802$) for the total group.

There was a strong negative correlation between the baseline fatigability value and the fatigability d-value that is independent of intervention group (APT $r = –0.801$ $p < 0.001$.; ABAT $r = –0.711$ $p < 0.001$), indicating a better treatment effect in subjects with a lower baseline value, regardless of the type of attention training.

To control for the effect of the baseline fatigability value on intervention outcome, a univariate analysis of covariance was carried out with the fatigability d-value as a dependent variable, treatment as an independent variable and the baseline value of fatigability as covariate. The result showed no significant effect from the type of intervention after controlling for the baseline value of fatigability, F (1, 55) = 0.307, $p = 0.581$.

Post hoc Analyses

To investigate the impact of ceiling effects of PASAT on the differences in the fatigability decrease observed between the groups, we counted the number of subjects who reached the maximum level (12 correct answers) in the first and last quintiles of PASAT at baseline and post-treatment. The result showed that,

TABLE 1 | Demographics and inclusion data for the Attention Process Training group (APT) and the Activity-Based Attention Training group (ABAT).

	APT *N* **= 31**	**ABAT** *N* **= 28**
Age years, mean (SD)	45.2 (11.8)	43.9 (11.2)
Gender, n (% females)	14 (45%)	6 (21%)
Education		
• Elementary ≤ 9 years, n (% participants)	1 (3%)	0
• High school (% participants)	7 (23%)	8 (29%)
• University < 4 years, n (% participants)	15 (48%)	14 (50%)
• University > 4 years, n (% participants)	8 (26%)	6 (21%)
Type of injury		
• Stroke, n (% participants)	26 (84%)	20 (71%)
• TBI, n (% participants)	5 (16%)	8 (29%)
Latency days, mean (SD)	60.1 (25.0)	58.8 (27.9)
Digit span forward	5.8 (1.1)	5.9 (1.3)
HADS-Anxiety, median (range)	5 (0–16)	3 (0–18)
HADS-Depression, median (range)	3 (0–15)	3 (0–15)

HADS, Hospital Anxiety and Depression Scale; TBI, Traumatic Brian Injury.

TABLE 2 | Values pre-training and post-training for the Attention Process Training group (APT) and the Activity-Based Attention Training group (ABAT) on neuropsychological measurements.

Measurements	Pre-training	Between group p-value	Post-training	Between group p-value	d-value	Within group p-value
PASAT total M (SD)APT, *N* = 31	34.4 (10.0)	0.932	49.2 (7.1)	0.502	14.8 (5.8)	<001
PASAT total M (SD)ABAT, *N* = 28	34.7 (13.8)		47.4 (11.9)*		12.8 (9.6)	<001
PASAT% Fatigability M (SD) APT, *N* = 31	–8.9 (8.3)	0.006	–4.9 (5.0)	0.890	4.0 (7.2)	0.004
PASAT% Fatigability M (SD) ABAT, *N* = 28	–3,7 (6.2)		–4.7 (6.2)*		–1.0 (8.8)	0.573
Ruff 2 & 7 ADS M (SD) APT, *N* = 31	127.8 (34.8)	0.801	135.5 (36.3)	0.812	8.1 (19.7)	0.031
Ruff 2 & 7 ADS M (SD) ABAT, *N* = 28	125.8 (23.3)		137.6 (28.6)*		13.1 (20.2)	0.002
Ruff 2 & 7 CSS M (SD) APT, *N* = 31	108.6 (27.1)	0.982	114.8 (26.4)	0.343	6.2 (13.4)	0.016
Ruff 2 & 7 CSS M (SD) ABAT, *N* = 28	108.5 (17.5)		121.0 (22.0)*		13.1 (17.2)	0.001

PASAT, Paced Auditory Serial Addition Test; ADA, Automatic Detection Speed; CSS, Controlled Search Speed; d-value, difference between pre- and post-training.
** One participant missing.*

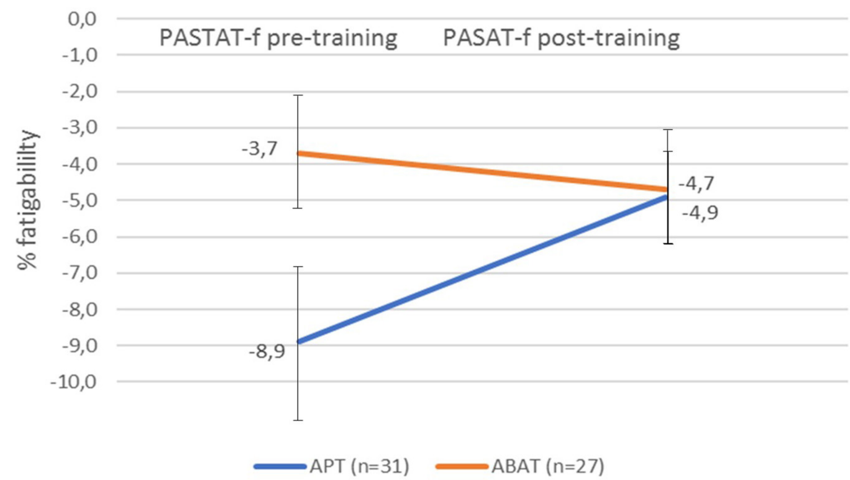

FIGURE 2 | Fatigability pre-training and post-training as measured by the PASAT-f for both treatment groups. Fatigability values represent the percentage of correct answers in the first quintile of the PASAT subtracted from the number of correct answers in the last quintile of the PASAT. A negative value indicates fatigability. The error bars represent SD. APT, Attention Process Training; ABAT, Activity-Based Attention Training; PASAT-f, Paced Auditory Serial Addition Test fatigability.

at baseline, 6 subjects (21%) in the ABAT group reached the ceiling in the first quintile and 1 subject (4%) in the last quintile, while in the APT group 5 subjects (16%) reached the ceiling in the first quintile and 0 in the last quintile. The Chi2 test revealed no significant differences in ceiling effect between the groups, either in the first (*Chi2 = 0.272, p = 0.602*) or in the last quintile (*Chi2* = 2.351, *p* = 0.125).

After treatment, in the ABAT group, 12 subjects in the first quintile (43%) obtained the maximal score and 8 subjects (29%) in the last quintile. In the APT group, 20 subjects (64%) obtained the maximal score in the first quintile and 5 subjects (16%) in the last quintile. No significant differences between the groups was found for either quintile; (*Chi2* = 2.351, *p* = 0.125), (*Chi2* = 1.513, *p* = 0.219).

DISCUSSION

The aims of this study were to evaluate the feasibility of attention training in reducing CF after ABI, and to investigate whether targeted attention training, APT, had a better effect on CF compared to standard activity-based training (ABAT) in the subacute phase after acquired brain injury.

A significant improvement was observed for both types of cognitive training, which was measured as improved performance in the automatic and controlled speed conditions in RUFF 2 & 7, indicating a positive effect of attention training on processing speed. Furthermore, we found that CF, defined as declining performance in terms of increased number of incorrect responses on PASAT, significantly decreased after training in the APT group, but not in the ABAT group.

However, the analysis of covariance revealed that the difference in fatigability-outcome between the groups was explained by differences at baseline. These results indicate that attention training has a better effect on CF in patients with higher levels of attention dysfunction at baseline than in those with milder attention impairment. Whether APT is superior to ABAT remains unclear.

The relationship between the impairment level and the rehabilitation effect may have important clinical implications for brain injury rehabilitation. Previous studies in geriatric populations show an association between lower baseline performance in a cognitive domain and greater gains after cognitive training in that same domain (Roheger et al., 2019). On the other hand, there are studies showing higher baseline scores to be predictive of cognitive training benefits in older subjects (McKitrick et al., 1999; Fairchild et al., 2013).

A possible interpretation of the results could be that external meta-cognitive support offered by the therapist, inherent in the APT-method, might benefit the lower-level performers. An alternative explanation could be that the observed difference in the results between the groups is a mere effect of the statistical phenomenon "regression to the mean" (Bland and Altman, 1994), the initially weaker group having more room to improve than the higher performing group. Partly speaking against this is the fact that we did not find any significant differences between the groups in terms of the number of subjects that reached the ceiling of PASAT at baseline or at follow up, neither in the first nor the last quintiles of the test. However, it is undeniable that there were subjects in both groups that might have had the capacity for further improvement, as several participants reached the maximum performance level on the PASAT.

The assumption that deficiency in sustained attention and information processing speed are crucial in the development of fatigue (Kohl et al., 2009) and CF (Schwid et al., 2003) was confirmed by the significant baseline correlations between PASAT-f and Ruff 2 & 7 ADS for the total group. The correlation between Digit Span, a simple measure of working memory and attention span, and PASAT-f might further support this presumption.

Surprisingly, we found no correlation for the cognitively demanding controlled speed condition of Ruff 2&7 (CSS) with fatigability, which would have been expected from a model suggesting that CF is more sensitive to tasks demanding cognitive control than automatically executed tasks (DeLuca, 2005; Lorist et al., 2005). However, as fatigue is considered domain specific (Kluger et al., 2013), successful performance on PASAT might be more dependent on sustained attention than on selective attention. Another explanation could have been a wider performance range on the Ruff CSS measure, but that was not the case.

No baseline correlation was found between anxiety and depression and PASAT-f, which is consistent with the findings of Möller et al. (2014) and supports the notion that objectively measured CF might not be as influenced by emotional states, as self-assessed fatigue (Arnold, 2008; Möller et al., 2014). Hence, the PASAT-f measure could be suitable for an investigation of the underlying mechanisms of fatigue in brain injury that are not related to depression.

From a methodological point of view, one could question the choice of partitioning the fatigability measure, PASAT-f, into quintiles, as the narrow ranges, given the limited task length, increased the risk of ceiling effects. Previously PASAT has been divided in different ways to capture fatigability. Sometimes the performance on the first half of the PASAT has been compared with the last half (Walker et al., 2012), sometimes the first third has been compared with the last third (Morrow et al., 2015). In this study, a division into quintiles, previously applied in studies where CF has been measured with psychomotor vigilance tasks (Möller et al., 2017; Berard et al., 2019) was carried out with the purpose of making the instrument sensitive to changes between the beginning and the end. A division into halves or thirds would have given more room for improvement, but at the cost of possible sensitivity loss.

An alternative approach to the assessment of CF has been to focus on variability in performance over time, rather than mean performance decrement, where higher degrees of variability are hypothesised to be linked to dysfunctions in cognitive control mechanisms (Wang et al., 2014). In this study we did observe a correlation between reduced CF assessed with PASAT-f and reduced intraindividual variability in PASAT. The measures are interdependent though, preventing firm conclusions from being drawn, and it is noteworthy that the variability did not correlate with the independent attention measures (Ruff 2 & 7 and Digit Span). Response time variability as a measure of CF has not been much used in studies on patients with stroke or TBI, as opposed to decrement-measures. However, this approach is of particular interest, since response-time variability, in contrast with performance-decline measures, has been shown to significantly correlate with subjectively reported fatigue in patients with MS (Bruce et al., 2010), and also in patients with mild TBI (Möller et al., 2017). In a future study it would be interesting to investigate the correlation between variability in performance and performance decline in brain injured patients more closely and to unravel whether variability in performance might be more closely related to subjective fatigue experience than objectively measured performance decrement.

Limitations

The study has some methodological weaknesses, apart from the issue regarding the principles for partitioning the PASAT-f discussed above. Due to the fact that the data was collected from a clinical trial not targeting CF (Bartfai et al., 2014), CF was not an inclusion criterion for participation. However, as CF is a cardinal symptom after ABI, we assumed that the randomisation will ensure an equal distribution in both groups. A preselection of patients with CF would have reduced the baseline difference between the intervention groups, thereby making comparisons of the results of the interventions for CF clearer and more convincing.

Secondly, the influence of spontaneous recovery on training effects in the early stage after ABI could be regarded as a limitation. However, both groups were in the same stage of recovery and, thus, those effects could be assumed to be similar. This problem could have been remedied by including a control group receiving no treatment, however, ethical issues preclude withholding treatment when it is available.

The interpretation of the PASAT results is slightly problematic. An initial practice effect has earlier been demonstrated (Tombaugh, 2006) with repeated administration. Therefore, we cannot rule out that some of the improvement was related to a practice effect.

Also, neither the patients nor the therapists were blinded to the type of intervention, which might have affected the results through placebo effects, even though different therapists were in charge of treatment and assessment. Lastly, it should be noted that the strict enrolment criteria used in this study (Markovic et al., 2017) demand caution in the generalisation of the results.

CONCLUSION

The advantage of this randomised controlled study is that it addresses cognitive training as a possible method for reducing CF – an area where studies are currently lacking. It also suggests that it might be feasible to reduce CF through attention training in patients with acquired brain injury. It can, therefore, inspire future studies, where objective measures are used as a complement to self-assessment scales to measure fatigue. The study also indicates that patients with high levels of CF might improve more from attention training than patients who have less severe CF. Whether structured or activity-based attention training is provided appears to be less important. Due to methodological drawbacks the results are tentative and future studies are required to confirm the validity of the findings. Such studies should include only patients exhibiting CF and the results of the intervention groups should be compared with the result of a control group receiving no attention training.

DATA AVAILABILITY STATEMENT

The raw data supporting the conclusions of this article will be made available by the authors, without undue reservation.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by the Ethics Review Board Stockholm (2007/1363-31 and 2012/3: 4). The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

AB and GM were responsible for study design. GM implemented the research. MM conceptualized the fatigability measure and did the statistical analyses and prepared the figures. AH was the first author and did the manuscript draft. All authors contributed to the manuscript.

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A Reinforcement Learning Approach to Understanding Procrastination: Does Inaccurate Value Approximation Cause Irrational Postponing of a Task?

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Procrastination is the voluntary but irrational postponing of a task despite being aware that the delay can lead to worse consequences. It has been extensively studied in psychological field, from contributing factors, to theoretical models. From value-based decision making and reinforcement learning (RL) perspective, procrastination has been suggested to be caused by non-optimal choice resulting from cognitive limitations. Exactly what sort of cognitive limitations are involved, however, remains elusive. In the current study, we examined if a particular type of cognitive limitation, namely, inaccurate valuation resulting from inadequate state representation, would cause procrastination. Recent work has suggested that humans may adopt a particular type of state representation called the successor representation (SR) and that humans can learn to represent states by relatively low-dimensional features. Combining these suggestions, we assumed a dimension-reduced version of SR. We modeled a series of behaviors of a "student" doing assignments during the school term, when putting off doing the assignments (i.e., procrastination) is not allowed, and during the vacation, when whether to procrastinate or not can be freely chosen. We assumed that the "student" had acquired a rigid reduced SR of each state, corresponding to each step in completing an assignment, under the policy without procrastination. The "student" learned the approximated value of each state which was computed as a linear function of features of the states in the rigid reduced SR, through temporal-difference (TD) learning. During the vacation, the "student" made decisions at each time-step whether to procrastinate based on these approximated values. Simulation results showed that the reduced SR-based RL model generated procrastination behavior, which worsened across episodes. According to the values approximated by the "student," to procrastinate was the better choice, whereas not to procrastinate was mostly better according to the true values. Thus, the current model generated procrastination behavior caused by inaccurate value approximation, which resulted from the adoption of the reduced SR as state representation. These findings indicate that the reduced SR, or more generally, the dimension reduction in state representation, can be a potential form of cognitive limitation that leads to procrastination.

Keywords: procrastination, value-based decision making, reinforcement learning, temporal difference learning, state representation, successor representation, dimension reduction

INTRODUCTION

Delaying a task until the last minute and struggling to meet the due date is not an enjoyable thing to do. While sometimes people do this because it is inevitable or the better choice to be made, there are also other times when people voluntarily postpone the task when it could be and would better to be avoided. This irrational but voluntary delay of a course of action is known as procrastination. Previous studies have suggested that such behavior can result in not only worse academic or working performances, but also anxiety and stress in the procrastinators (e.g., Day et al., 2000; Stead et al., 2010). Procrastinators can be fully aware of the bad consequences that could potentially arise, as it was mentioned that most of procrastinators wish to reduce procrastination [mentioned in Steel (2007) by citing (O'Brien, 2002)]. The question is then raised why humans would make such seemingly irrational decisions in the first place, even when they know that such postponing could potentially worsen the situation.

Both task characteristics, such as task aversiveness and timing of rewards and punishments, and certain personality traits, such as lack of self-control and high degree of impulsivity, have been found to contribute to procrastination behavior (Steel, 2007). As it happens when the long-term and distant values give way to immediate experiences, it is also interpreted as a form of self-regulation failure (Rozental and Carlbring, 2014).

Along with these empirical findings, researchers also set out to build theoretical frameworks of procrastination. In particular, Temporal Motivation Theory (Steel and König, 2006) has been proposed as a comprehensive formulation of the mechanisms underlying procrastination. Derived from expectancy theory and hyperbolic discounting, the theory describes one's motivation to complete a task by integrating the expectancy and the value of a task, divided by the time delay and the impulsiveness (i.e., one's sensitivity to the delay). More recently, integrating the Temporal Motivation Theory and the self-regulation failure perspective, the temporal decision model (Zhang et al., 2019b) has been proposed. This model explicitly incorporates engagement utility or task aversiveness as an important factor related to procrastination.

Referring to these existing models, in the present study, we attempt to model procrastination from a different perspective, which is value learning and value-based decision-making. When faced with a task, whether to finish it now or to procrastinate until later is indeed a decision to be made. As mentioned above, one suggested reason for procrastination is because the procrastinators fail to prioritize values in the distant future (i.e., "delay" as in Temporal Motivation Theory), and choose immediate values instead. Task aversiveness considered in the temporal decision model, or effort cost for task engagement, should entail negative values. How humans learn and integrate these values to choose whether to procrastinate or not would thus be an interesting question in terms of value learning and value-based decision making.

Value leaning and value-based decision making, including those involving effort cost, have been widely studied in humans (e.g., Croxson et al., 2009; Kool et al., 2010; Skvortsova et al., 2014; Nagase et al., 2018; Lopez-Gamundi et al., 2021) as well as in animals (e.g., Salamone et al., 1994; Walton et al., 2003; Floresco et al., 2008; Gan et al., 2010; Cai and Padoa-Schioppa, 2019). These behaviors and their neural mechanisms have been modeled (e.g., Niv et al., 2007; Collins and Frank, 2014; Kato and Morita, 2016; Möller and Bogacz, 2019) using the framework of reinforcement learning (RL) (Sutton and Barto, 1998). It is grounded by accumulated suggestions in the past few decades that human and animal behavior can be approximated by RL models, certain neural signals appear to represent RL variables [in particular, dopamine's encoding of reward prediction error (RPE) (Montague et al., 1996; Schultz et al., 1997) and striatal encoding of action values (Samejima et al., 2005)], and cortico-basal ganglia circuits could implement RL and action selection mechanisms (e.g., Doya, 1999; Frank et al., 2004; Lo and Wang, 2006; Khamassi and Humphries, 2012; Helie et al., 2013; Morita et al., 2016; see Niv and Montague, 2008; Lee et al., 2012 for a comprehensive review). It is thus reasonable to consider procrastination, a behavior also involving the process of value-based decision-making, on the basis of RL.

There have already been studies applying RL to procrastination (Lieder and Griffiths, 2016; Lieder et al., 2019). In their study, procrastination was considered to be a choice of the inferior option with larger proximal reward but smaller overall value due to, as suggested by the authors, cognitive limitations. They then proposed an innovative idea based on the RL theory, which was adding "pseudo-rewards" so that the optimal option will always have the maximal proximal reward (original + pseudo) and can be chosen even by the most short-sighted decision maker with cognitive limitations. The authors demonstrated in behavioral experiments with human subjects that their method successfully reduced procrastination resulting from myopic decisions.

It has, however, remained elusive exactly how (and what) cognitive limitations lead to a non-optimal choice (i.e., choice of an action whose true value is smaller than that of the optimal action). It has been suggested in the RL framework (Daw et al., 2005; Dolan and Dayan, 2013) that humans show both goal-directed and habitual behaviors, potentially approximated by model-based and model-free RL, respectively. The habitual or model-free behavior is suggested to be computationally efficient but less flexible, which in a sense reflects cognitive limitations and potentially underlies unhealthy behaviors (Story et al., 2014). Recent studies (Momennejad et al., 2017; Russek et al., 2017) have shown that humans may have also adopted an intermediate behavior between goal-directed/model-based and habitual or model-free behaviors by using a particular type of state representation named the successor representation (SR) (Dayan, 1993). As an intermediate type between model-based and model-free RL, SR-based behavior is more flexible than model-free RL, but still has some limitations as compared to fully model-based RL.

Another possible source of cognitive limitations would be dimension reduction in state representation in the brain (Gershman and Niv, 2010; Niv, 2019). As there is a tremendous number of states in the environments surrounding the humans that should not be able to be individually represented in the human brain, some sort of dimension reduction is thought to be necessary. Although low-dimensional representation can be efficient (Niv, 2019), dimension-reduced representations of states can inevitably be inadequate. For example, representing the agent's position in the three-dimensional space by two-dimensional (x and y) coordinates cannot tell at what height (altitude) the agent exists. Inadequate state representation could cause inaccurate valuation and lead to non-optimal choice behavior.

Combining these notions, in the present study, we considered that humans may adopt a dimension-reduced version of SR (Gehring, 2015; Barreto et al., 2016; Gardner et al., 2018), in particular, the goal-based reduced SR (Shimomura et al., 2021) (see section "Methods"). We explored whether and how an RL model with the reduced SR generated procrastination behavior. More specifically, we examined if procrastinating choice, which is non-optimal in terms of true values, can nevertheless be optimal in terms of approximated values based on the approximation of state values as a linear function of features in the reduced SR in a model of Student's behavior during vacation after a school term.

METHODS

Modeling the Student's Behavior in the School Term and the Vacation Period

We simulated a situation where a student experienced the school term and then started the vacation. The student, who was not allowed to procrastinate while working on assignments in the classroom during the school term, became able to choose freely whether to procrastinate while working on assignments at home during the vacation. We modeled the Student's behavior of working on each single set of assignments (e.g., a set of math problems or short essays) by an episode of actions of an agent moving from the start state to the goal state (**Figure 1A**). As shown in **Figure 1A**, we assumed five states, and this could potentially represent the following situation, for example: each set of assignment requires about an hour of concentration (focused attention) in total, and if the student can be continuously focused for 10–15 min, s/he needs about 4–6 times of concentration, each of which could correspond to each state (except for the goal state) in our model. Notably, however, there is a study (Wilson and Korn, 2007) arguing that the frequently claimed 10–15 min duration for Student's attention during lectures was hardly supported by the literature, and here we considered it just as an intuitive example. At each episode, the agent started from the start state, and selected at each time-step whether to go to the next state ("GO" action) with cost imposed, or stay at the current state ("STAY" action) with no cost, until reaching the goal state, where reward could be obtained (the sequential "GO" and "STAY" architecture is shared with the model of Shimomura et al. (2021) dealing with addiction, but the cost for "GO" action was introduced in the present model). The agent initially experienced 20 episodes, corresponding to the school term, under the policy of choosing "GO" at all states (i.e., without any procrastination). Subsequently, the agent experienced another 20 episodes, corresponding to the vacation period, during which the agent chose "GO" or "STAY" according to the approximated values (described below).

Notably, the "school-term/vacation" paradigm is not necessarily limited to the literal school-term or vacation. More generally, the "school-term" period could potentially simulate an "in-class" situation where the student is under supervision by the teacher or supervisor and needs to take actions under instruction. The "vacation" period, on the other hand, could potentially be analogous to a situation outside of the class where the student has the freedom to take actions.

Goal-Based Reduced Successor Representation (SR) of States

As described in the Introduction, based on the recent suggestions of SR and dimension reduction in state representation in the brain, we assumed that the agent had acquired a dimension-reduced version of SR, specifically, the goal-based reduced SR (Shimomura et al., 2021) of each state under the policy without procrastination taken in the school term (**Figure 1B**). Specifically, we considered the discounted future occupancy of the final successor state (i.e., the goal state) under the policy of choosing "GO" at all states as the feature variable representing each state. Feature variable x for k-th state S_k ($k = 1, \ldots, n$; S_n corresponds to the goal state, and $n = 5$ was assumed) was assumed to be:

$$x(S_k) = \gamma^{n-k}$$

where γ is the time discount factor ($\gamma = 0.85$ was assumed in most simulations, but we also examined a case with $\gamma = 0.95$). We assumed that this representation had already been established at the beginning of the initial 20 episodes of the school term that were simulated, and that it was rigid enough to remain unchanged even after the vacation period began and the agent started to also choose "STAY," although later we also examined the case where the reduced SR was slowly updated during the vacation period.

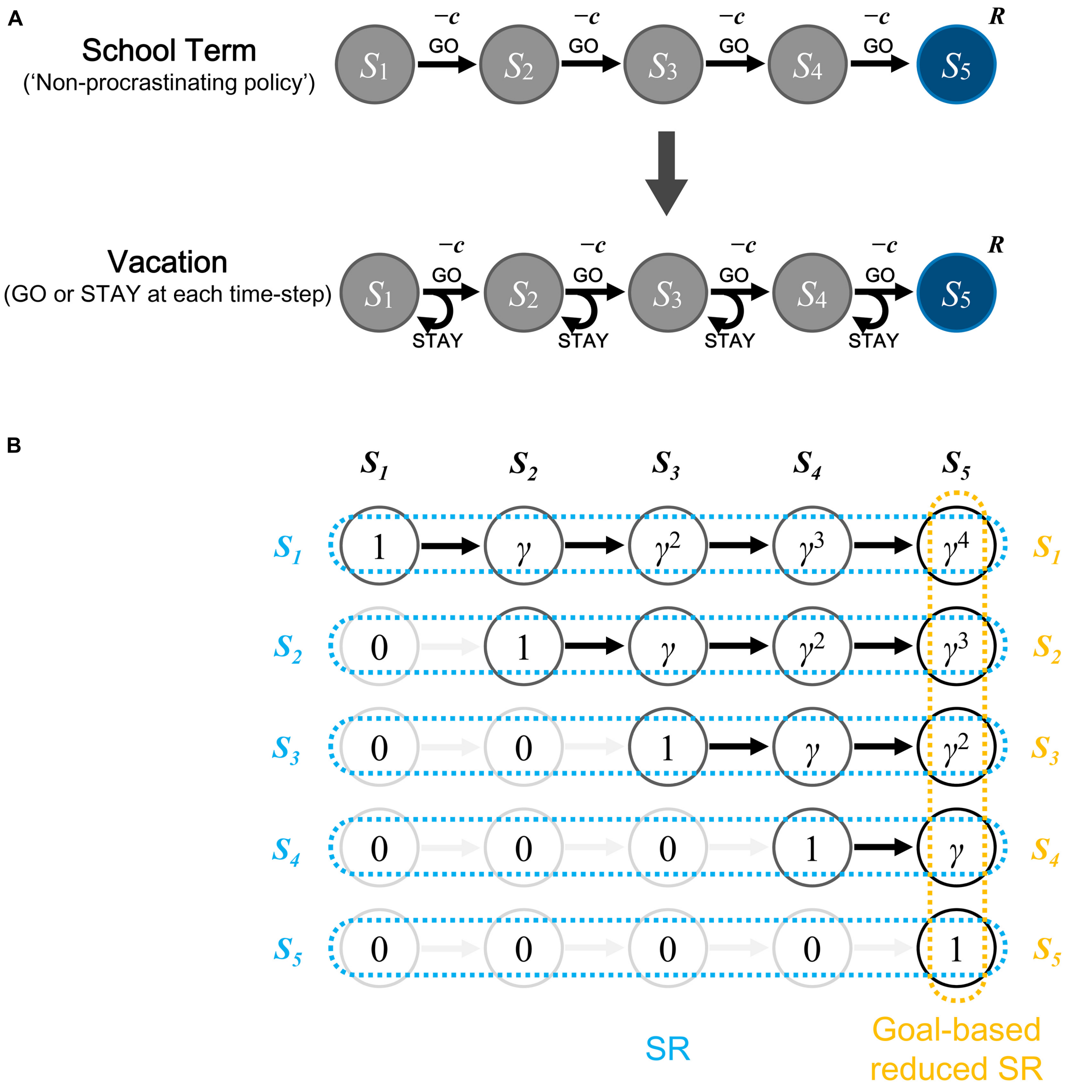

FIGURE 1 | Schematic diagrams of the model and the reduced successor representation (SR). **(A)** Schematic diagrams of the model. There are 5 states for each episode, with S_1 the start state and S_5 the goal state. The student first experiences 20 episodes of school term, choosing "GO" at all time-steps ("non-procrastinating policy"), and then enters vacation for another 20 episodes during which the choice to "STAY" or "GO" (i.e., to procrastinate or not) is made according to the approximated values of these actions. Cost (c) is imposed for "GO" action and reward (R) is given at the goal state (S_5). **(B)** Schematic diagram of the SR and the goal-based reduced SR under the policy without procrastination. The SR is the way to represent each state by a set of discounted future occupancies of all the states, i.e., to represent S_1 as $(1, \gamma, \gamma^2, \gamma^3, \gamma^4)$ (where γ is the time discount factor), S_2 as $(0, 1, \gamma, \gamma^2, \gamma^3)$, S_3 as $(0, 0, 1, \gamma, \gamma^2)$, S_4 as $(0, 0, 0, 1, \gamma)$, and S_5 as $(0, 0, 0, 0, 1)$, as indicated by the light blue marks. The goal-based reduced SR is the way to represent each state by the discounted future occupancy of only the goal state, i.e., to represent S_1 as γ^4, S_2 as γ^3, S_3 as γ^2, S_4 as γ, and S_5 as 1, as indicated by the orange marks.

Approximated State Values Based on the Reduced SR, and Their Updates

The agent was assumed to approximate the state value of state S_k under the policy that the agent was actually taking by a linear function of the feature variable x:

$$\tilde{v}(S_k) = wx(S_k)$$

where $\tilde{v}(S_k)$ denotes the approximated state value of S_k. Such an approximation of value function by a linear function of features has been made as a standard assumption (Montague et al., 1996; Schultz et al., 1997). It can potentially be implemented through dopamine-dependent plasticity in the brain. The coefficient w was updated through temporal difference (TD) learning at each time-step:

$$\delta(t) = r(t) + \gamma\tilde{v}(S(t+1)) - \tilde{v}(S(t))$$

$$w \leftarrow w + a\delta(t)x(S(t))$$

where $\delta(t)$ denotes the TD reward prediction error (RPE), $S(t)$ the state at time t, $r(t)$ the reward or cost [modeled as negative $r(t)$] obtained at time t, and a, the learning rate. The reward/cost

$r(t)$ was assumed to be $R = 1$ when the agent reached the goal state, -c (representing the cost) when the agent chose "GO," and 0 otherwise. The cost amount c was assumed to be 0.1 in most simulations, but we also examined the cases with c = 0, 0.01,..., 0.15. In most cases shown in the Results, the learning rate a was assumed to decrease over episodes (m = 1, . . ., 20):

$$a = 0.5/(1 + 0.2m),$$

simulating habituation to the situation, in both the initial 20 episodes corresponding to the school term and the subsequent 20 episodes corresponding to the vacation period (i.e., the learning rate was assumed to once increase at the beginning of the vacation period). We also examined the cases where the learning rate was constant at 0.2 or 0.4 in both school term and vacation period. The initial value of w for the initial 20 episodes (the school term) was set to 0, and for the subsequent 20 episodes (the vacation period), was set to the final value of w at the end of the initial 20 episodes.

Approximated Action Values Based on the Reduced SR, and Action Selection

As mentioned above, we assumed that the agent initially experienced 20 episodes during the school term under the policy of choosing "GO" at all states (i.e., without any procrastination). Subsequently, action "GO" or "STAY" was selected at each time-step according to their approximated values in a soft-max manner. We assumed that the agent computed the approximated values of the actions "GO" and "STAY" at state S_k (k = 1,., 4) by using the approximated state values under the policy that the agent was taking (described above) as follows:

$$\tilde{q}(S_k, GO) = \gamma\tilde{v}(S_{k+1}) - c$$

$$\tilde{q}(S_k, STAY) = \gamma\tilde{v}(S_k)$$

Action was then assumed to be selected according to the following probability:

$$Prob(A) = e^{b\tilde{q}(S_k,A)} \ / \ \{e^{b\tilde{q}(S_k,GO)} + e^{b\tilde{q}(S_k,STAY)}\}$$

where A is "GO" or "STAY," and b is a parameter representing the inverse of the degree of exploration (i.e., inverse temperature). In most cases shown in the Results, the inverse temperature was assumed to be constant at 20. We also examined the cases where the inverse temperature was 10 or 30.

True State/Action Values

We explored if the agent's behavior, determined by the approximated values based on the reduced SR, could be said to be irrational in reference to true values under the policy that the agent was taking. The true state value under the policy without procrastination for the initial 20 episodes (i.e., without "STAY") can be exactly calculated as:

$$v(S_k) = \gamma^{n-k}R - C_k$$

where R represents the reward at the goal state, assumed to be 1 as mentioned above, and C_k stands for the summation of all the discounted future costs:

$$C_1 = c + \gamma c + \gamma^2 c + \gamma^3 c$$

$$C_2 = c + \gamma c + \gamma^2 c$$

$$C_3 = c + \gamma c$$

$$C_4 = c$$

After the initial 20 episodes, the agent could freely select an action and therefore the true state values under the policy that the agent was taking should change accordingly. We considered that the agent (or the agent's brain) could potentially estimate these values by using TD learning based on individual representation of states, in parallel with the reduced SR-based TD learning described above. Specifically, we assumed that the estimated true state value under the policy that the agent was taking $\hat{v}(S)$ was updated as:

$$\delta'(t) = r(t) + \gamma\hat{v}(S(t+1)) - \hat{v}(S(t))$$

$$\hat{v}(S(t)) \leftarrow \hat{v}(S(t)) + a\delta'(t)$$

with the initial values for $\hat{v}(S)$ set to the abovementioned true state values under the non-procrastinating policy. Then, given these estimated true state values, estimated true action values under the policy that the agent was taking were calculated as:

$$\hat{q}(S_k, GO) = \gamma\hat{v}(S_{k+1}) - c$$

$$\hat{q}(S_k, STAY) = \gamma\hat{v}(S_k)$$

Apart from the state/action values under the policy that the agent was taking, we can also consider the optimal state/action values, i.e., the state/action values under the optimal policy, as defined in the RL theory (Sutton and Barto, 1998). In our model with the abovementioned standard parameter values (n = 5, γ = 0.85, R = 1, and c = 0.1), the optimal policy is considered to be the non-procrastinating policy (i.e., without choosing "STAY"), because taking a "STAY" results in one more time-step discounting of the reward and all of the future costs whose (discounted) sum is positive. We considered that the agent (or the agent's brain) could also potentially estimate the optimal action values based on individual representation of actions, for example, if Q-learning can be implemented in the brain (c.f., Roesch et al., 2007; Morita, 2014; Morita et al., 2016). On the other hand, it would be difficult for the agent to approximate the optimal action values based on the reduced SR, given that approximation of value function as a function of features is harder for off-policy, than for on-policy, learning (c.f., chapter 11 of Sutton and Barto, 2018).

"Penalty," or "Regret," for Taking Action "STAY"

We also conducted separate sets of simulations, in which a "penalty" for "STAY" choice depending on the elapsed time, or an unpredictable "regret" for "STAY" choice, was added to the original model. The "penalty" term was introduced to simulate the devaluation of "STAY" choice caused by the pressure to procrastinate as the deadline approaches and/or the elapsed time increases. We added "$-\ p(t_v)c_p$" to the approximated value of "STAY" used for action selection and the true value of "STAY," as well as the TD RPEs [$\delta(t)$ and $\delta'(t)$] upon taking "STAY." The parameter c_p controls the amount of the "penalty," which was set to 0.1, and $p(t_v)$ is a function of time step in the vacation period (t_v) that is 0 until t_v becomes a certain value, specifically, 150 time-steps, and thereafter linearly increases, specifically, according to $(t_v - 150)/150$.

The unpredictable "regret" term, on the other hand, was added to simulate "the sense of guilty" after choosing "STAY" (i.e., procrastinating). Different from the "penalty" for the "STAY" choice, the "regret" term was not added to the approximated value of "STAY" used for action selection, but only added to the true value of "STAY" as well as the TD RPEs [$\delta(t)$ and $\delta'(t)$] upon taking "STAY," in order to simulate that regret only showed up after "STAY" had been chosen. Specifically, we added "$-\ c_r$" to the true value of "STAY" and the TD RPEs [$\delta(t)$ and $\delta'(t)$] upon taking "STAY," where c_r is a parameter representing the amount of the "regret," which was set to 0.02.

Slow Updates of the Reduced SR During Vacation

As mentioned above, so far, we assumed the goal-based reduced SR to be rigid and remaining unchanged in the vacation period. However, we also examined the case where the reduced SR was slowly updated during vacation. In the reduced SR, each state is represented by its feature variable that is the discounted future occupancy of the goal state, which can be said to be a sort of temporal proximity to the goal. As the agent changes the policy from the non-procrastinating one to the procrastinating one, the agent will need more time to reach the goal state, and thus the temporal proximity to the goal state will change (decrease). If the reduced SR changes according to the change in the policy, the feature variable for each state should also change accordingly. Such a change in the reduced SR can be done through TD learning (Shimomura et al., 2021), in the same manner as in the case of the genuine SR (Gershman et al., 2012). Specifically, the feature variable for state $S(t)$ [i.e., $x(S(t))$] other than the goal state was updated by $\alpha_{SR}\delta_{SR}(t)$, where $\delta_{SR}(t) = \gamma x[S(t+1)] - x[S(t)]$ was the TD error for the feature variable and α_{SR} was the learning rate for this update, which was set to 0.05.

Simulations

Simulations were conducted 10,000 times for each condition by using MATLAB.

RESULTS

Learning of the Reduced SR-Based Approximated Values During the School Term

Figure 2A shows the change of the coefficient "w" of the reduced SR-based approximated state value function during the school term, in which the agent was assumed to take the policy of choosing "GO" all the time. The agent transitioned from state S_1 (corresponding to the leftmost in **Figure 2A**) to the goal (state S_5, rightmost) in each episode from the first episode (topmost) to the 20th episode (bottommost), and each color square in **Figure 2A** indicates the value of "w" at the timing just after the agent left the corresponding state in the corresponding episode. **Figure 2B** presents the same data in a different way: each line indicates the over-episode change of "w" just after the agent left each state. As shown in the figures, the coefficient "w" generally increased over the episodes, while there was a gradual decrease from S_1 to S_4, followed by a sharp rise at the goal state (S_5), in each episode. After the update for 20 episodes, the coefficient at every state showed a tendency to gradually approach a stable value. This indicates that the agent gradually learnt, through the TD learning, the reduced SR-based approximated state values.

Procrastination Behavior at the First Episode in the Vacation Period

Figure 3Aa shows the difference between the values of actions "GO" and "STAY" at each state at the beginning of the first episode in the vacation period, for the true values under the non-procrastinating policy (i.e., choosing "GO" only) (black line) or the reduced SR-based approximated values (red line), averaged across simulations. As shown in the figure, the true value of "GO" was larger than the "STAY" value at every state, and this gap widened as the agent approached the goal state. In contrast, the reduced SR-based approximated value of "GO" was smaller than that of "STAY" at all states, though this gap narrowed as the agent approached the goal state. This contradiction indicates that the agent behaving according to the approximated values should make irrational choices of "STAY," i.e., procrastination. Specifically, although the action "GO" had larger values than "STAY" in terms of the true values, the reduced SR-based approximated values of "GO" were smaller than those of "STAY," and thus the agent should tend to choose "STAY" more frequently than "GO."

Notably, the agent was assumed to update the approximated values at every time step (to approximate the values under the policy that the agent was taking) and make choices according to such continuously updated approximated values. **Figure 3Ab** shows the difference between the approximated values of "GO" and "STAY" at the time when the agent initially entered each state in the first episode in the vacation period, averaged across simulations. The value at S_1 in this figure indicates the value at the beginning of the vacation, which is the same as the one shown in **Figure 3Aa**, but the average values at S_2–S_4 deviate from the values in **Figure 3Aa**, reflecting the continuous updates of the approximated values.

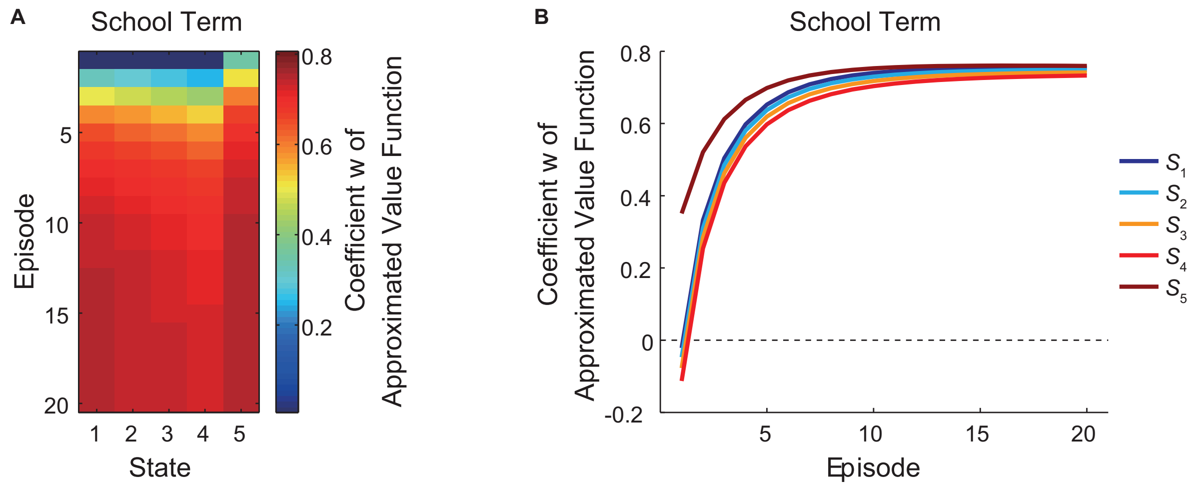

FIGURE 2 | Learning of the reduced SR-based approximated values during the school term. The change of the coefficient "*w*" of the reduced SR-based approximated state value function during the school term is shown in two different ways. **(A)** The horizontal axis indicates states ($S_1 \sim S_5$, from the left to the right), and the vertical axis indicates the episode (1st∼20th, from the top to the bottom). The color of each square indicates the value of "*w*," according to the right color bar, at the timing just after the agent left the corresponding state in the corresponding episode. **(B)** Each line indicates the change of "*w*" just after the agent left each state (indicated by the different colors shown on the right) over episodes (horizontal axis).

Figure 3B shows the mean number of times for the agent to choose "STAY" at each state at the first episode in the vacation period, averaged across simulations, and **Figure 3C** shows the distribution of the number of times of "STAY." As expected from the larger approximated values of "STAY" than the values of "GO," the agent made more than one "STAY" at every state on average. As approaching the goal state, the tendency of procrastination gradually decreased, and this can also be expected from the decrease in the difference between the approximated values of "GO" and "STAY" across states. Notably, however, as shown in **Figure 3C**, the distributions of the number of times of "STAY" for the four states were wide and skewed, and largely overlapped with each other.

Changes in the Reduced SR-Based Approximated Values During the Vacation Period

Figure 4A shows the change of the coefficient "*w*" of the reduced SR-based approximated state value function under the policy that the agent was taking, averaged across simulations, during the vacation period, in which "GO" and "STAY" could be chosen freely. **Figure 4B** presents the same data in a different way: each line indicates the over-episode change of "*w*" just after the agent left each state. As shown in the figures, for each state, the coefficient "*w*" generally decreased during the vacation period, while there is again a gradual decrease from S_1 to S_4 and a sharp rise at the goal state (S_5) in each episode. This general decrease across episodes indicates that the reduced SR-based approximated state values under the policy that the agent was taking became lowered during the vacation period, and this is considered to reflect that the policy itself gradually changed as we will see below.

Changes in the Procrastination Behavior During the Vacation Period

The red lines in **Figure 5A** show the over-episode changes of the difference between the reduced SR-based approximated values of actions "GO" and "STAY" under the policy that the agent was taking at entering each state, and the red line in **Figure 5B** shows the value difference at the 20th episode, averaged across simulations. **Figure 5C** shows the over-episode changes of the mean number of times for the agent to choose "STAY" at each state, and **Figures 5D,E** show the mean number of times to choose "STAY" at the 20th episode, averaged across simulations, and its distribution, respectively. As shown in **Figure 5A**, the difference between the approximated values of "GO" and "STAY" at entering every state widened over episodes (i.e., became more negative). Reflecting this, there is a clear trend of increasing in the tendency of procrastination behavior over episodes (**Figure 5C**). Meanwhile, the decreases in the absolute difference of the approximated values of "GO" and "STAY" and in the procrastination tendency across states within an episode remained consistent across episodes. It can thus be said that the agent's procrastination behavior was reduced as getting closer to the goal state but was generally getting worse across the episodes.

The black lines in **Figure 5A** show the over-episode changes of the difference between the estimated true values of actions "GO" and "STAY" under the policy that the agent was taking at entering each state, and the black line in **Figure 5B** shows the value difference at the 20th episode, averaged across simulations. As shown in the bottom panel of **Figure 5A**, the "GO"—"STAY" difference in the estimated true values at entering S_4 increased across episodes. By contrast, as shown in the top panel of **Figure 5A**, the "GO"—"STAY" difference at entering S_1 decreased across episodes, and eventually became negative, as also appeared in **Figure 5B**. This indicates that at this point, choosing "STAY"

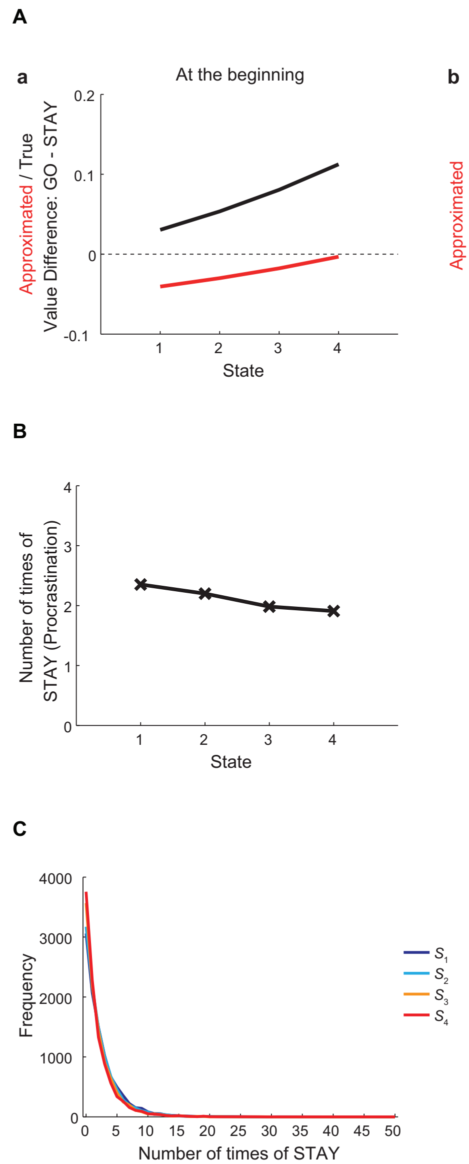

FIGURE 3 | Procrastination behavior at the first episode in the vacation period. **(A)** (a) The difference between the values of actions "GO" and "STAY" at each state at the beginning of the first episode in the vacation period, for the true values under the non-procrastinating policy (black line) or the reduced SR-based approximated values (red line), averaged across simulations. (b) The difference between the approximated values of actions "GO" and "STAY" at the time when the agent initially entered each state in the first episode in the vacation period. The error bars at S_2–S_4 indicate the average ± standard deviation (SD) across simulations [the value at S_1 indicates the value at the beginning of the first episode in the vacation period, which is the same as the one shown in (a)]. **(B,C)** The across-simulation mean **(B)** and distribution **(C)** of the number of times for the agent to choose "STAY" at each state at the first episode in the vacation period.

at S_1 has finally become a choice of a higher-(estimated)-true-value option under the procrastinating policy that the agent was actually taking. Notably, however, the optimal policy for the agent, in terms of the RL theory, is the non-procrastinating policy (choosing "GO" only) as mentioned in section "Methods," and the true action value of "GO" under the optimal policy (i.e., the optimal action value of "GO") was higher than that of "STAY," as shown in the black line in **Figure 3A** and the leftmost point of the black line in **Figure 5A**, regardless of the policy that the agent was actually taking.

Dependence of the Procrastination Behavior on the Cost of "GO" Action

So far, we assumed that the amount of cost imposed on each "GO" action was 0.1, which was one tenth of the amount of reward obtained at the goal. Next, we varied the amount of cost while the amount of reward was fixed and observed how the agent's behavior changed. **Figure 6A** shows how the mean number of times for the agent to choose "STAY" at each state at the first episode in the vacation period, averaged across simulations, changed when the amount of cost was varied. **Figure 6B** shows

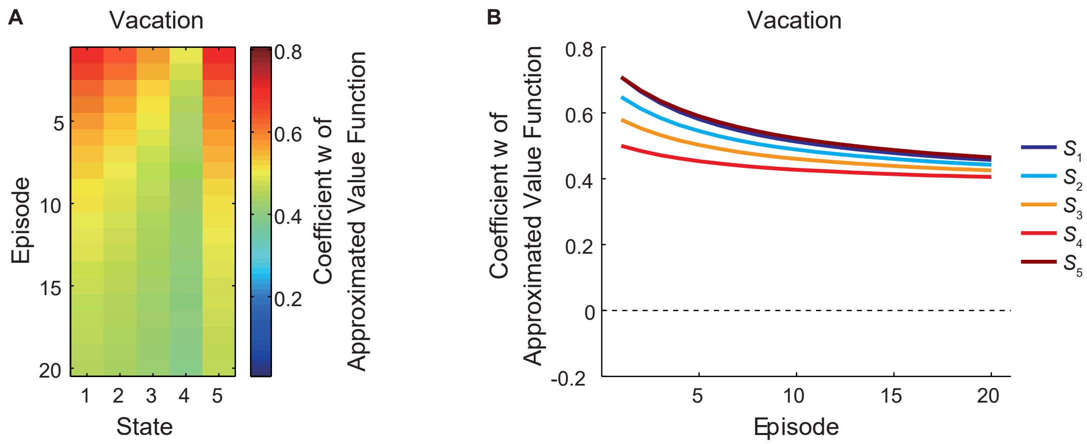

FIGURE 4 | Learning of the reduced SR-based approximated values during the vacation period. The change of the coefficient "*w*" of the reduced SR-based approximated state value function during the vacation period is shown in two different ways **(A,B)**. The notations are the same as those in **Figure 2**.

the results for the 20th episode in the vacation period. As shown in these figures, the agent's procrastination behavior deteriorated as cost became heavier.

Intuitive Mechanism of Procrastination in the Model and Effects of Parameter Variations

Here we explain the intuitive mechanism of how procrastination is generated in the model, and see how changes of parameters would bring to the model's behavior by manipulating cost, time discount factor, learning rate and inverse temperature. For the true values, taking "GO" action can be said to be more advantageous than taking "STAY" action for the agent because of the following two factors: (1) if reaching the next state by taking "GO," the reward will be less temporally discounted as the time needed to reach the goal state will decrease; and (2) if reaching the next state by taking "GO," the remaining future costs will also decrease as the cost associated with that "GO" action will already have been paid, while "GO" is disadvantageous than "STAY" because of the associated cost. The approximated values, on the contrary, fail to incorporate the decrease in the remaining future costs properly because the approximated state value is a linear function of the feature of each state, which is discounted reward value at the goal, and is not directly related to cost amounts (although costs have indirect effects through the weight *w*). This results in that the increase in the approximated state value across states is less steep than that in the true state value (**Figure 7A**), and therefore the agent using the approximated values for action selection could underestimate the "GO" value, and thereby make procrastination depending on parameter values.

When the cost is small (0.05) as compared with its original amount (0.1), even for the approximated values based on the reduced SR, choosing "GO" would become more advantageous than "STAY" and would induce little procrastination behavior (**Figure 7B**). However, whether the cost is large or small needs to be considered relative to reward size and the rate of temporal discounting (i.e., increment of reward value from one state to next due to decrement of discounting). When the discount rate was changed to a milder level (**Figure 7C**, discount factor changed from the original value 0.85 to 0.95 and the cost remained 0.05 as in **Figure 7B**), there should be less difference in discounted reward values across states, and thereby even the small cost (0.05) made action "STAY" more advantageous than "GO" in terms of approximated values, which in turn made the agent procrastinate.

We also examined the effects of changes in the learning rate or the inverse temperature. The learning rate was originally assumed to be initially high and gradually decreasing across episodes at both school and vacation periods. When set as constant values at 0.2 or 0.4, the overall patterns of the approximated and true values were not drastically changed from the original ones (**Figures 8A,B**, respectively), even though the weight *w* continued to vary largely across states in the case where the learning rate was 0.4 (**Figure 8C**). Therefore, the assumption that learning rate decreases across episodes would not be crucial for the current model to generate procrastination behavior. Regarding the inverse temperature, when set to a smaller value (10) than the original value (20), the overall patterns of the approximated and true values were not drastically changed (**Figure 8D**). When set to a larger value (30) (**Figure 8E**), the number of times of "STAY" increased, as expected from the increased degree of exploitation, and the values in the 20th episode in the vacation look affected.

Modifications to the Model

We also conducted separate sets of simulations, in which a "penalty" for "STAY" choice depending on the elapsed time, or an unpredictable "regret" for "STAY" choice, was added to the original model. The "penalty" was added to the approximated value of "STAY" used for action selection and the true value of "STAY," as well as the TD RPEs upon "STAY" choice. In contrast, the unpredictable "regret" was added only to the true value of "STAY" and the TD RPEs upon "STAY" choice but not to the approximated value of "STAY" used for action selection,

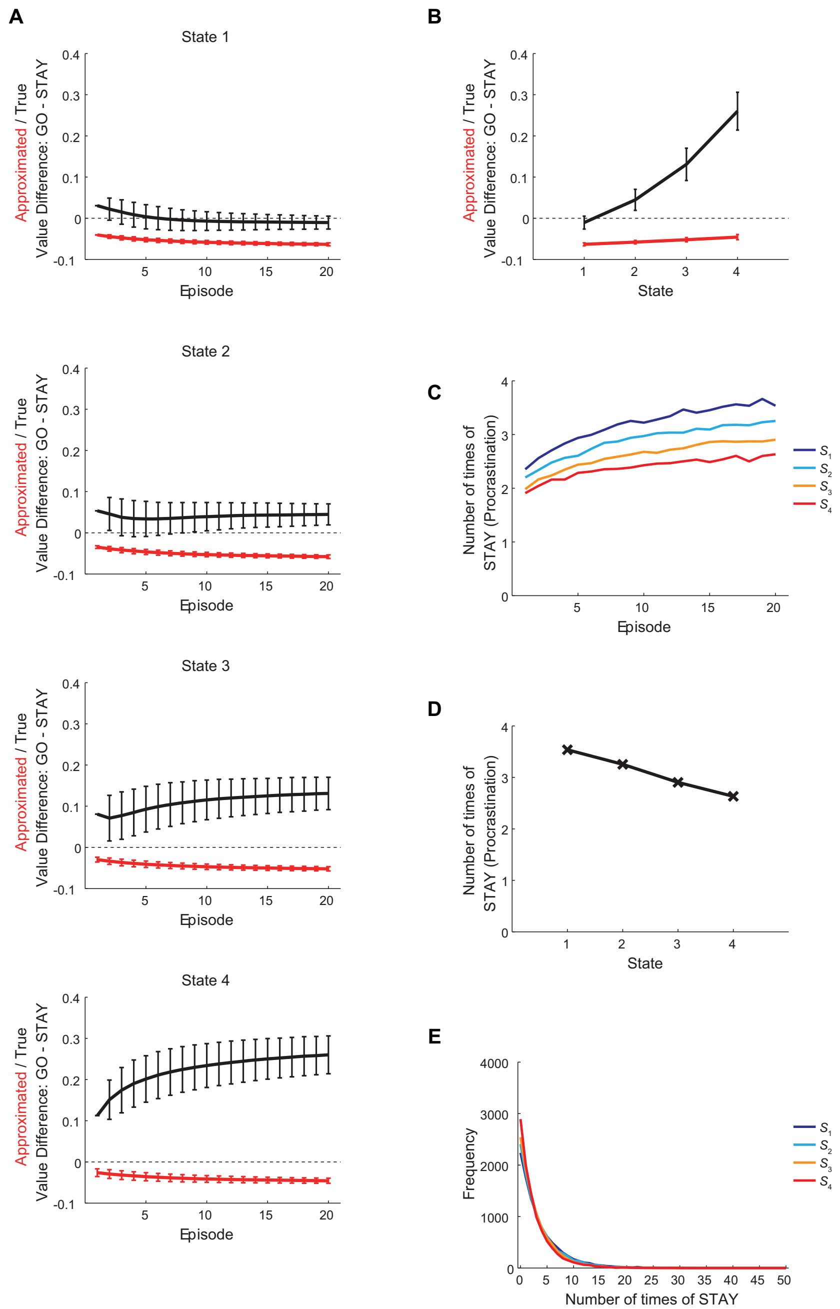

FIGURE 5 | Changes in the procrastination behavior during the vacation period. **(A)** The over-episode changes of the difference between the values of actions "GO" and "STAY," for the estimated true values under the policy that the agent was taking (black lines) and the reduced SR-based approximated values (red lines) (only the action values when the agent initially entered each state at each episode were used for calculation), except that the leftmost points of the black lines and of the red line for S_1 indicate the values under the non-procrastinating policy. The error bars indicate the mean ± SD across simulations. **(B)** The difference between the values of actions "GO" and "STAY" at the time when the agent initially entered each state in the 20th episode in the vacation period, for the estimated true values (black line) or the reduced SR-based approximated values (red line). The error bars indicate the mean ± SD across simulations. **(C)** The over-episode changes of the mean number of times for the agent to choose "STAY" at each state. **(D,E)** The across-simulation mean **(D)** and distribution **(E)** of the number of times for the agent to choose "STAY" at each state at the 20th episode in the vacation period.

assuming that the agent could not foresee the regret before actually taking "STAY" and thus could not incorporate it into the approximated value of "STAY." **Figure 9A** shows the results when adding the "penalty" for "STAY" choice, which appeared after 150 time-steps (since the beginning of the vacation period) and thereafter linearly increased. For all states, the number of times of "STAY" (i.e., procrastinating) initially increased, but then decreased, and the approximated values of "GO"

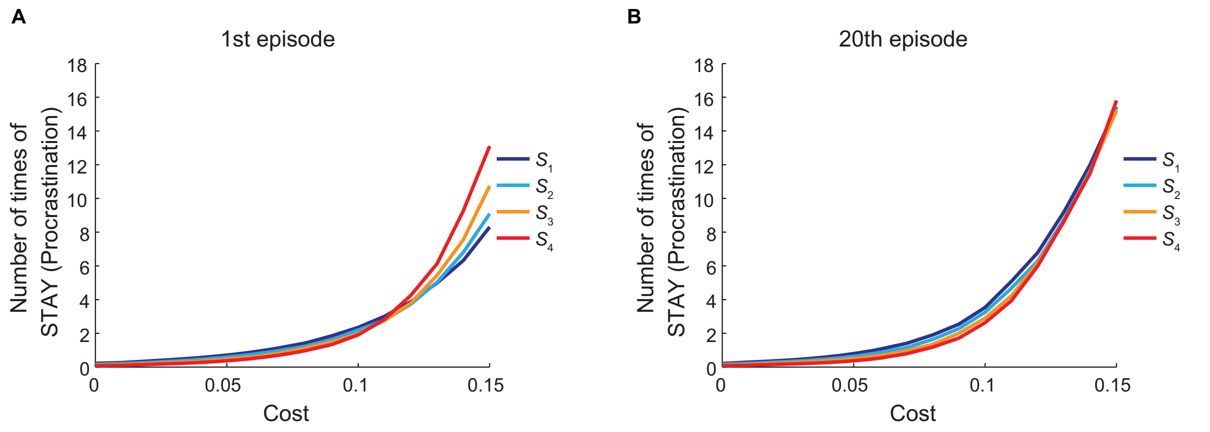

FIGURE 6 | Dependence of the procrastination behavior on the cost of "GO" action. The horizontal axis indicates the amount of cost for "GO" action. The vertical axis indicates the across-simulation mean number of times for the agent to choose "STAY" at each state (indicated by the color of each line) at the first **(A)** or the 20th **(B)** episode in the vacation period.

exceeded "STAY" at the 20th episode. These results suggested that adding the "penalty" to "STAY" choice would be able to reduce procrastination. **Figure 9B** shows the results when adding the "regret" for "STAY" choice. The results suggested that contrary to the "penalty," the "regret" after choosing "STAY" did not improve procrastination but even worsened the situation.

We further simulated the case where the reduced SR was slowly updated, through TD learning using the TD error of the feature variable, depending on the policy that the agent was actually taking during the vacation period. **Figure 9C** shows the results. Across episodes, the number of times of "STAY" at states except for S_4 initially increased, but eventually became decreasing at all the states, and the approximated value of "GO" at S_4 eventually exceeded the value of "STAY" at the 20th episode. These results indicated that such an update of the reduced SR could reduce procrastination.

DISCUSSION

This study sets out to investigate procrastination behavior from the perspective of value learning and value-based decision making. We assumed the goal-based reduced SR for state representation and modeled a series of actions and choices of a "student" during "school term" and "vacation" with cost for forward state transition and reward for reaching the goal state. The results suggested that the student, who firstly learned and updated the state value under the non-procrastinating policy during school term, soon started to procrastinate when choices can be freely made. This procrastination behavior was reduced as the student approached the goal state within the episode, but generally worsened across the episodes and with the increase of cost.

Implications of the Present Model and the Simulation Results

Humans may make non-optimal choices due to inaccurate valuation. In the case of procrastination, procrastinators may weigh in favor of the proximal but non-optimal rewards, and against the optimal but distant reward, and this inaccurate valuation could result from cognitive limitations (Lieder and Griffiths, 2016; Lieder et al., 2019). However, exactly what sort of limitations would cause such inaccurate valuation, which further leads to procrastination, has remained elusive. In the current study, we assumed that this inaccuracy in valuation resulted from a form of state representation, which was the goal-based reduced SR. With the cost ahead and the reward in relatively distant future, the inaccurate value approximation based on the reduced SR drove the agent to procrastinate, which in turn made the reward even more distant. The estimated true value under the policy that the agent was taking, on the other hand, suggested that it was better to choose "GO" action over "STAY" action most of the times (for S_1, the "STAY" value became on average slightly larger than the "GO" value as shown in **Figure 5A**). Although the agent first experienced episodes under the optimal policy (i.e., the non-procrastinating policy), the learned approximated values of states based on the reduced SR were already inaccurate. The inaccurate approximation of state values caused the discrepancy between the true and approximated action values and hindered the agent from making optimal decisions. Our results indicated that the reduced SR that is rigid (i.e., not easily updated) could be one of the mechanisms to explain procrastination.

As described in section "Methods," we conducted estimation of true state values under the policy that the agent was taking during vacation through TD learning, along with TD learning of the approximated values. We considered that human brain could potentially make such an estimation of true policy-dependent state values, or even also an estimation of the true optimal action values as mentioned in section "Methods." Possibly, such an estimation of true values could be one of the forms of value predictions in non-procrastinators. That is, it seems possible that people who can make such an estimation of true values may procrastinate less, while people who cannot might procrastinate more. Another possibility would be that human (brain) can have these different values at the same time, but the reduced SR-based approximated values can take dominance in controlling

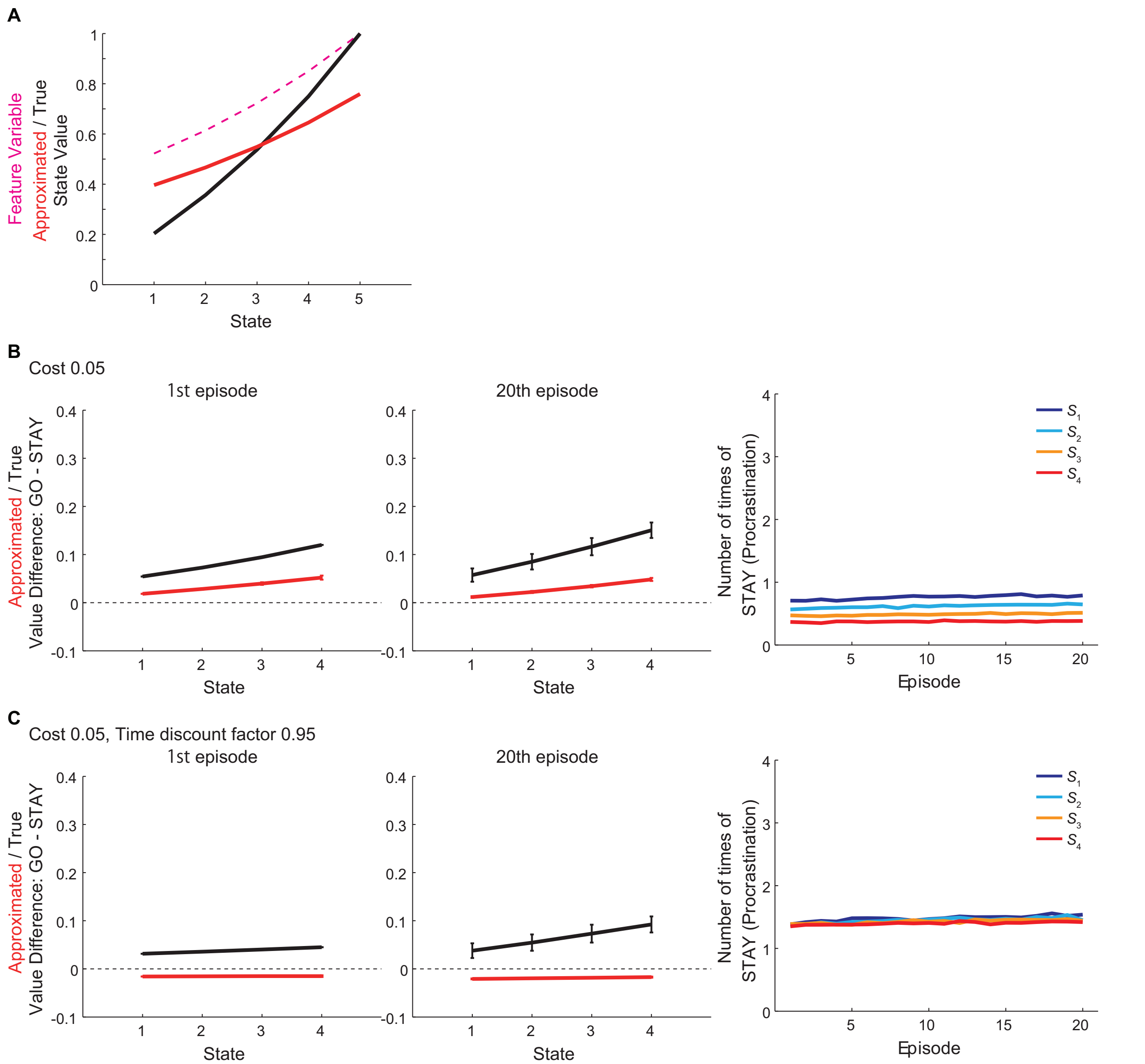

FIGURE 7 | Intuitive mechanism of procrastination in the model and effects of parameter variations. **(A)** The black line indicates the true state value under the non-procrastinating policy. The red line indicates the approximated state value, which is a linear function of the feature variable of each state (magenta dashed line) based on the reduced SR, under the non-procrastinating policy. **(B)** Results of simulations where the cost was changed from the original value (0.1) to 0.05. *Left and middle panels*: The difference between the values of actions "GO" and "STAY" at the time when the agent initially entered each state in the 1st (left panel) and 20th (middle panel) episode in the vacation period, for the estimated true values (black line) or the reduced SR-based approximated values (red line). The error bars indicate the mean ± SD across simulations. *Right panel*: The over-episode changes of the mean number of times for the agent to choose "STAY" at each state. **(C)** Results of simulations where the cost c and the time discount factor γ were changed from their original values 0.1 and 0.85 to 0.05 and 0.95, respectively. Configurations are the same as those in **(B)**.

choice behavior, depending on individuals and/or conditions, or at least have some effects on choice (unless there is specific mechanism to inhibit their effects). This possibility seems to be in line with the suggestion that most procrastinators wish to reduce procrastination [mentioned in Steel (2007) by citing (O'Brien, 2002)]. It could be due to the different valuation systems in human brain yielding contradictory results, and one prevailing over the other.

Apart from the goal-based reduced-SR that we assumed, there could be other forms of state representation which can also account for cognitive limitation that leads to inaccurate valuation. In particular, state representation by low-dimensional features generally has a risk of inadequacy and thereby inaccurate valuation. Further research would be needed to test possible relations of various forms of state representation to procrastination. On the other hand, inadequate

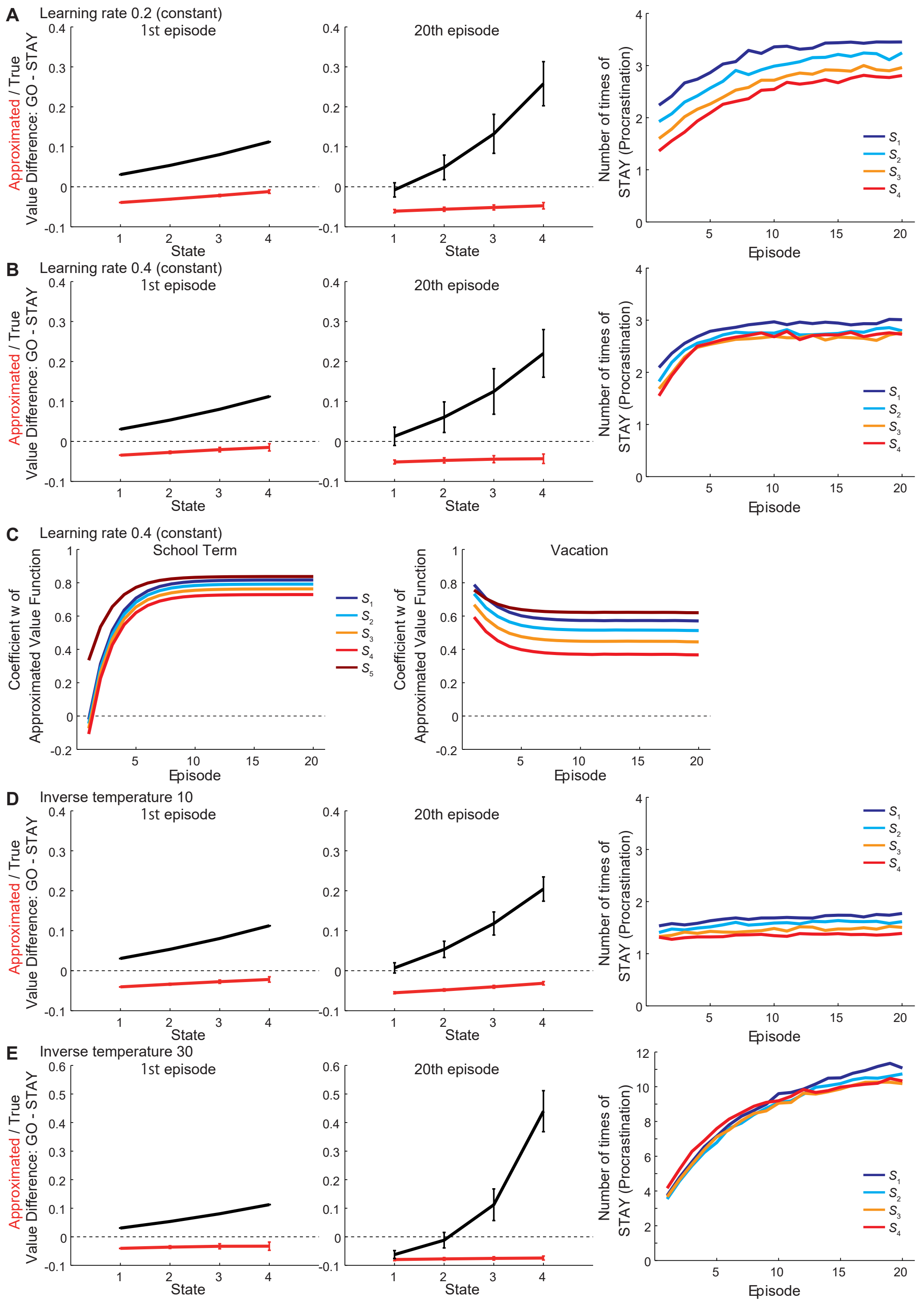

FIGURE 8 | Results with different assumptions on the learning rate or the inverse temperature. **(A,B)** Results of simulations where the assumption about the learning rate *a* was changed from the original one to being constant at 0.2 **(A)** or 0.4 **(B)**. *Left and middle panels*: The difference between the values of actions "GO" and "STAY" at the time when the agent initially entered each state in the 1st (left panel) and 20th (middle panel) episode in the vacation period, for the estimated true values (black line) or the reduced SR-based approximated values (red line). The error bars indicate the mean ± SD across simulations. *Right panel*: The over-episode changes of the mean number of times for the agent to choose "STAY" at each state. **(C)** The change of the coefficient "*w*" of the reduced SR-based approximated state value function during the school term (left panel) and the vacation period (right panel) in the case where the learning rate *a* was constant at 0.4. Each line indicates the change of "*w*" just after the agent left each state over episodes. **(D,E)** Results of simulations where the inverse temperature *b* was changed from the original value (20) to 10 **(D)** or 30 **(E)**. Configurations are the same as those in **(A,B)**.

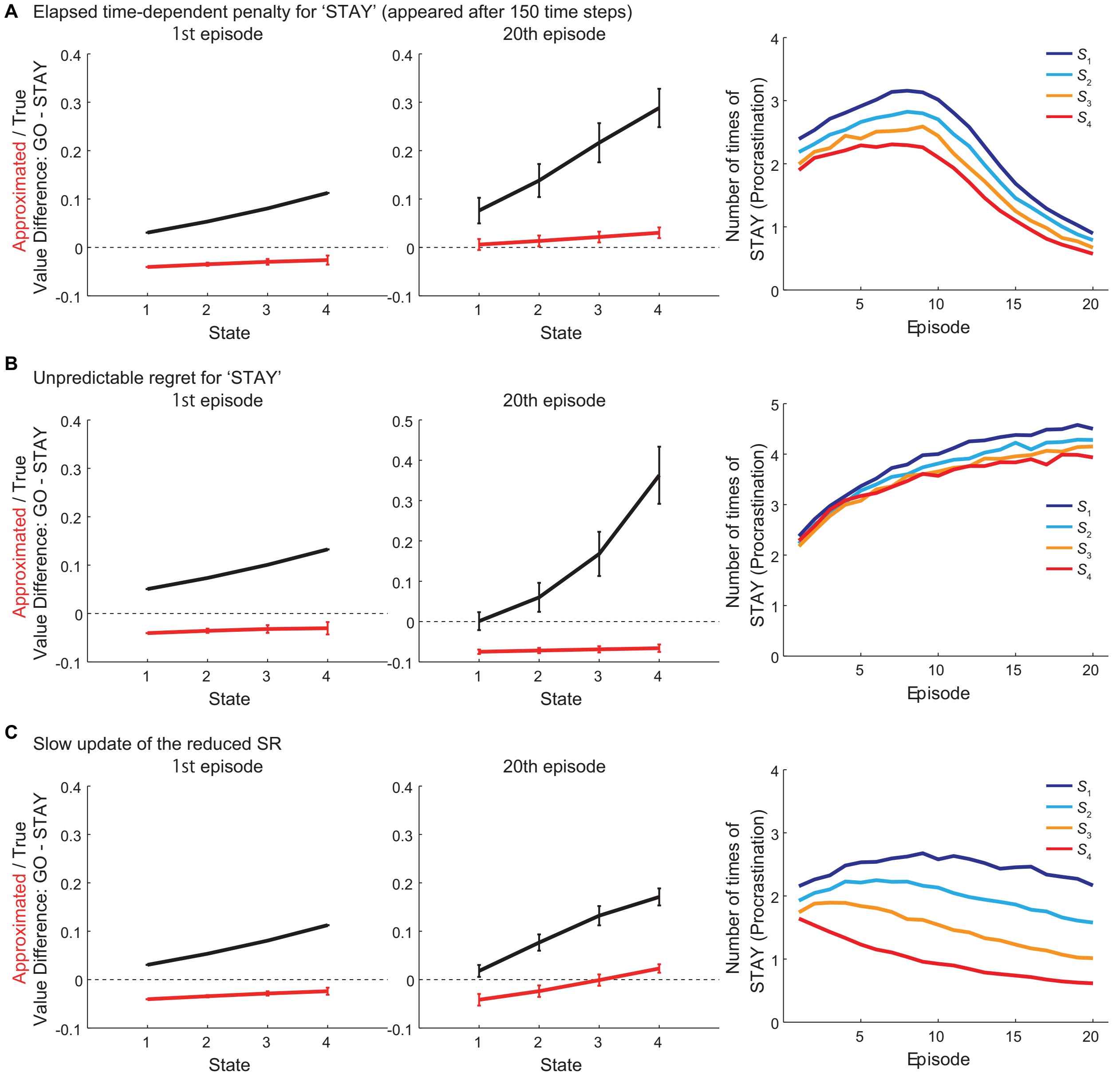

FIGURE 9 | Results of simulations with modifications to the model. **(A)** Results of simulations where a "penalty" for "STAY" choice depending on the elapsed time (appeared after 150 time-steps since the beginning of the vacation period) was added to the model. *Left and middle panels*: The difference between the values of actions "GO" and "STAY" at the time when the agent initially entered each state in the 1st (left panel) and 20th (middle panel) episode in the vacation period, for the estimated true values (black line) or the reduced SR-based approximated values (red line). The error bars indicate the mean ± SD across simulations. *Right panel*: The over-episode changes of the mean number of times for the agent to choose "STAY" at each state. **(B)** Results of simulations where an unpredictable "regret" for "STAY" choice was added to the model. Configurations are the same as those in **(A)**. **(C)** Results of simulations where the reduced SR was slowly updated during the vacation period. Configurations are the same as those in **(A)**.

state representation and inaccurate valuation due to low-dimensional state representation can be a potential mechanism for problematic behavior, or even psychiatric disorders, other than procrastination. Recent work (Shimomura et al., 2021) proposed that rigid goal-based reduced SR can contribute to the difficulty in cessation of habitual (addictive) reward obtaining. Meanwhile, there have been reports of possible relations between behavioral addiction and procrastination (e.g., Li et al., 2020; Yang et al., 2020). Future study is desired to examine if inadequate state representation underlies the coexistence of procrastination and addiction.

Relations to Other Studies

Previous psychological models, including the Temporal Motivation Theory (Steel and König, 2006) and the temporal decision model (Zhang et al., 2019b), have incorporated the hyperbolic type of temporal discounting in the formulation. In particular, the time inconsistency or "myopic preference reversal" (Kirby and Herrnstein, 1995), occurring in hyperbolic or quasi-exponential discounting, has been proposed to be a cause of procrastination (O'Donoghue and Rabin, 1999; Steel and König, 2006), as well as of other impulsive or unhealthy behavior (reviewed in Story et al., 2014 with a critical view). The current framework based on RL, however, showed that even only incorporating the assumed exponential discounting could generate procrastination behavior. Although it has been indicated that temporal discounting of humans and animals generally has resemblance to hyperbolic discounting (Myerson and Green, 1995; Mazur, 2001), while very hyperbolic discounting (i.e., severe discounting for a short delay) may be seen in some people and/or conditions, less hyperbolic and more exponential-like discounting could possibly be observed in others. Our model could provide a mechanistic explanation of procrastination in the latter cases.

Procrastination has been shown to be negatively correlated with scales related to self-control or planning (Steel, 2007). In our model, inaccurate value approximation caused by the reduced dimension of state features could lead to non-optimal action choices, and this could be framed as non-optimal planning. Also, it was reported (Taylor et al., 1998) that mental simulation of the process of goal reaching including detailed steps, named process simulation, facilitated performance whereas mental simulation of goal outcome, named outcome simulation, did not. Another study (Oettingen, 2012) also implicated that fantasizing or daydreaming about the desired future (i.e., the goal) could hinder the pursuit of the goal. Focusing just on the goal outcome, paying little attention to the intermediate steps, could potentially lead to a formation of, and/or reliance on, state representation based particularly on the goal state. In our model, value approximation based on the goal-based reduced SR has an inability to properly incorporate step-by-step decrement of remaining future cost, and it leads to procrastination as explained in the Results. In this regard, it is tempting to speculate that the abovementioned behavioral results for better performance with process simulation but not with outcome simulation could potentially be because the different ways of mental simulations led to different ways of state representation.

In our model, procrastination behavior was generally worsened across episodes, unless the "penalty" was added or the reduced SR was updated. In the literature, a study that objectively measured academic procrastination by examining homework initiation (Schiming, 2012) reported that generally students procrastinated more along with the progress of the academic term. However, that study examined homework during the term rather than in the vacation, and it is not sure if there are any potential links between their results and ours. Also, in our model, whereas the unpredictable "regret" coming after procrastinating did not really help with reducing procrastination, the "penalty" of procrastinating, which could potentially represent the pressure of deadline, did reduce procrastination. The latter could be regarded as an implementation of the suggested effectiveness of deadlines (Ariely and Wertenbroch, 2002), although if so, where such penalty comes from remains to be addressed.

There has not been direct evidence to support that the reduced SR is actually implemented in human brain, but there are some indirect implications. SR has been proposed to be hosted in the hippocampus and the prefrontal cortex (Russek et al., 2017; Stachenfeld et al., 2017). The possibility that the goal-based reduced SR, in addition to or instead of the genuine SR, is hosted in these regions seems in line with the observed negative correlation between the ventromedial prefrontal cortical and hippocampal blood-oxygen(oxygenation)-level-dependent (BOLD) signals and the distance to the goal (i.e., signals increase as the goal becomes closer, as in the feature variable in the goal-based reduced SR) (Balaguer et al., 2016). A resting-state functional magnetic resonance imaging (fMRI) study (Zhang et al., 2016) found positive correlation between behavioral procrastination and the regional activity of parahippocampal cortex, an area neighboring the hippocampus. Moreover, an event-related fMRI study (Zhang et al., 2019a) has shown that a decreasing coupling of hippocampus-striatum mediated the promoting effect of insufficient association between task and outcome on procrastination. These findings appear to support, to some degree, the rationale of modeling procrastination behavior under the reduced SR-based model in the present study.

Limitations, Predictions, and Perspectives

The present study is a theoretical proposal of a hypothetical mechanism of procrastination, and its clear limitation is the absence of experiments. Further studies with human subjects will need to be undertaken to validate the model. Whether, or to what degree, humans adopt the reduced SR based on the goal state, which can be generalized to the states with immediate reward or punishment, can be tested by behavioral experiments to examine if they can adapt to changes in reward sizes more easily than to changes in reward locations (as proposed in Shimomura et al., 2021). Then, our present model predicts that the degree of adoption of the goal-based reduced SR is correlated with the degree of procrastination, especially in people whose temporal discounting is less hyperbolic (more exponential). Also, as explained in the Results, in our model, what causes procrastination (i.e., choice of action "STAY") is that one of the benefits of taking the action "GO" (i.e., "decrement of remaining future cost") cannot be properly taken into account if the agent resorts to approximated values based on the reduced SR. Therefore, it is expected that explicitly informing the subject of such an information (e.g., by showing remaining future cost, and its decrement by "GO" choice, by a bar indicator) would promote the "GO" choice. Our model predicts that procrastination can be mitigated by this way especially in procrastinators whose temporal discounting is not very hyperbolic.

There are also limitations of our work in terms of modeling. We assumed that the agent had acquired the reduced SR, and based on it, the approximated values were learned, but how the reduced SR itself had been acquired was not addressed. Moreover, our model assumes the school term-vacation setting, which could potentially be applied to in-class and out-of-class settings to some extents, but there should be situations that cannot be well captured by our model. Furthermore, the model does not include things that can be related to procrastination, such as alternative rewards or deadlines (although we did examine the effects of elapsed time-dependent penalty for "STAY" choice). Constructing models that can address these issues is an important future direction. Also, our model is based on the TD RL theory and the suggested representation of TD RPE by phasic dopamine signals, but it has been suggested that tonic or slowly changing dopamine signals or baseline dopamine levels may represent or relate to something different from TD RPE, in particular, action vigor or motivation (Niv et al., 2007; Howe et al., 2013; Collins and Frank, 2014; Hamid et al., 2016; Möller and Bogacz, 2019; but see also Kato and Morita, 2016; Kim et al., 2020). Also, distributional RL theory, which concerns not only the expected value but also the variance (uncertainty) or distribution of rewards, has been developed (Morimura et al., 2010; Bellemare et al., 2017; Dabney et al., 2018), and how reward uncertainty or distribution can be encoded in the basal ganglia and/or dopamine systems has been suggested (Mikhael and Bogacz, 2016; Dabney et al., 2020). It is also an interesting direction to model procrastination behavior taking these concepts beyond the conventional dopamine TD RPE hypothesis into account.

Notwithstanding the limitations, we would like to emphasize the strengths of this study. As mentioned in the Introduction, procrastination can be considered to be a form of value-based decision making, which has been extensively studied by combining behavioral, physiological, or neuroimaging experiments and RL models, leading to proposals of concrete mechanisms of how specific brain regions or neural populations encode specific variables or parameters. The present study tries to connect procrastination to the rich literature of value-based decision making, and thereby could help further our understanding of procrastination behaviors. In addition, laboratory study of procrastination can be challenging for task design, as the time for experiments is usually limited and not long enough for the participants to procrastinate. Looking from the value-based decision-making perspective, however, could potentially bring different possibilities for future practice.

DATA AVAILABILITY STATEMENT

The original contributions presented in the study are publicly available. This data can be found here: https://github.com/GigiiY/Procrastination_ReducedSR.

AUTHOR CONTRIBUTIONS

ZF and KM developed and elaborated the model with the reduced SR for procrastination, which KM conceived of, and conducted the simulations. Before these, AMN developed different reinforcement learning models with temporal discounting of mental effort cost for model fitting of behavior in order to explain procrastination, and discussed them with KM. ZF, AMN, and KM explored and discussed previous related studies. ZF drafted the original manuscript, and KM revised it with reference to comments of ZF and AMN. All authors contributed to the article and approved the submitted version.

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The Cognitive-Vestibular Compensation Hypothesis: How Cognitive Impairments Might Be the Cost of Coping With Compensation

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Previous research in vestibular cognition has clearly demonstrated a link between the vestibular system and several cognitive and emotional functions. However, the most coherent results supporting this link come from rodent models and healthy human participants artificial stimulation models. Human research with vestibular-damaged patients shows much more variability in the observed results, mostly because of the heterogeneity of vestibular loss (VL), and the interindividual differences in the natural vestibular compensation process. The link between the physiological consequences of VL (such as postural difficulties), and specific cognitive or emotional dysfunction is not clear yet. We suggest that a neuropsychological model, based on Kahneman's Capacity Model of Attention, could contribute to the understanding of the vestibular compensation process, and partially explain the variability of results observed in vestibular-damaged patients. Several findings in the literature support the idea of a limited quantity of cognitive resources that can be allocated to cognitive tasks during the compensation stages. This basic mechanism of attentional limitations may lead to different compensation profiles in patients, with or without cognitive dysfunction, depending on the compensation stage. We suggest several objective and subjective measures to evaluate this cognitive-vestibular compensation hypothesis.

Keywords: vestibular, cognitive effort, cost, compensation, effort

Abbreviations: BVL, bilateral vestibular loss; CVS, caloric vestibular stimulation; DHI, dizziness handicap inventory; GVS, galvanic vestibular stimulation; MCR, mean caloric response; NVI, neuropsychological vertigo inventory; SVDS, subjective vestibular disability score; UVD, unilateral vestibular deafferentation; UVL, unilateral vestibular loss; VEMP, vestibular evoked myogenic potentials; VHIT, video head impulse test; VHQ, Vertigo Handicap Questionnaire; VL, vestibular loss; VOR, vestibulo-ocular reflex; VSS, Vertigo Symptom Scale.

INTRODUCTION

Animal models and artificial stimulation studies on healthy human participants have delivered a growing body of evidence supporting a clear link between the vestibular system, emotional and cognitive impairments. This body of research consistently shows that postural imbalance which appears after (artificially created) vestibular damage is linked to cognitive changes, mostly related to space perception difficulties (Zheng et al., 2006; Lenggenhager et al., 2008; Lopez et al., 2010; Machado et al., 2012a,b; Ferrè et al., 2013a,b; van Elk and Blanke, 2014; Besnard et al., 2015; Deroualle et al., 2015). However, according to the clinical experience from Ear, Nose and Throat doctors (ENT) and their multidisciplinary teams, a high variety of patient profiles do not match this scientific evidence. For example, patients might present with a vestibular pathology associated with mild residual postural instability, but show no objective cognitive impairment measured by neuropsychological tests. At the same time, patients frequently complain of subjective emotional or cognitive difficulties, some variables missing in animal or healthy human artificial stimulation studies. Disentangling these different dimensions could help to disentangle the complex variety of observed patients profiles. Too few investigations have tried to quantify the specific contribution of each potential variable, and the results of the literature exploring vestibular-damaged patients profiles remain heterogeneous.

We present a novel hypothesis to explore the heterogeneous clinical profile of vestibular-damaged patients. Taking into account their degree of postural imbalance; their objective cognitive neuropsychological performances; and their degree of subjective cognitive, physical, and emotional complaints would lead to a comprehensive approach. We focus on the potential role of cognitive effort that patients have to invest to compensate their vestibular pathology. To quantify this effect, we apply a neuropsychological model, based on Kahneman's Capacity Model of Attention, which will allow to integrate the existing findings from the literature in the framework of our hypothesis. The contribution of this model to clinical observations of a variety of patients' profiles will be discussed, as well as a protocol that could be applied retrospectively on (un)published data. We are convinced that this cost-benefits approach could shed new light on clinical vestibular research.

Animal and Artificial Stimulation Research

In rodent animal studies, vestibular loss (VL) is typically associated with spatial memory and navigation impairments (Russell et al., 2003; Baek et al., 2010; Besnard et al., 2012; Machado et al., 2012a); as well as with increased anxiety-like behaviour (Machado et al., 2012b). Moreover, spatial memory impairments seem to persist in time, at least up to 14 months after bilateral vestibular deafferentation (BVD), suggesting that cognitive deficits may be permanent despite a possible adaptation to the physical symptoms such as oscillopsia (Baek et al., 2010).

Similarly, studies on healthy human participants have also demonstrated specific cognitive impairments using artificial vestibular stimulation. For example, galvanic vestibular stimulation (GVS) modulated spatial perception bias in a bisection line task (Ferrè et al., 2013a) and random number generation (Ferrè et al., 2013c). Caloric vestibular stimulation (CVS) changed the perception of body part position in space, relative to body schema causing a bias in perceived object size, hand length, and hand width (Lopez et al., 2012). Vestibular stimulation caused by a rotatory chair influenced self-centred mental imagery, but not 3D object mental rotations (Deroualle et al., 2015).

These observations in animal and healthy human artificial stimulation research have led to patient studies, which have attempted to replicate results and identify common neural pathways. However, the generalisation to patient studies is complex for several reasons. Firstly, animal studies mostly use maze tasks for practical reasons, creating a literature bias toward the investigation of spatial memory compared to other cognitive functions. This under-representation of other cognitive functions makes the comparison with clinical population more difficult, as patients report many other cognitive decrements. Secondly, the understanding of the mechanisms of how artificial vestibular stimulation influences cognition is not yet well understood (Grabherr et al., 2015). Finally, the acute temporary character of the stimulation influence on cognition may not be an appropriate comparison to long-term chronic vestibular pathology.

Regarding patient studies, the variety of cognitive and emotional measures impairs the comparison to studies using artificial stimulation. Original patient studies have mostly used subjective questionnaires, consistently showing significant increases of emotional, physical, and cognitive complaints compared to control participant responses (Eagger et al., 1992; Yardley et al., 1992; Yardley and Putman, 1992; Godemann et al., 2004; Gómez-Alvarez and Jáuregui-Renaud, 2011; Alghwiri et al., 2013; Lahmann et al., 2015; Lacroix et al., 2016; Semenov et al., 2016; Liu et al., 2019). Comparison with animal and artificial stimulation research is difficult, as no questionnaires are used in animal research and very few questionnaires have been used with human artificial stimulation research. Fortunately, recent patient studies have included more objective neuropsychological measures (such as virtual mazes, computerised reaction time tasks, etc. . .), allowing some comparison.

Variability of Results in Vestibular-Damaged Patient Studies

Objective neuropsychological assessment through computerised measures has contributed to a better understanding of VL patient cognition. Specific cognitive deficits have been identified for spatial cognition, short-term memory, executive functions, processing speed, and visuospatial abilities, particularly in patients with bilateral vestibular loss (BVL) when compared to patients with unilateral vestibular loss (UVL) or healthy controls (Grabherr et al., 2011; Popp et al., 2017; Deroualle et al., 2019). However, contrary to the global coherence observed in animal and stimulation studies, patient studies show less straightforward results. Several additional reasons can be identified for this discrepancy, mostly highlighting methodological differences between protocols.

Similarly to animal studies, some vestibular-damaged patient research has used orientation tasks such as the Virtual Morris Water Task (VMWT). Chronic BVL patients demonstrate impairments in this task, which are associated with a decreased hippocampal volume (Schautzer et al., 2003; Brandt et al., 2005). However, this structural change has not always been found in other studies of BVL (Cutfield et al., 2014) or UVL patients (Hüfner et al., 2007). In addition, studies exploring body perception in space have demonstrated depersonalisation symptoms (where one feels detached from one's own body) in BVL and UVL patients (Sang et al., 2006; Jáuregui-Renaud et al., 2008a,b), but a subsequent study failed to evidence these effects using a subjective questionnaire in chronic BVL patients (Deroualle et al., 2017). So far, it remains unclear whether VL patients present specific cognitive deficits, such as space or numerical processing; or if the effects are rather more general cognitive deficits involving executive functioning (Risey and Briner, 1990; Moser et al., 2017).Whereas the extent of VL can be controlled in animal studies through surgical or chemical procedures, patient research needs specific physiological measures to assess the degree of this VL. Caloric testing (Mean Caloric Response – MCR, values from caloric irrigation in°/s from both ears with cold −30°C- and warm −44°C- water), vestibular evoked myogenic potentials (VEMP; registering information from two muscles effectors and allowing testing of otolithic receptors) or the video head impulse test (VHIT; measuring high acceleration for the six canals) provide complementary information about the current statute of the VL. However, it remains unclear whether and how these physiological measures are related to cognitive deficits.

Popp et al. (2017) evidenced a correlation between the degree of VL measured trough the MCR and two tasks measuring visuospatial abilities and memory in BVL patients. They also found a correlation between the VHIT outcomes and some aspects of memory and executive functions; for both UVL and BVL patients. However, no correlations were found between those physiological measures and processing speed, nor with the Corsi Block Tapping task. Those inconsistencies underline the complexity of establishing mechanistic links between the different dimensions. Other studies have searched for a link between physiological measures of the VL, cognitive and emotional impairments, and brain changes. For example, Helmchen et al. (2009) showed that UVL patients who recovered the best after the VL (at least at the physiological level, measured by the MCR), had a higher increase in grey matter volume (GMV; inferior insular temporal GMV increase), as a sign of their recovery. At the same time, their results demonstrated a volume increase in the vestibular insular cortex and superior temporal gyrus (STG) that was negatively correlated with the patient's subjective vestibular disability score (SVDS), indicating that patients with higher subjective clinical complaints had a higher increase in these cerebral areas. On the other hand, no correlation was found in a subsequent study (Göttlich et al., 2016) between hippocampus volume and patient's subjective measures [Vertigo Handicap Questionnaire (VHQ) (Tschan et al., 2010), Vertigo Symptom Scale (VSS) (Tschan et al., 2008), and SVDS]; nor with the quantitative assessment of the vestibulo-ocular reflex (VOR gain).

In addition, vestibular pathologies frequently accompany sensorineural hearing loss (SNHL) and many patient studies have not adjusted for this comorbidity (Smith, 2021). Recent research showed that cognitive function could be affected differently by each pathology, with specific challenges in immediate memory and language tasks for SNHL patients, and worse performance in attention tasks for VL patients (Dobbels et al., 2019). Statistically significant differences on the VMWT have been found between VL patients and healthy controls in studies where some VL patients (only one or two patients on the total sample) had mild hearing loss (Brandt et al., 2005; Kremmyda et al., 2016); whereas no differences were found in a larger VL patients sample when adjusting for hearing status (Dobbels et al., 2020).

Although the results presented above could be linked to the different measures used, the variety of vestibular pathologies studied could also provide a potential explanation. The many different types of vestibular pathologies (Ménière's disease, vestibular neuritis, vestibular schwannoma, vestibular migraines, Benign Paroxysmal Positional Vertigo, vestibular nerve resection, or vestibular areflexia), as well as the different types of recovery a patient can experience, add complexity to this research field and warrant further investigation.

Variety of Pathophysiology and Clinical Expression of the Patient Recovery

The early stage of a vestibular dysfunction is associated with diminished postural and oculomotor control, abnormal body perception in space, and autonomic symptoms. Fortunately for patients, VL triggers a vestibular pathway reorganisation called vestibular compensation, allowing for rapid improvements in postural control, action control, and improved body perception in the environment (Lacour et al., 2016). This vestibular compensation mechanism has been described as composed of three stages: Restoration, Habituation, and Adaptation. In addition, within the adaptation stage, a distinction is made between "sensory substitution" and "behavioural substitution." Sensory substitution means that patients rely (intentionally or not) on other sensory modalities such as visual or somesthetic information in order to compensate for the impaired vestibular input. Behavioural substitution indicates that patients use other neural networks to mimic or replace vestibular function (Lacour et al., 2016).

Although compensation mechanisms are increasingly documented in animal research, there is a level of idiosyncrasy in human patient recovery that cannot be fully explained by the animal models. While some patients very rapidly succeed in returning to a normal balance; others only partially recover at the postural level, with a highly variable functional impact on their quality of life. The type of VL (UVL versus BVL) may partially explain the variety of compensation profiles in patients

(Lacour et al., 2009). However, even in similar pathology, such as unilateral vestibular deafferentation patients (UVD), at least 20% of the patients may present persistent complaints of postural imbalance and incomplete long-term compensation (Reid et al., 1996; Halmagyi et al., 2010). Functionally compensated patients regarding the physiological impairment may nonetheless continue to present subjective complaints about their quality of life, with emotional and cognitive difficulties. These dimensions can be measured with specific questionnaires (Lacroix et al., 2016). Although several premorbid patient characteristics such as age (Gauchard et al., 2012); psychological factors (Yardley and Redfern, 2001); illness perception and coping strategies (Ribeyre et al., 2016); or the level of physical activity (Gauchard et al., 2013) seem to play a role in the recovery process, the way these different variables interact remains largely unknown.

The Complexity of Cognitive-Vestibular Compensation Assessment in Patients

Compensation mechanisms of VL patients are typically investigated with various physiological measures, among which the improvement of the gain and phase of the VOR trough saccades (VOR) (Curthoys, 2000; Curthoys and Halmagyi, 2007; Macdougall and Curthoys, 2012; Ranjbaran et al., 2016); the postural score changes on dynamic posturography platforms (Gauchard et al., 2012; Parietti-winkler et al., 2016); or changes in the GMV (Helmchen et al., 2011; Hong et al., 2014). However, these assessments focus solely on the vestibular compensation at the physiological level. Furthermore, it is not always possible to implement these measures in clinical settings, with a varying degree of access to diagnostic resources (Agrawal et al., 2020). Most of the time, patient recovery is evaluated based on the clinical reduction of physical symptoms, such as a better postural control. Therefore, the persistence of subjective emotional or cognitive complaints such as agoraphobia, persistent fatigue or attentional disorders usually leads to supplementary (neuro)psychological consultations, where standard gold-standard measurements are not always sensitive enough to detect specific impairments.

Whereas it is widely accepted that postural recovery can vary from one patient to another, little is known about the associated subjective emotional or cognitive impairments, which might be the cost of a successful postural recovery. Guidetti et al. (2008) compared 50 unilateral labyrinthine-defective patients (without vertigo) to healthy controls using the Symptom Check List questionnaire (SCL-90; Derogatis et al., 1976) and several objective cognitive measures. These authors report that patients showed significantly higher levels of subjective anxiety and lower scores on the objective visual memory Corsi block task. However, no correlation analyses were performed between the measures, and no physiological compensation measures such as postural control were recorded. This type of analysis is essential if we want to understand whether patients presenting subjective physical or emotional complaints (despite postural compensation), also present specific cognitive neuropsychological impairments.

DETERMINING THE COGNITIVE AND EMOTIONAL COST TO MAINTAIN A FUNCTIONAL POSTURAL BALANCE AFTER A VESTIBULAR DAMAGE

Allocation of Resources Models

Various theoretical models attempt to explain how individuals allocate (willingly or not) resources when facing challenging actions, and what could be the cost of this allocation in terms of fatigue (Pattyn et al., 2018). In sleep research, it is well established that sleep loss and fatigue decrease the individual resources available to the task and increases the effort required to perform the task (Williamson et al., 2011). This compensatory model postulates that fatigue will primarily affect the secondary task activities, since primary task activities are protected (Robert and Hockey, 1997). In the vestibular domain, dual-task paradigms (where a participant performs a postural and a cognitive task at the same time) are similarly used to demonstrates competition between the cognitive resources needed to complete two tasks and a resulting cost to performance (Bigelow and Agrawal, 2015). Therefore, we advocate to apply a neuropsychological model to take into account the degree of perceived subjective compensation (at the physical, emotional, and cognitive level) in addition to objective physiological and cognitive measures.

The Kahneman's Capacity Model of Attention Adapted to Vestibular Damaged Patients

According to Kahneman's Capacity Model of Attention (Egeth and Kahneman, 1975), there is a limited quantity of cognitive resources that can be allocated to any given task. Therefore, Kahneman's model applied to VL predicts that a patient with successful physiological compensation (where cognitive resources are successfully used to maintain postural control, thereby preventing falls), would have reduced cognitive resources for other cognitive tasks in comparison to patients with non-successful compensation (i.e., where no cognitive resources are used for vestibular compensation) (Bigelow and Agrawal, 2015). Unlike sleep loss, where the compensatory efforts have to be provided temporarily in specific situations (when the restoration of sleep can be achieved later), the loss of vestibular information requires a continuous adaptation of the body to maintain a proper balance. Therefore, it is highly plausible that the cost of adaptation will affect specific cognitive abilities. This cost might fluctuate and increase with time, depending on the compensation stage, and consequently affect patients' emotions and quality of life.

This hypothesis is in contrast to the traditional view, which assumes that the more physiologically or physically affected vestibular-damaged patients would show more cognitive or emotional disorders. We suggest that non-compensated patients (no use of cognition to compensate for posture) would not show difficulties in cognition as all their cognitive resources remain available. On the contrary, after successful physiological compensation, the loss of resources (dedicated to maintain

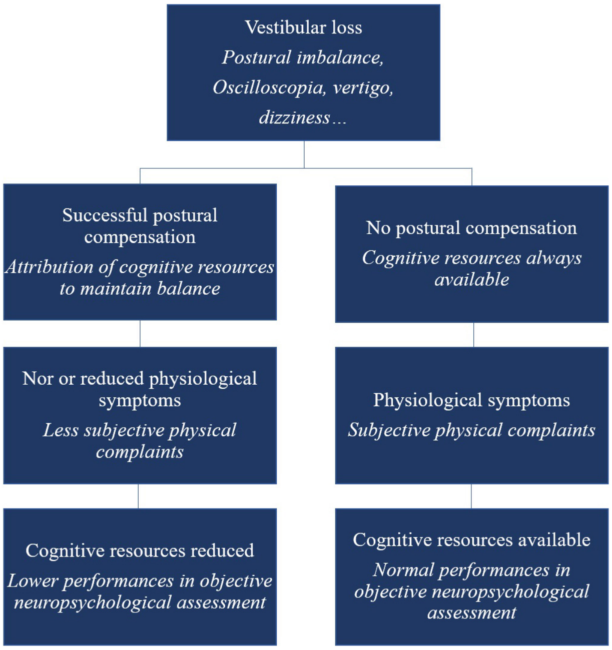

FIGURE 1 | Schematic overview of the cognitive-vestibular compensation hypothesis – adaptation of the Kahneman's Capacity Model of Attention to vestibular-damaged patients.

balance) will affect cognitive abilities. With postural recovery achieved, patients' subjective perception of their physical capacities could be positively affected, leading to a counter-intuitive observation: less complaining patients (at least at the physical level) would be more cognitively impacted (see **Figure 1**). This cognitive-vestibular compensation hypothesis could explain an apparent absence of group effect in some studies (as the different types of compensation could cancel each other out amongst the different patients depending on their compensation stage).

Evidence Supporting the Cognitive-Vestibular Compensation Hypothesis

Although our hypothesis has never been investigated as such, there is tentative evidence supporting the assumption that only vestibular-damaged patients with successful compensation would show specific cognitive impairments. Redfern et al. (2004) evaluated the cognitive profiles of 15 UVL patients that were described as "well-compensated" (showing no symptoms of dizziness or postural deficits). When compared to controls in a dual-task paradigm, they demonstrated significantly slower reaction times during a choice and inhibitory reaction time task (the secondary task) while performing a postural task (the main task). However, three of the patients were described as "not perfectly well-compensated" (with abnormal results in posturography or vestibulo-ocular function) and these three patients showed faster reaction times than the other patients. It is possible that the three patients were only in the early stage of their compensation adaptation process and had sufficient cognitive resources available for the cognitive tasks. These preliminary results suggest that different compensation profiles may interfere with cognitive abilities. However, until now, the relationship between cognitive impairments and the various degrees of postural compensation has not been systematically investigated, and more research is needed to explore our hypothesis.

FUTURE PERSPECTIVES

Future research investigating how cognitive impairments might be the cost of coping with compensation would benefit from a degree of standardisation in assessment, including subjective and objective measures.

Regarding subjective assessment, we suggest using specific questionnaires to systematically determine how patients describe their own level of compensation. One validated gold standard questionnaire, the Dizziness Handicap Inventory (DHI; Jacobson and Newman, 1990), provides specific subscales evaluating physical, functional, and emotional self-perception of the patient's vestibular state. It has been demonstrated that the functional subscale is related, at least partially, to GMV increase in visual and cerebellar areas; and therefore may be used as a potential sign of vestibular compensation (Helmchen et al., 2009; Hong et al., 2014; Lacour et al., 2016). Future studies could use the DHI to separate patients into subgroups, based on the scores for these subscales. Using a large patient group, it should be possible to analyse retrospectively if patients with higher versus lower levels of physical complaints show different objective cognitive results.

Regarding our cognitive-vestibular hypothesis, we predict that patients with higher levels of physical complaints are less physiologically compensated, and therefore will show preserved cognitive abilities. We propose to use specific subjective cognitive measures to test this hypothesis. The cognitive-failure questionnaire (CFQ; Broadbent et al., 1982), or the neuropsychological vertigo inventory (NVI; Lacroix et al., 2016), may offer helpful insight into patients' own perception of their cognitive state. These questionnaires have already demonstrated their sensitivity by allowing for the identification of different profiles among different types of VL (Liu et al., 2019). However, it has not been possible so far to determine whether patients with higher levels of subjective cognitive complaints have higher objective cognitive deficits and what would be their state of physiological compensation. To the best of our knowledge, this has never been measured in such a holistic approach.

Regarding objective cognitive assessment, we thus propose that future research should include challenging assessments, taking into account the degree of cognitive effort required by the tasks. A recent study testing vestibular-damaged children yielded a distinction between dynamic (involving a "mental movement" during the execution to solve the task, such as in mental rotation) and static tasks [not involving mental movement, such as when performing a target detection task (Lacroix et al., 2020)]. We suggest that the cognitive tasks involving dynamic processes require greater cognitive resources than the static ones, and therefore are more likely to be sensitive to successful vestibular compensation. Alternatively, the level of cognitive resources involved in the tasks could also be estimated based on the amount of executive functions involved in the tasks. Executive functions have been shown to play a role in gait disturbances, and several simple tasks to assess these can easily be used in clinics (Yogev-Seligmann et al., 2008).

Finally, at the physiological level, we propose to investigate the role of compensatory ocular saccades as it seems that different patterns of saccadic response may predict different profiles of patient compensation (Macdougall and Curthoys, 2012). Correlations between objective and subjective cognitive measures on the one hand, and ocular saccades on the other hand, would allow to understand the seemingly random inter-individual differences in patients populations. In addition, physiological measures of brain volume and brain connectivity modifications could also help define different compensatory profiles. Neuroanatomical studies have previously demonstrated that CVS measuring the vestibular impairment were correlated with structural brain changes such as GMV (Helmchen et al., 2011; Hong et al., 2014; Lacour et al., 2016). Recent research also shows asymmetric cerebellar hyperactivity in patients with vestibular migraine, which could be linked to compensation after vestibular rehabilitation (Liu et al., 2020). Based on these findings, we suggest that patients who are compensated at the postural level (hence with mild clinical signs of vestibular impairment and lower subjective complaints) could exhibit an increase in GMV in specific areas such as visual cortices and cerebellum, similar to what has been observed by Hong et al. (2014). We suggest that this increase could be linked to changes in performance in the objective neuropsychological measures, differentiating between compensated and non-compensated patients. Reciprocally, the increases in GMV that Hong et al. (2014) found in the vermis and the prefrontal cortex could be related to visual dependence.

If our cognitive-vestibular compensation hypothesis is incorrect, and deafferentation is the sole cause of cognitive difficulties, VL patients with reduced objective cognitive performance should present a high level of physical and subjective cognitive disorders, whatever their degree of physiological compensation. Measuring compensation in vestibular-damaged patient is challenging, and interesting new perspectives have recently emerged such as trying to harmonise physiological measurements through a compensation index based on functional balance performance (Verbecque et al., 2021). The use of a new computational model of the vestibular system may also contribute to more fine-grained measures of cognitive costs associated with postural compensation (Mast and Ellis, 2015; Ellis and Mast, 2017). Cognitive rehabilitation such as mental imagery training in BVL patients could reduce physiological symptoms particularly in BVL patients that learn to rely more on anticipated sensory input and less on the impaired sensory measures (Ellis et al., 2018).

Investigating the cognitive-vestibular compensation hypothesis would allow for a better understanding of how the compensation mechanism operates; whether the patient is aware of this adaptation process; and which measures can be used to disentangle between compensated and non-compensated profiles. It would also open the door to the publication of non-significant results of objective cognitive function deficits in vestibular-damaged patients, when these data contrast with significant subjective cognitive or physiological measures. The exact cognitive cost of vestibular compensation might thus be objectivated.

DATA AVAILABILITY STATEMENT

The original contributions presented in the study are included in the article/supplementary material, further inquiries can be directed to the corresponding author/s.

AUTHOR CONTRIBUTIONS

EL wrote the manuscript. ME and ND critically revised the manuscript for important intellectual content. ND supported the initial funding. JV and MV made significant contributions to the manuscript. NP critically revised the manuscript and coordinated the collaboration to write the manuscript and provide the second funding. All authors contributed to the article and approved the submitted version.

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A Proposal for a Unifying Set of Definitions of Fatigue

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In this paper, we propose a set of unifying definitions that are useful in all areas of fatigue research while remaining neutral to the various theories about fatigue. We first set up two criteria and four desiderata that a definition for interdisciplinary use needs to fulfill: (i) non-circularity, (ii) finiteness, (iii) broadness, (iv) precision, (v) neutrality, and (vi) phenomenon-focus. We argue that other existing attempts to unify definitions within fatigue research do not fulfill all of these criteria and desiderata. Instead, we argue for a set of stipulative definitions, centered around performance measures and subjective estimations, is required in order to maximize clarity. In total, a set of 13 distinct definitions of fatigue and fatigue-related phenomena is presented. These definitions will help facilitate communication between different researchers, link phenomena from divergent research fields together, facilitate application and knowledge production, and increase the specificity for hypothesis testing.

Keywords: fatigue, definitions, fatigability, effort, sensation of fatigue, performance of fatigue

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INTRODUCTION

Fatigue is a phenomenon studied in various research fields, such as cognitive neuroscience, exercise physiology, psychology, and the medical sciences. The sentiment that we need a good and widely accepted set of definitions of fatigue, and related terms, has been echoed by several authors (Hockey, 2013; Kluger et al., 2013; Pattyn et al., 2018). For example, different studies denote the phenomenon of decreased cognitive performance after a period of activity as; central fatigue (Friedman et al., 2007; Kluger et al., 2013), cognitive fatigue (Bailey et al., 2007; Ackerman and Kanfer, 2009; Wylie and Flashman, 2017), mental fatigue (Inzlicht et al., 2014), fatigability (Kluger et al., 2013), cognitive fatigability (Walker et al., 2019), and ego-depletion (Baumeister et al., 2018). The variety of concepts used for the same phenomenon, both within and between different research fields, hinders interdisciplinary collaboration and has the possibility to generate confusion, miscommunication, which affects knowledge production. Additionally, the use of different terms in different fields hinders communication to the extent that advances are kept within one field and do not reach researchers in other fields, leading to the "reinventing-the-wheel" phenomenon. A more ethical problem would occur, if a substantial problem in the research of a phenomenon is detected in one field but not communicated to other fields, since it would expose participants unnecessarily to experiments and potential harms. For example, several multilab studies (Hagger et al., 2016; Dang et al., 2021; Vohs et al., 2021), and several meta-analyses (Carter et al., 2015) have not been able to demonstrate an *ego-depletion* effect in healthy adults, which should be taken into considerations for researchers in other fields that are using similar experimental designs. These problems could be handled once definitions are applied to all areas of fatigue research.

In section "What is Wanted From a Definition to Increase Crossdisciplinarity Communication?," we propose four desiderata that any definition should fulfill to improve crossdisciplinarity communication. We exemplify how these desiderata can be used to evaluate common definitions in fatigue research. In section "Defining the Performance Part of Fatigue" we define the performance aspect of fatigue. In section "Defining the Subjective Estimation Part of Fatigue" we define subjective estimation of fatigue. From section "Defining the Performance Part of Fatigue" and "Defining the Subjective Estimation Part of Fatigue," a set of 13 unifying definitions are generated (summarized in **Table 1**), that are useful in all areas of fatigue research. Technical terms can be found in the Glossary, to guide the reader through the definitions.

WHAT IS WANTED FROM A DEFINITION TO INCREASE CROSSDISCIPLINARITY COMMUNICATION?

To increase crossdisciplinarity communication, we need some form of consensus about the central terms used to describe the phenomena the field aims to explore. A disagreement about a phenomenon, e.g., fatigue, is interesting, and could potentially lead to studies advancing the field, but a disagreement that is just the result of attaching different meanings to the same terms, a mere verbal dispute, is neither interesting nor productive. For us to have interesting *dis*agreements about fatigue, mental fatigue, etc., we must first have an agreement about the meaning of the terms. We have to agree to disagree, as it were. For that, we need definitions. The most common definitions in science are real and stipulative definitions. Importantly, what counts as a successful or unsuccessful definition is different for real definitions and stipulative definition. A real definition could be right or wrong (e.g., it could be wrong because it is contradicted by empirical evidence), but a stipulative definition is useful or not useful (e.g., it could be circular, or too narrow/broad/vague to be useful for a particular purpose). Real definitions are when we relate a *species*, the basic/smaller units of classification, to a *genus*, the higher order/group unit of classification. For example, male and female are two different species of the genus adult human beings. A real definition aims at finding the real or essential characteristics of the thing or phenomenon in question. To discover the real definition of a term one needs to investigate the thing or things denoted by the term. However, useful this might be in other scientific enterprises, in the field of fatigue we would argue, the classifying units are at the moment too vague. Thus, to increase crossdisciplinarity communication, we would argue the need right now is rather to find a way to reach consensus about the terms used, meaning that we need a set of stipulative definitions. A stipulative definition is the introduction of a new term:

> "For instance, the sentence 'Someone has ARDS if and only if she has an acute respiratory distress syndrome' is a definition of the new term 'ARDS.' It introduces this term as a short name for the longer sequence 'acute respiratory distress syndrome' and establishes the syntax of its use ('x has ARDS') in order that one avoids to say, for example, 'x bears ARDS.' Thus, a [stipulative] definition is always a nominal definition (nomen = name), and as such, it is a stipulative sentence that introduces a term, and is never a constative or descriptive sentence to state or assert something. For instance, the term "ARDS" describes or reports nothing. Definitions are uninformative. They are only regulative and useful." (Sadegh-Zadeh, 2015, p. 95).

Technically, a definition[1] such as "X is Y," is made up of the *definiendum*, the word or phrase defined in a definition (in this case X), and the *definiens (*plural *definientia*), the sentence or phrase that defines the definiendum (in this case Y). The goal of this paper is to suggest novel and better definitions of key terms. In other words, we will offer stipulative definitions of key terms in the manner of: "Fatigue *is* X" or "X *is* Y and Z." But what does "is" mean in a stipulative definition? When we say "X *is* Y," we mean that everything that is true of X is also true of Y, and everything that is false of X is also false of Y. The "is" in a stipulative definition can thus be translated to "if and only if" (called a *biconditional*) e.g., "X *if and only if* Y."

There are two minimal criteria that any stipulative definition needs to fulfill in order to be a successful definition (see chapter 6 in Sadegh-Zadeh (2015) for further discussion on definitions):

i) *Non-circularity*, i.e., no part of the term defined (definiendum) should be defined by itself or have already been used in the definitions of a prior definition.
ii) *Finiteness:* the definition chain cannot be infinite, i.e., at the end of our definitions, there should be some *primitives* or undefined terms that typically get their meaning from *ostensive procedures*.

For example, if fatigue was defined as "the feeling of exhaustion, weariness or lack of energy," and exhaustion in turn was defined by "the feeling of fatigue, weariness or lack of energy," then fatigue would both be the definiendum and the definiens, making it circular. The purpose of the *finiteness* criterion is that the definition needs in some way be related back to reality. Thus, it might be that X is defined by Y, and Y is defined by Z, but at the end of the definition chain Z needs to be defined by something like Q, where Q is an undefined primitive which gets its meaning from meta-linguistic practices, such as an ostensive procedure, e.g., "This (pointing to Q) is Q," "the color marine blue is this (pointing to an object that is marine blue)" or "what you are feeling now (indicating this moment) is angst." In our natural language, words and terms do not always fulfill these two criteria, but regardless of how hard it is in our scientific language they need to be fulfilled or the terms lose their connection to reality.

However, fulfilling only these minimal criteria will not be sufficient to serve as candidates for useful and clear definitions across research fields. In addition, we propose four *desiderata* i.e., something that we want or desire from a definition to be considered a good definition in terms of it being applicable across research fields.

iii) Broadness: the definitions are broad enough to be used in a variety of research fields.

[1] Henceforth we will only work with stipulative definitions.

TABLE 1 | The definitions.

Construct	Definition	More lose paraphrases of the definitions
Fatigability (1)	*If and only if* there is the decrement in magnitude or rate of a performance criterion relative to a reference value over a given time of task performance.	*Is the* decrement in performance between two timepoints. *Is the* decrement *in performance over a consecutive time.*
X fatigability (2)	*If and only* if the fatigability of X.	*Is the* decrement *in X performance over a consecutive time.*
Effort (3)	*If an only if* there are forces exerted by the individual in order to reach some goal.	
Sensation of fatigue (4)	*If and only if* there is a sensation of (i) feeling the need for rest, or (ii) mismatch between effort expended and actual performance.	*Is the feeling of either needing to rest or* mismatch between effort expended and actual performance.
Sensation of X fatigue (5)	*If and only if* there is a sensation of (i) feeling the need for X rest, or (ii) mismatch between X effort expended and actual X performance.	*Is the feeling of either the need for X rest or* mismatch between X effort expended and actual X performance.
State fatigue (6)	*If and only if* there is a momentary sensation of fatigue.	*Is the estimation of sensation of fatigue at this moment.*
Trait fatigue (7)	*If and only if* there is an overall disposition and intensity of fatigability and sensation of fatigue, during T period of time.	*Is the general tendency of fatigability and sensation of fatigue.*
Prolonged state fatigue (8)	*If and only if* there is an overall disposition and intensity of fatigability and sensation of fatigue, during the last week.	*Is the general tendency of fatigability and sensation of fatigue, after recent events.*
State X fatigue (9)	*If and only if* there is a momentary sensation of X fatigue.	*Is the estimation of sensation of X fatigue at this moment.*
Trait X fatigue (10)	*If and only if* there is an overall disposition and intensity of X fatigability and sensation of X fatigue, during T period of time.	*Is the general tendency of X fatigability and sensation of X fatigue.*
Prolonged state X fatigue (11)	*If and only if* there is an overall disposition and intensity of X fatigability and sensation of X fatigue, during the last week.	*Is the general tendency of X fatigability and sensation of X fatigue, after recent events.*
Pathological fatigue (12)	*If and only if* the trait fatigue estimated by the individual or caregiver to interferes with usual and desired activities.	*Is when general tendency of fatigability and sensation of fatigue is perceived to interfere with everyday life.*
Pathological X fatigue (13)	If and only if the pathological fatigue identifiable as caused by, or consequence of, or sequel to a disease/disorder/trauma and *if and only if* the level of trait fatigue is worse after the disease/disorder/trauma than before.	*Is when the general tendency of fatigability and sensation of fatigue is perceived to interfere with everyday life and is caused by, or consequence of, or sequel of X.*

iv) Precision: the definitions are precise enough to avoid multiple interpretations.
v) Neutrality: the definitions should not appeal or depend on any particular theory.
vi) Phenomenon-focus: our definitions to a minimal extent involve explanations, since our goal is to reach consensus about the phenomenon explained (explanandum) and not about the explanations (explanans).

We will show how these criteria and desiderata can be successfully fulfilled. We will use an example of a diagnosis, and though this paper is not aiming to provide any diagnosis of fatigue, it is a useful example of how the regulation of a stipulative definition work. "Agoraphobia is characterized by marked and excessive fear or anxiety that occurs in response to multiple situations where escape might be difficult or help might not be available" (WHO, 2021). This fulfills the *non-circular* and *finiteness* criteria since none of the terms "excessive fear," "excessive anxiety," "escape," "help" is defined by "agoraphobia" and the definition chain ends with primitives or diagnostic criteria's. It fulfills the *broadness* desideratum since it is applicable and used in the same way in various fields. It fulfills the *precision* desideratum since it gives criteria for what it means for someone to have agoraphobia. It does not depend on any specific theory of fear or anxiety and thus fulfills the *neutrality* desideratum. Lastly it fulfills *phenomenon-focus,* since the "occurs in response to," is not an explanation but rather part of the state of affair and thus a phenomenon.

Now that we have established how the criteria and the desiderata work, we analyze two definitions of fatigue used in the literature. Although both fail to fulfill all four desiderata, they both are successful in highlighting important properties or structures needed to be taken into account in any general definition of fatigue.

> "[Fatigue is] a subjective lack of physical and/or mental energy [and] which is perceived by the individual or caregiver to interfere with usual and desired activities." (published by The Council for Clinical Practice Guidelines and the Paralyzed Veterans of America in 1998 cited in Béthoux (2006).

We can begin our analysis by defining "subjective lack of," "mental energy," "physical energy," "perceive to interfere," "caretaker," and "usual and desired activities." If none of these terms and their defining phrases refer back to any of the other, they fulfill the *non-circular* criterion, e.g., defining "mental energy" should not involve the terms "subjective lack of" or "fatigue." If we suppose the continuation of the defining process ends with a satisfying primitive, then the *finiteness* criterion is fulfilled. In this case, a possible end to the definition chain could be "mental energy is this or that (pointing out a behavior, a feeling or an experience)" or "a caregiver is such and such." It might be jarring to say that "mental energy" is a primitive, but for the sake of argument we suppose that it works, and we will shortly show that the concept is problematic for other reasons. The definition does fulfill the criterion of *phenomenon-focus*, since no part of the definition invokes an explanation.

This definition of fatigue is used within the medical science, but does it fulfill the desideratum of *broadness*? Both yes and no. As a specific definition of what we later will call subjective estimations of fatigue, it could be used in various field from exercise science to medicine. As an overarching definition aiming to encapsulate all aspects of fatigue, however, it would not meet the *broadness* desideratum, since there are some parts of the study of fatigue that are not captured. Typical study designs in other fields would not fulfill this definition. For example, an exercise scientist aiming to study the effects of intense training on cognition or a social psychologist wanting to study the effect of sustained attention on cognition, both set up their experiments in a way that the participants either do prolonged period (e.g., 1 h) of intense training or a sustained attention task followed by a cognitive task. However, the participants respond to the training or the sustained attention task, if the activity is not "perceived as interfering with usual and desired activities," such as continuing working or studying after the experiment, then this would not be fatigue according to the definition. As such the definition is too narrow.

Also, the invocation of mental energy creates a problem. Fatigue has often been described with the help of the metaphors such as "lack of energy" or "running on fumes." A metaphor "is understanding and experiencing one kind of thing in terms of another" (Lakoff and Johnson, 2003, p. 5), to reveal or create structural similarities. A metaphor follows the formula of "X is *like* Y," which is different from "X *is* Y." In some cases, it can create a false equivalence of the two, leading to mistaking the map for the world, as it were. However, intuitive a metaphor might be, it is vague by nature and should be avoided as part of a stipulative definition for the sake of clarity (Hockey, 2013; Pattyn et al., 2018), and as such it fails the *precision* desideratum. On the other hand, if mental energy is not used as a metaphor, it could be understood as a theoretical construct. If this is the case then it should not be interpreted or used differently between theories, otherwise it can seriously confuse the debate. For example, are we referring with "mental energy" to Spearman's theory of intelligence (Spearman, 1927) or O'Connor's three dimensional model (O'Connor, 2006) or something else? Thus, by using mental energy as a theoretical construct, it will fail the *neutrality* desideratum.

A commonly used and promising candidate for being a unifying definition of fatigue is one given by Aaronson et al. (1999). They set out to propose a definition of fatigue based on their research:

> "[Fatigue is] The awareness of a decreased capacity for physical and/or mental activity due to an imbalance in the availability, utilization, and/or restoration of resources needed to perform activity (. . .) Fatigue occurs when this system is out of balance – that is, when there are insufficient resources either because the demand or need is too great or because mechanisms of utilization and restoration are disturbed." (Aaronson et al., 1999, p. 46).

This definition of fatigue is made up of two propositions and a connective that links the two propositions. (A) "the awareness of a decreased capacity," (B) "imbalance in the availability, utilization, and/or restoration of resources needed to perform activity," and (C) the connective "due to," that links (A) and (B) together. We will now examine how each part fails to fulfill at least one of our desiderata for consensus use, and as a consequence either leads to confusion, misunderstanding, or a likelihood not to interpret it literally.

The problem with (A) is that the use of "awareness" has several disadvantages which make it fail to fulfill both the *broadness* and *precision* desiderata. Firstly, awareness is too cognitive and enables an interpretation such as "Diana abstractly theorizes that she has a decreased capacity." The fact that this interpretation is possible, together with other possibilities like "Diana *feels* that she has a decreased capacity," makes it too broad and thus fails the *precision* desideratum. Secondly, "awareness" is a success term. Just like seeing is a success term to the extent that when our visual perception does not match reality, we call it illusion or hallucination, rather than a state of seeing. The same is true for awareness, i.e., if Diana is aware of X, then X is the case. This on the other hand makes it too precise, and thus fails the *broadness* desideratum, since in many fields of study, such as exercise science and medical science, one is interested in studying situation where individuals have the feeling of not being able to continue with an activity, without it actually being the case that they cannot continue with the activity.

The problem with (B) is that it fails the desideratum of *phenomenon-focus*, since it is an explanation not required for identifying the phenomenon. The problem with (C) is that "due to" is too strong, and fails the *broadness* desideratum. A literal interpretation of this definition requires Diana to have the awareness of the cause of the imbalance. In some circumstance one might know the cause, like after a long day at work, but there are other situations, perhaps due to an undetected tumor or hormonal imbalance it is not known and would imply that a patient seeking help for fatigue is not fatigued according to this definition, and thus this definition would not be useful when studying patients suffering from fatigue or fatigue related problems within the medical sciences. There are, however, many good parts to the definition, which we will come back to later toward the end of section "Defining the Subjective Estimation Part of Fatigue", but we first need to define some related terms.

In a paper aiming to put forward a unifying taxonomy for fatigue, Kluger et al. (2013) argue that when dealing with fatigue, we should distinguish between the "subjective sensations" and the "objective changes in performance." We will make a similar distinction between the phenomenon identified by "subjective estimations," by Kluger et al. (2013) called perceptions of fatigue, and the phenomenon identified by "objective measurements," which denote the performance of fatigue. Section "Defining the Performance Part of Fatigue" deals with the performance of fatigue, and section "Defining the Subjective Estimation Part of Fatigue" with the subjective estimations of fatigue.

DEFINING THE PERFORMANCE PART OF FATIGUE

In this section, we will start out with suggesting novel or improved definitions of phenomena related to the performance part of fatigue. We will anchor our positive account in already established definitions from the literature and see if they meet our desiderata. In this section, definition (1 and 2) will be presented.

Kluger et al. (2013) discuss the concept of fatigability at the performance level of fatigue:

> "fatigability is defined as the magnitude or rate of change in a performance criterion relative to a reference value over a given time of task performance or measure of mechanical output" (Kluger et al., 2013, p. 411).

This definition fulfills most of our desiderata except for *precision*, since it does not specify in which direction the change needs to go in order for it to count as fatigability. The last part, "measure of mechanical output," is made redundant by the "relative to a reference value over a given time." We would argue that the "over a given time" would suffice. With these few alterations, we suggest the following definition.

(1) Fatigability is the decrement in magnitude or rate of change in a performance criterion relative to a reference value over a given time of task performance.

It is important to highlight here that the definition of fatigability excluded the possibility of something being fatigability when there is no performance change or lack of improvement. For example, in Skau et al. (2019), we found that patients suffering from problems with fatigue after a mild traumatic brain injury (TBI) performed on a cognitive task (Digit Symbol Coding) (Wechsler, 2010) equivalently well at two time points that were intermediated with 1.5 h of intense cognitive activity. At the same time, healthy controls improved their performance. According to definition (1), this would not be fatigability in the TBI patients. Of course, one could change the definition to involve a healthy population reference group. By not improving like they possibly would, one could call it fatigability in relation to some fatigability quotient. Although desirable, we would argue that it is not needed to identify the phenomenon in question and is not needed for our purposes. One could also define another term, let us say "improvability," and claim that they fail to fulfill that criterion. That is also a desirable thing to do, but again, it is not needed to talk about fatigability. In the same study, the TBI patients also rated their state fatigue (more on this later) before and after the cognitive activity. That they reported being more fatigued after the experiment compared to before does not mean fatigability since the task of reporting on one's subjective state is not a performance but rather an estimation.

What type of fatigability a researcher is interested in varies, be it physical, mental, cognitive or emotional fatigability. Whether cognitive or emotional fatigability exist depends on what is involved in the term "cognitive" or "emotional," and is part of the theories of the different research domains. With definition (1) we can add the domain of inquiry/the domain affected to the definition, e.g., "X fatigability," where X can be replaced by different domains such as "*cognitive* fatigability" or "*physical* fatigability." This would help communication between different research field and generate transparency. For example, from definition (1), we can derive a fatigability effect, i.e., the difference in performance between time point t_1 and time point t_2, and the larger difference, the more fatigability. In social psychology, the focus has been on the ego-depletion effect, which is identical to the fatigability effect. It is only the theories [such as the strength model (Baumeister et al., 2018) and motivational theory (Inzlicht et al., 2014)] and explanatory constructs of willpower and self-control that are different from other fields such as medicine. For example, in both social psychology (Carter et al., 2015), exercise science (Yanagisawa et al., 2010; Alves et al., 2012), and medical sciences (Skau et al., 2019) the unit of measurement are change in reaction time on a Stroop task, and could thus be denoted "cognitive fatigability," since the Stroop task is a classic cognitive task. Thus, we propose the following definition:

(2) X fatigability is the fatigability of X.

An alternative for researchers that would still want to keep their "within-research field terminology," such as "ego-depletion," is that the applicable definition is adapted and integrated, e.g., one could write the following: "Our results show an ego-depletion effect (the within research field terminology), in other words a cognitive fatigability effect (the cross-research field terminology)." The same holds for phenomena such as physical fatigue and motor fatigue. As definition (2) is formulated, only the domain affected (X) is determined. If one wants, it is possible to add "where the effort is of Y" e.g., cognitive fatigability where the effort is of physical/mental/cognitive/emotional performance or something else.

A term often related to performances is that of peripheral fatigue. Torres-Harding and Jason define Peripheral fatigue as: "failure to sustain force or power output because of 'failure in neuromuscular transmission, sarcolemmal excitation, or excitation-contraction coupling,' implying neuromuscular dysfunction outside of the central nervous system, or CNS" (Torres-Harding and Jason, 2005). This is how many definitions of peripheral fatigue are constructed (Wylie and Flashman, 2017), however, if we take out the explanatory part, we end up with "failure to sustain force or power output." This failure would be equivalent to *fatigability* or *physical fatigability*, which is why we do not include peripheral fatigue in our set of definitions. The same argument goes for the term central fatigue. We do not advise against the use of central and peripheral fatigue, since it is part of many taxonomies and is used relatively consistent in the literature, but we want to point out that it often serves as an explanation of mechanisms behind a phenomenon, and we are here only interested in consensus about the phenomenon explained.

DEFINING THE SUBJECTIVE ESTIMATION PART OF FATIGUE

In this section we will discuss the definitions of the subjective estimation part of fatigue. Here definition (3–13) will be presented in a consecutive order. As in the previous section, we will begin our discussion with the work of Kluger et al. (2013), who use the term "perceptions of fatigue" as follows:

> "Perceptions of fatigue refer to subjective sensations of weariness, increasing sense of effort, mismatch between effort expended and actual performance, or exhaustion (Kluger et al., 2013, p. 411)".

Here, the authors define perception as a subjective sensation, but we propose to only denote it "sensation of," for the sake of brevity[2]. The above definition is a disjunctive made up of four parts "weariness," "increasing sense of effort," "mismatch between effort expended and actual performance," and "exhaustion." We will first discuss "increasing sense of effort" and "mismatch between effort expended and actual performance," since it introduces the term "effort." Kluger et al. (2013) do not define effort, but Massin (2017) showed in an overview of different accounts of effort (i.e., theories of effort), that both the resource-based accounts and the force-based accounts are functionally equivalent, but that force-based accounts are explanatorily more fundamental. Even though our desideratum of *neutrality* implies that we should avoid definitions of terms that depend on a particular theory, since the force-based account and the resource-based accounts are functionally equivalent, this definition will be as broad as possible. Thus, we will use the force-based account statement of effort.

(3) (Effort is) the forces exerted (by the individual) in order to reach some goal (Massin, 2017, p. 243).

Since the goal is the individual's goal, "the force exerted" needs to be part of the individual's volition, i.e., that applying the force to some extent is optional. The optionally applied force is aimed to meet the demands, which the individual perceives, to be required to reach the goal. Here the word "perceives" is used in its broadest form, in a way that a mouse perceives what to do when facing an obstacle. Although there is a close relationship between effort and fatigue, having sensation of "increasing sense of effort" with our definition (3) would mean that every time there is a sensation of increasing sense of "force exerted," there would be a sensation of fatigue, which would be too broad to be useful. Instead, we propose eliminating "increased sense of effort" but keeping "mismatch between effort and actual performance."

Regarding the concepts of weariness and exhaustion, we need to be careful not to break the criterion of *non-circularity*, as illustrated in section "What is Wanted From a Definition to Increase Crossdisciplinarity Communication?". Choosing to define fatigue in terms of weariness and exhaustion sets certain strict limits on how they can be defined. While it is tempting to define fatigue in terms of weariness and/or exhaustion, and similarly tempting to define exhaustion and/or weariness in terms of fatigue, we must choose one or the other to avoid circularity. Given these considerations, how do Kluger et al. (2013) define weariness and exhaustion? Unfortunately, neither weariness nor exhaustion is expanded upon by them so we cannot know what exact definition they had in mind. Possibly, they had no specific definitions in mind, but instead wanted the terms to be treated either as primitives or as undefined terms in order to be defined in the future or by others. While, as was just argued, we *could* define fatigue in terms of weariness and/or exhaustion, it is still an open question whether we *ought* to do so.

When it comes to mechanistic explanations or definitions of performance, there are cases where fatigue and exhaustion are defined differently (Aaronson et al., 1999). Thus, these would be available as means of defining fatigue without breaking the criterion of *non-circularity*. Unfortunately, in the definition of the sensation of fatigue it is specifically *the sensation* of exhaustion and weariness that is part of the definition. When it comes to the sensation of fatigue, exhaustion and weariness, they are commonly used as synonyms (Kristensen et al., 2005; Loy et al., 2018; Boolani et al., 2019), which would reintroduce the circularity. Even so, attempts to define the sensation of fatigue do point at two other phenomena that are not used as synonymous of fatigue and which might serve better in terms of fulfilling the four desiderata: *exertion* and *tiredness*.

One such attempt is Phillips' review of definitions of fatigue. He highlights that any whole definition of fatigue needs to take into account the experience of fatigue (which is what this section is about) and he also highlights the importance of exertion and tiredness:

> "However, popular use of the word in everyday language in phrases like "mental fatigue," "adrenal fatigue" or "battle fatigue" do seem to reflect dictionary definitions in that someone or something is "tired" to the extreme specifically because of some overuse, overexposure or exertion. Capturing this would thus seem to be important for the face validity of a whole definition of fatigue (. . .) A whole definition would do well to maintain face validity by describing how fatigue is experienced as a result of exertion" (Phillips, 2015, p. 49 and 53).

Let us consider exertion and tiredness. As we have defined effort previously, it seems exertion cannot be understood as an independent term [for a good discussion of effort and exertion see Steele (2021)]. Exertion is accounted for by definition (3) of effort. If we expand the sensation of "mismatch between effort and actual performance" with our definition (3) it becomes "mismatch between 'the forces exerted by the individual in order to reach some goal' and actual performance." Thus, unfortunately, exertion does not add anything that is not already accounted for by our definition of effort.

Tiredness, on the other hand, is not accounted for by our definition of effort. In Phillips' review it is made clear that although the experience of tiredness is part of the experience of fatigue, tiredness should not necessitate sleepiness (Phillips, 2015), i.e., it should not be the case that every time Diana feels sleepy, she also feels fatigue, violating the *precision* desideratum. But what does "tiredness that does not necessitate sleepiness" mean? We again suggest that we leave the term tiredness behind and instead use "feeling the need for rest." The "need for rest" is both broad enough to include sensations of participants in exercise studies, as well as the reports from stroke patients. It is also precise enough since "need for rest" does not necessitate sleepiness. With these modifications, we propose the following definition:

[2] We are here following Kluger et al. (2013) in treating "sensation" and "perception" as synonymous. Some argue that we ought to make a distinction between them, see e.g., (Smith, 2002; Burge, 2010; Steele, 2021) for various suggestions on how to draw that distinction. Unfortunately, though Steele, Burge and Smith all agree that we ought to make that distinction, they disagree on how to draw it. For the purpose of this paper, we will not take a stance on this issue. The framework provided in this paper ought to be easy to expand upon with a distinction between perception and sensation if required.

(4) Sensation of fatigue is the sensation of (i) feeling the need for rest or (ii) mismatch between effort expended and actual performance.

We can now generate a further construct by adding a dimension/domain as we did in the previous section.

(5) Sensation of X fatigue is the sensation of (i) feeling the need for X rest, or (ii) mismatch between X effort expended and actual X performance.

Here X can be cognitive/mental/physical/emotional or whichever domain the research in question is about.

An additional important distinction to make is that between state and trait. State and trait are commonly used with constructs such as anxiety and fatigue. State usually refers to how an individual "feels here and now," whereas trait denotes something more latent that does not quickly change. An inconvenience with this terminology, is that state, in contrast to a process and event, is sometimes defined as something having homogenous temporal parts (Mulligan and Smith, 1986), thus the difference between state and trait cannot be reduced to having heterogenous or homogenous temporal parts. Thus, we need in our definition make sure that the difference is due to the transitory aspect of the state, and the long-lasting property of trait (Julian, 2011). Thus, we will keep this terminology due to its broad and consistent use, but in the definition make the time span more explicit.

(6) State fatigue is the momentary sensation of fatigue.

That state fatigue is momentary means that it can change relatively fast within minutes or hours as other sensations can. Definition (6) has a peculiar property that needs to be highlighted. If the condition of the sensation of fatigue is not fulfilled, then there is no state fatigue according to this definition, i.e., if Diana does not have any sensation of feeling the need for rest, or a mismatch between effort expended and actual performance, at this moment, then we cannot say that she has any state fatigue at all, but rather a lack of state fatigue.

On the other hand, trait fatigue is more stable and enduring and does not change rapidly but over weeks, months, or years.

(7) Trait fatigue is the overall disposition and intensity of fatigability and sensation of fatigue, during T period of time.

Since trait fatigue is defined as a disposition, it means that every human always has trait fatigue to a varying degree since a disposition to never have fatigability or sensation of fatigue is still a disposition. That the time clause (T period of time) is, to some extent, arbitrary. It could just as well be 3 weeks or few months or years. This period of time should best be fixed by the researchers within the different fields. Even if T was 3 weeks or years the same phenomenon is denoted, it is only different practices between the fields that are different. However, the time should not be much shorter since there is an intermediate phenomenon that is more stretched out in time than state fatigue but does not have the same characteristic of trait fatigue. This intermediate phenomenon is often recognized when trying to estimate trait moods. One does not want something unexpected that just happened recently, within a few days, to affect the estimation. We will denote this "prolonged state fatigue."

(8) Prolonged state fatigue is the overall disposition and intensity of fatigability and sensation of fatigue, during the last week.

The difference between the phenomenon of trait fatigue and prolonged state fatigue is the effect of recovery. Let us say that Diana has relatively low trait fatigue, but due to intense stress and lack of sleep during the workweek, her performance on Friday gets quickly worse, and she has an intense sensation of fatigue. However, one day of rest and a good night's sleep would change that. This would not be the case for trait fatigue, where only one night of sleep would not automatically change her dispositions.

All these definitions (6–8) can be extended to:

(9) State X fatigue is the momentary sensation of X fatigue.
(10) Trait X fatigue is the overall disposition and intensity of X fatigability and sensation of X fatigue, during T period of time.
(11) Prolonged state X fatigue is the overall disposition and intensity of X fatigability and sensation of X fatigue, during the last week.

Here X refers to a specific domain such as cognitive/mental/physical/emotional, whereas T is a period of time. The final definition is that of pathological fatigue. There are several diagnoses related to fatigue that require the cause of the fatigue to be identifiable, e.g., for cancer-related fatigue, the fatigue needs to be caused by cancer (Mitchell, 2010), for exhaustion disorder, the fatigue needs to be caused by a prolonged stressful work period or environment (Jonsdottir et al., 2013). We propose to divide it into two separate definitions.

(12) Pathological fatigue is the trait fatigue estimated by the individual or caregiver to interfere with usual and reasonable desired activities.
(13) Pathological X fatigue is the pathological fatigue identifiable as caused by, or consequence of, or sequel to a disease/disorder/trauma and if and only if the level of trait fatigue is worse after the disease/disorder/trauma than before.

Definition (12), similar to that of Béthoux (2006) (which was analyzed in section "What is Wanted From a Definition to Increase Crossdisciplinarity Communication?"), does not relate pathological fatigue to any source, whereas (13) does. The addition of "reasonable" desired activities should here be understood as activities that are within the realm of possibilities for the person to do, i.e., if Diana perceives her trait mental fatigue to interfere with her desire to run a marathon every day, that would not be reasonable in this sense and hence not pathological fatigue, whereas interference with spending time with friends and family or work would.

Definition (13) is not meant to exclude or replace any diagnoses. On the contrary, it is instead meant to relate the different diagnoses to the other definitions, e.g., cancer-related fatigue is pathological cancer fatigue, or exhaustion disorder is pathological stress-related fatigue. The biconditional within the definition (13) is added to enable the use of the broader and vaguer terms "the consequence of" or "sequel to." Otherwise, the definition would get the embarrassing property that even if an individual got a lower degree of trait fatigue after the disease/disorder/trauma that would be seen as pathological cancer fatigue. Definition (13) leaves it open for each fatigue-related problem to have additional symptoms or signs. For example, a sensitivity to light and sound is common for individuals suffering from fatigue after exhaustion disorder or TBI (Johansson and Rönnbäck, 2014), which is not part of definitions (1–13).

Now that all definitions are done one could try to generate a general definition of fatigue as we discussed in the end of section "What is Wanted From a Definition to Increase Crossdisciplinarity Communication?". If we change awareness to a sensation, then it is possible that Diana can have the sensation X, while X not being the case, e.g., Diana can have the sensation as of decreased capacity for physical activity, without the actual presence of a decreased capacity for physical activity. This would fulfill both the *broadness* and *precision* desiderata. A proposal would be that fatigue is "the presence of fatigability or the sensation of fatigue." Although this definition would solve the problems presented in section "What is Wanted From a Definition to Increase Crossdisciplinarity Communication?" (e.g., the disjunct would solve the "due to" problem), we would argue that such a definition will not be useful. There is still an open question about to what extent fatigability and sensation of fatigue are related, and as phenomena they are separate and indeed many times studied separately. Having the term "fatigue" defined as such could generate confusion since it would not be clear whether one studied fatigability, sensation of fatigue or both. To refer to fatigue in this way, as a disjunct, might be useful in everyday language, but we suggest that it ought to be avoided in scientific discourse. All definitions[3] are summarized in **Table 1**, together with possible paraphrases that keep the meaning of the more precise definitions.

[3] Some researchers have highlighted the distinction between active and passive fatigue, to separate between sensation of fatigue or fatiguability caused by intense work or by boredom. Passive fatigue is caused by prolonged, monotonous, boring work, whereas active fatigue is cause by prolonged task related work (Pattyn et al., 2018). This distinction is not possible without invoking an explanation (cause) or a theory dependent understanding of the specific terms in the definiens (boring, motivation). It is, however, possible for researchers interested in studying this to just add the causal part after any definition, e.g., passive fatigability is fatigability due to boredom and active fatigability is fatigability due to task related activity.

CONCLUSION

The proposed set of unified minimally theoretical definitions are summarized in **Table 1**. These constructs can now be imputed with empirical data. Taxonomies can be created or related to the definitions from different fields and theories, that can help settle both verbal or a genuine difference between studies/theories/research fields. It can be applied when comparing the over 250 different scales created to measure fatigue (Hjollund et al., 2007). The definitions are created to the effect that constructs such as emotional fatigue, physical fatigue, or stress fatigue, as other researchers has investigated, can be applied, for instance to definitions (2, 5, 8–10). The definitions (2, 5, 8–10) are also formulated in such a way that such definitions might be redundant, and most importantly, all definiens are usable in all research fields of fatigue.

DATA AVAILABILITY STATEMENT

The original contributions presented in the study are included in the article/supplementary material, further inquiries can be directed to the corresponding author/s.

AUTHOR CONTRIBUTIONS

SS: conceptualization and writing first draft. KS: writing – review and editing. H-GK: writing – review and editing. All authors contributed to the article and approved the submitted version.

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GLOSSARY

Terms	Definition
Stipulative definition	A sentence that introduces a new term and standardizes and regulates how that particular term is to be used.
Real definition	When we relate a species, the basic/smaller units of classification, to a genus, the higher order/group unit of classification.
Definiendum	The term defined in a definition.
Definiens (definientia)	The sentence or phrase that defines the definiendum.
Desideratum (Desiderata)	Something that is considered desirable or favorable.
Explanandum	A phenomenon (term or a sentence) explained in an explanation.
Explanans	The sentence that explains the explanandum.
Primitive	An undefined term that cannot be defined further and typically gets its meaning from ostensive procedures.
Connective	Words or phrases that connects two or more sentences, clauses or phrases.
Conjunction (*connective*)	"And." The sentence "A and B" is true if A is true and B is true, otherwise the sentence is false.
Disjunction *(connective)*	"Or." The sentence "A or B" is true if A is true or B is true or both A and B is true, otherwise the sentence is false.
Biconditional *(connective)*	"If and only if." The sentence "If and only if A then B" is true, if A and B are true or false at the same time otherwise the sentence is false.
Ostensive procedure	Introducing the meaning of something by pointing out or showing.

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The Impact of Cognitive and Physical Effort Exertion on Physical Effort Decisions: A Pilot Experiment

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Research suggests that cognitive fatigue has a negative impact on physical activity participation. However, the mechanisms underlying this effect are yet unclear. Using an effort-based decision-making paradigm, we examined whether individuals weigh physical effort-costs more strongly when they are cognitively or physically fatigued. Twenty university students visited the lab on three occasions. On each visit, participants underwent a manipulation that was designed to either induce cognitive fatigue (i.e., 2-back task), physical fatigue (i.e., handgrip exercise), or served as a control condition (i.e., documentary watching). After the manipulations, participants performed an effort-based decision-making task in which they decided for 125 offers whether they accepted the offer to exert the required level of physical effort to obtain rewards that varied in value. The probability to accept offers declined with increasing effort requirements whereas the general probability to accept offers was not reduced by any of the experimental conditions. As expected, the decline in accepted offers with increasing effort requirements was stronger after prolonged exertion of physical effort compared to the control condition. Unexpectedly, this effect was not found after exerting cognitive effort, and exploratory analyses revealed that the impact of physical effort exertion on physical effort-based decisions was stronger than that of cognitive effort exertion. These findings suggest that people weight future physical effort-costs more strongly after exerting physical effort, whereas we could not find any evidence for this after exerting cognitive effort. We discuss multiple explanations for this discrepancy, and outline possibilities for future research.

Keywords: cognitive fatigue, motivation, effort-based decision-making, physical activity, exercise psychology

INTRODUCTION

Participation in sufficient physical activity is paramount for health and well-being (Strain et al., 2020). However, inactivity levels in high-income western countries have increased from 31.6% in 2001 to 36.8% in 2016 (Guthold et al., 2018) and insufficient physical activity remains one of the leading causes of non-communicable diseases worldwide. Interestingly, many individuals not meeting the recommended levels of physical activity would like to be more active (Rhodes and De Bruijn, 2013). It is estimated that only 54% of people who intend to be physically active actually achieve their goal. Advancing our understanding of the psychological barriers for engaging in physical activity therefore is of vital relevance.

A growing body of literature suggests that cognitive effort exertion and cognitive fatigue negatively affect physical activity behavior (Van Cutsem et al., 2017b; Brown et al., 2020). Cognitive fatigue is a complex psychobiological state resulting from cognitive effort exertion and is characterized by feelings of low energy, low positive affective states and a reduced motivation to exert effort (van der Linden, 2011; Hockey, 2013). Importantly, it is expected that cognitive fatigue not only reduces motivation for cognitive effort but also for physical effort (Martin et al., 2018; Müller and Apps, 2019). While previous studies indeed find negative effects of prior cognitive exertion and fatigue on subsequent physical *behavior* (Van Cutsem et al., 2017b; Brown et al., 2020), previous studies did not find evidence for a reduced *motivation* for exerting physical effort when being fatigued after performing cognitively demanding tasks (Van Cutsem et al., 2017b). However, these studies used self-reports to assess motivation, which are inherently limited by participants' ability and willingness to express this motivation accurately (Chong et al., 2016; Massar et al., 2018; Brown and Bray, 2019). Therefore, the motivational consequences of prior cognitive effort exertion (and consequential fatigue) for subsequent physical behavior require additional examination.

Examining effort-based decision-making provides an alternative approach to uncover potential motivational consequences of cognitive fatigue for physical activity participation. Specifically, Müller and Apps (2019) suggest that fatigue modulates the cost-benefit analyses underlying the decision to exert future effort. The costs of effort are expected to weigh more heavily within the cost-benefit trade-off when someone is fatigued (Kanfer, 2011), which reduces the probability to engage in effortful activities (Müller and Apps, 2019). This motivational consequence of fatigue is thought to cross-domains (Müller and Apps, 2019), meaning that cognitive fatigue changes the decision-making process for both cognitive and physical effort. Thus, fatigue has been characterized by a trans-domain intolerance of effort (i.e., "the intolerance of *any* effort," Thorndike, 1914), which could explain why cognitive fatigue may negatively affect subsequent decisions to engage in physical behavior (cf. see Marcora, 2010, Pageaux, 2014; Martin et al., 2018 for alternative approaches focusing on changes in the *perception* of effort in a fatigued state).

Recent studies tapping into the effort-based decision-making process for physical behavior in a fatigued state provide preliminary support for changes in cost-benefit analyses. Brown and Bray (2019) showed that after a cognitively fatiguing task, participants intended to perform (and actually performed) a subsequent cycling exercise at lower intensities than after watching a documentary. Cognitive fatigue may have led to an increase in the perception of effort (Marcora, 2010; Pageaux, 2014), a reduced willingness to exert effort (Müller and Apps, 2019), or both (Martin et al., 2018). Furthermore, Harris and Bray (2019, 2021) showed that after a cognitively demanding Stroop task, the self-reported cost-benefit balance for a subsequent cycling task turned out more negatively than after watching a documentary, and this reduced the probability that participants chose to cycle. Finally, Iodice et al. (2017) showed that participants' preferences for low-effort activities were stronger when they were *physically* fatigued compared to a control condition, which implies that physical fatigue made people more sensitive to the perceived costs of future effort. Together, these studies seem to point at the importance of effort-costs for future physical tasks when investigating the impact of fatigue on subsequent physical behavior.

However, some elements of previous studies prohibit definite conclusions about the role of effort-based decision making and changes in cost-benefit analyses in a fatigued state. To date, the assessment of effort-based decisions has been examined for a single choice for physical activity (Brown and Bray, 2019; Harris and Bray, 2019, 2021), which does not enable researchers to assess the avoidance of specific effort costs when fatigued. Moreover, cost-benefit scores were obtained using self-report scales (i.e., Harris and Bray, 2019, 2021). Most studies thus missed the opportunity to assess cost-benefit trade-offs without being limited by participants' ability and willingness to express their motivation accurately (Chong et al., 2016). An exception comes from Iodice et al. (2017), who employed an actual effort-based decision paradigm in which physical effort was operationalized as task duration. However, in this case, the researchers exclusively focused on the impact of *physical* fatigue on the decisions for physical effort. Thus, it remains unclear what the consequences of cognitive fatigue are for physical effort-based decision-making.

Therefore, we aimed to investigate the impact of cognitive fatigue and physical fatigue on the subsequent decision-making process for exerting physical effort. Note that although we were primarily interested in the effects of cognitive fatigue on decision-making for exerting physical effort, we also included a condition meant to influence physical fatigue to validate the effort-based decision task, and to compare cognitive with physical fatigue. We examined effort-based decision making with a consequential choice task in which participants needed to indicate whether they accepted offers to exert a certain amount of effort for a certain reward. Similar procedures have been extensively tested in animal and human subjects, and such effort-based decisions are considered a valid way to assess motivation to exert effort (for overviews, see Chong et al., 2016; Pessiglione et al., 2018). Furthermore, such effort-based decision-making tasks are interesting to use in the domain of fatigue (Massar et al., 2018), because they allow for repeated consequential decisions within individuals that are not contaminated by actually performing the effortful behaviors (e.g., by informing participants that they will be asked to execute a selection of their decisions after the decision task has ended; Bonnelle et al., 2015; Iodice et al., 2017; Le Heron et al., 2018).

We expected that increments in physical effort requirements of offers would reduce individuals' probability to accept offers (i.e., main effect of effort requirement; **hypothesis 1**; Hull, 1943). Moreover, we hypothesized that experimentally manipulated cognitive fatigue would negatively influence individuals' probability to accept offers, independent of the effort requirements (main effect of cognitive fatigue condition; **hypothesis 2a**; Martin et al., 2018; Müller and Apps, 2019). Similarly, we expected that also physical fatigue would negatively affect participants' probability to accept physically effortful offers (main effect of physical fatigue condition; **hypothesis 2b**; Müller and Apps, 2019). Most important, we expected an interaction between fatigue condition and effort requirement such that the negative effect of effort requirements on the probability to accept offers would be stronger in the cognitive fatigue condition (**hypothesis 3a**; Martin et al., 2018; Müller and Apps, 2019) and the physical fatigue condition (**hypothesis 3b**; Iodice et al., 2017) compared to the control condition[1].

MATERIALS AND METHODS

Participants

University students were recruited through the research participation system of Radboud University. Eligibility criteria for participation included to be 18–25 years old, having Dutch, English or German as mother tongue, and having at least moderate understanding of the English language. For practical reasons, we preregistered to test a convenience sample within a specific timeframe (November 4th, 2019 until January 1st, 2020; for preregistration, see https://osf.io/zp7te/). Twenty university students participated in our study within this period, of which 17 provided full data (i.e., three sessions, see Procedure and Materials for details), 2 provided data for the first two sessions and one participant provided data for the first session only. This sample size was identical to that of Iodice et al. (2017), who investigated similar effects. The sample consisted of 17 women and 3 men ($M_{\text{age}} = 20$, range: 18–25) and were either German ($n = 14$), Dutch ($n = 5$), or English ($n = 1$). Participants were instructed to refrain from drinking alcohol in the 24 h before testing and from caffeine consumption on testing days. Written consent was obtained from all participants and participation was rewarded with course credits and a performance-dependent lottery in which participants could win a Fitbit.

Procedure and Materials

The experiment had a counterbalanced[2] within-subject design which consisted of three lab visits on three separate days, with a recovery period of at least 48 h between each visit. **Figure 1A** provides an overview of the procedure. On each day, participants' maximum voluntary contraction (MVC) was determined by squeezing in a tailor-made dynamometer three times for 5 s, as hard as they could. Next, participants underwent one of the three experimental conditions for 45 min: cognitive fatigue, physical fatigue or the control condition. Before and after each manipulation, subjective cognitive fatigue and physical fatigue were assessed. Following the manipulations, participants performed a familiarization session in which they experienced different physical effort levels. After several practice trials, they performed an effort-based decision-making task in which they were required, on a trial-by-trial basis, to decide whether they would perform a particular physical effort for a particular magnitude of reward (details below). During this assessment, they did not yet perform the effort because the aim of the effort-based decision-making assessment was to obtain an index of their decisions uncontaminated by physical exertion during the task. To ensure valid decisions, the decision task was made consequential. That is, participants actually performed a representative selection of 40% of their choices after the decision task (i.e., all 25 unique combinations of reward and effort were selected randomly twice) and were informed about this procedure before the decision-making task. After completing all three sessions, participants were debriefed and reimbursed. This procedure has been reviewed and approved by the ethics committee of Radboud University (ECSW-2019-118) and the hypotheses and analyses were preregistered before data collection on the Open Science Framework (for preregistration, see https://osf.io/zp7te/).

Fatigue Manipulations

To induce *cognitive fatigue*, participants performed the 2-back working memory paradigm (Kirchner, 1958) for 45 min (see **Figure 1B**). During the task, individual letters appeared on the computer screen and participants had to indicate whether the current letter was the same as the letter two trials before (i.e., 2-back). Each letter was presented for 500 ms, followed by an inter-trial-interval of 2,500 ms. Before the next letter appeared, participants should respond by pressing either "Z" (2-back) or "M" (no 2-back) on a qwerty-keyboard. The task consisted of 880 trials with target letters being present on 25% of the trials. All letters ("B," "C," "D," "E," "G," "J," "P," "T," "V," and "W") were presented in capitalized white, Times New Roman against a black background. Before the actual task started, participants received instructions on screen and performed a brief practice session consisting of 32 trials. The 2-back task has been used to induce cognitive fatigue in previous research (e.g., Hopstaken et al., 2015, 2016).

Physical fatigue was induced with an intermittent hand-grip exercise (Hilty et al., 2011) of 45 min (see **Figure 1C**). Similar to the procedures of Iodice et al. (2017), the task that was used to induce physical fatigue thus strongly resembled the physical effort about which participants made effort-based decisions (see below). Participants were instructed to squeeze the dynamometer for 13 s while delivering a required level of grip force as indicated by an interactive computer display and then waited for 5 s until the next trial started. On the first trial, the required grip force was set to 30% of each participants MVC and increased by 10% after two consecutive successes, or decreased by 10% after two consecutive failures. This way, the task was physically fatiguing for all participants while it also ensured that participants could deliver the required force levels throughout the task (i.e., with increasing

[1]As we were primarily interested in the willingness to exert effort as a function of effort requirements, we did not formulate expectations about rewards. We did vary the reward levels in order to create incentive compatible offers without evoking heuristic responses (e.g., only accepting low-effort offers). Exploratory reward-analyses can be found in **Supplementary Material**.

[2]For three participants, the actual order of sessions deviated from the original counterbalanced scheme.

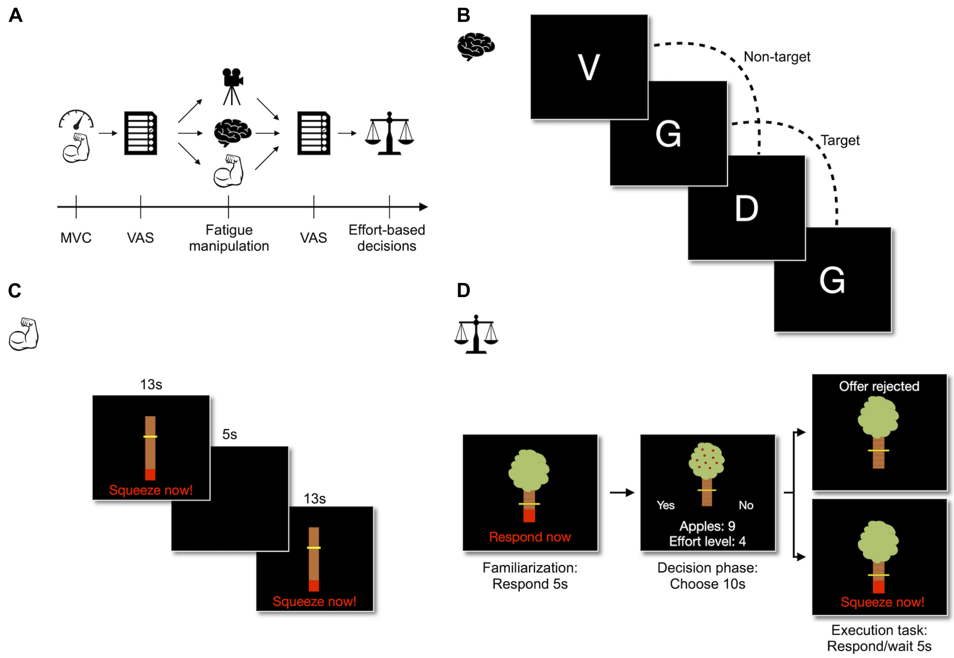

FIGURE 1 | Experimental and task procedures. MVC, Maximum Voluntary Contraction; VAS, Visual Analog Scales measuring cognitive fatigue and physical fatigue. **(A)** Participants visited the lab on three occasions. In each visit, participants underwent one of the three fatigue manipulations (i.e., documentary watching, 2-back task or handgrip exercise) before and after which subjective cognitive and physical fatigue were assessed on a single-item VAS-scale. At the end of each visit, participants performed the effort-based decision-making task. **(B)** In the 2-back task, participants responded to letters appearing on the computer screen. Participants indicated whether the presented letter was the same as two letters before by pressing the corresponding key on a keyboard ("Z" = target, "M" = non-target). **(C)** In the 45-min handgrip exercise, participants squeezed at the required force level for 13 s after which they had a short break of 5 s. **(D)** The effort-based decision-making task consisted of three subtasks. In the familiarization task, participants squeezed at the different force levels (16–80% of MVC) for 5 s. In the decision-making task, participants indicated whether they accepted the offer by pressing the left or right arrow key on the keyboard (position of "yes" and "no" varied per trial). In the execution task, participants performed 40% of the choices made in the decision-making trials. They either squeezed at the required force level for 5 s to obtain the reward of accepted offers or waited for 5 s on the rejected offers. On all squeezing tasks (handgrip exercise, familiarization task and execution task), the yellow bar represented the required force level while the red filling indicated the force participants were currently delivering. Panel D: Adapted from Le Heron et al. (2018). CC BY 4.0.

muscle fatigue). In total, the task consisted of 120 trials, divided over three blocks that were separated by a 45-min break.

In the *control condition*, participants watched the 45-min documentary "Planet Earth—From Pole to Pole" (Fothergill et al., 2006). Documentary watching is frequently used as a control condition in fatigue research (e.g., Marcora et al., 2009; Radstaak et al., 2011; Van Cutsem et al., 2017a) and this specific documentary was chosen as it could be presented in each participant's mother tongue (i.e., Dutch, German or English). To stimulate engagement, participants were informed that after watching the documentary, they would be asked to indicate for several screenshots whether it was taken from the documentary or not.

Directly before and after each manipulation, participants reported their subjective cognitive and physical fatigue on two single-item VAS-scales (ranging from "Not at all" to "Extremely": "How mentally/physically fatigued do you currently feel?"). To enhance task motivation, participants were informed they took part in a lottery for winning a Fitbit and that their chances of winning depended on task performance on each of the three manipulation tasks. Specifically, with each successful trial or response, participants increased their chances of winning the Fitbit. Participants did not receive performance feedback during or after the experimental sessions to prevent consequences of (perceived) good or bad performance. This specific procedure was selected to ensure that each trial of each experimental condition was considered equally important for winning the Fitbit.

Effort-Based Decision-Making

To quantify participants' decision-making for physical effort, an adapted version of the accept/reject paradigm (Bonnelle et al., 2015; Le Heron et al., 2018) was used (see **Figure 1D**). In this task, participants repeatedly chose to accept or reject offers consisting of varying levels of rewards and physical effort. For each offer, participants decided whether they were willing to invest the required level of physical effort to obtain the reward. These offers were visually presented as an apple tree, with a yellow bar on the tree trunk representing the physical effort level (16, 32, 48, 64, or 80% of the participant's MVC), and the number of apples hanging in the tree representing the rewards (1, 3, 6, 9, or 12 apples). The task consisted of 25 unique offers (i.e., 5 effort levels × 5 reward levels) and each unique offer was presented 5 times, resulting in 125 trials that were divided over 5 blocks. These trials were presented to participants in the exact same random order to prevent between-participant and between-session differences in trial order affecting choice behavior. On each trial, participants had 10 s to indicate whether they accepted the offer by pressing either the left or right arrow-key (key definition varied on a trial-by-trial basis to prevent response biases). Participants also learned that the amount of money they could earn with their decisions depended upon the number of apples they would collect by exerting the trial-based amount of effort during a subsequent execution task in which they would receive a selection of their decisions. Furthermore, they were only informed about the exact value of apples when they received their earnings after completing all test sessions (Bonnelle et al., 2015; Le Heron et al., 2018). This was done to control possible individual differences in weighing of the absolute reward value.

Right before the decision-making task, participants performed a familiarization session in which they actually experienced the five levels of physical effort by squeezing the dynamometer at the required force levels twice. They also completed a practice block of 18 trials to get used to the decision-making procedure. Following the decision-making task, participants performed the execution task, which consisted of 50 trials that were drawn from the decision-making task (40% of trials). Specifically, each unique combination of effort and reward was selected twice, once from the first and once from the second half of the decision-making task. Depending on the choices made during the decision-making task, they could either squeeze the dynamometer for 5 s at the required force level to obtain the apples (i.e., accepted offers), or wait for 5 s until the next trial started (i.e., rejected offers). During this execution phase, participants could not change the choices made earlier during the decision task (e.g., squeezing on a previously rejected trial did not lead to any reward). The execution phase thus only served to increase validity of the decision-making task and these execution data were not analyzed for answering the research question. Each gathered apple represented 1/3 eurocent, which meant that participants could earn up to €3 extra in total.

Analysis

Data were first screened for invalid trials on which participants gave an erroneous response (i.e., a different key than the response keys) or no response at all. All analyses were performed in the statistical programming software R (R Core Team, 2020). In line with our preregistration, all hypotheses were tested using (generalized) linear mixed-effects models [(G)LMM] with the (g)lmer function (lme4 package; version 1.1-23; Bates et al., 2015). Following the advice of Barr et al. (2013), we used a maximal random effects structure to prevent inflation of Type I errors. Robust p-values were obtained with Type III bootstrapped Likelihood Ratio tests using the "mixed" function (afex package; version 0.27-2; Singmann et al., 2015). *Post hoc* tests were performed with the "emmeans" function (emmeans package; version 1.4.4; Lenth, 2020) or by re-testing the GLMM within the conditions of interest. Zero-sum coding was used for all factorial predictors.

Manipulation Checks

To investigate whether the manipulations had their intended effects, we ran an LMM testing whether participants experienced the cognitive fatigue condition to be more cognitively fatiguing than the physical fatigue condition and the control condition[3]. The model included a fixed intercept and fixed effects for condition (cognitive fatigue, physical fatigue, control), time (pre, post) and the interaction term Condition × Time. In addition, the model included a per-participant random adjustment to the fixed intercept (i.e., "random intercept") as well as to the fixed slope of time (i.e., "random slope"). *Post hoc* analyses were performed to investigate which conditions differed from one another.

Second, we investigated whether participants experienced the physical fatigue condition to be more physically fatiguing than the cognitive fatigue condition and the control condition. The model included a fixed intercept and fixed effects for condition (cognitive fatigue, physical fatigue, control), time (pre, post) and the interaction term Condition × Time. In addition, the model included a per-participant random adjustment to the fixed intercept as well as to the fixed slopes of cognitive fatigue and time. *Post hoc* analyses were performed to compare the specific experimental conditions.

Finally, two exploratory analyses (i.e., not preregistered) were performed to obtain insight into participants' performance on the 2-back task. These analyses and outcomes can be found in **Supplementary Material**.

Main Analyses

To investigate to what extent the probability to accept offers during the decision-making task was influenced by the effort requirements (hypothesis 1), the fatigue manipulations (hypothesis 2a and 2b), or the interaction between the fatigue manipulations and the physical effort requirements (i.e., physical effort slope per condition; hypothesis 3a and 3b), we ran a GLMM[4]. The model included a fixed intercept and fixed slopes for the within-subject factors condition (cognitive fatigue, physical fatigue, control), physical effort requirement (continuous) and the interaction term Condition × Physical Effort Requirement[5]. In addition, a per-participant random adjustment to the fixed intercept as well as per-participant

[3]We preregistered to only compare increases in subjective cognitive fatigue between the cognitive fatigue condition and the control condition to conserve statistical power. Also, for physical fatigue, we preregistered to only compare increases in subjective physical fatigue between the physical fatigue condition and the control condition. All other *post hoc* and omnibus comparisons were performed for completeness but these analyses should thus be considered exploratory.

[4]Only data of the decision-making task were analyzed as the potential responses (i.e., squeeze or wait) within the actual execution task depended on the choices made within the decision-making task.

[5]We preregistered to analyze our data separately in order to conserve statistical power for the effects that we were specifically interested in (i.e., the contrast between the two fatigue conditions and the control condition). The present study was meant to be a pilot study in a convenience sample (i.e., determined by a predefined time limit). As we could not predict the exact number of participants that would take part in our study within this time frame, we were conservative in our tests and restricted ourselves to analyses that exclusively tapped into the contrasts of interest (i.e., experimental conditions against control condition). Including all conditions in one analysis would result in more complex interactions, with more effects to estimate and resulting in a reduced statistical power. The

random adjustments to the fixed effects were included in the model. *Post hoc* analyses were performed to investigate which specific levels of effort and fatigue differed from one another.

RESULTS

Data-screening revealed that of the 6,750 decision-making trials, only 10 were invalid because participants did not respond ($n = 5$) or pressed an invalid key ($n = 5$). These trials were excluded from further analyses.

Manipulation Checks

To test whether the fatigue manipulations evoked (domain-specific) subjective fatigue, two manipulation checks were performed. For an overview of all self-reported states before and after each experimental condition (see **Table 1**).

Subjective Cognitive Fatigue

In the first manipulation check, we compared the increases in self-reported cognitive fatigue between the cognitive fatigue, physical fatigue, and control condition. The main effect of condition was significant [$\chi^2(2) = 21.763$, $p = 0.001$] as well as the main effect of time [$\chi^2(1) = 13.817$, $p = 0.001$]. Crucially, also the interaction term Condition x Time was significant [$\chi^2(2) = 11.612$, $p = 0.003$], indicating that the increase in self-reported cognitive fatigue differed between the three conditions. Our confirmatory *post hoc* analysis compared the increases between the specific experimental conditions. This analysis revealed that subjective cognitive fatigue increased significantly more in the cognitive fatigue condition than in the control condition [$b = 8.12$, $SE = 2.02$, $t(39.99) = 4.03$, $p < 0.001$]. Interestingly, exploratory *post hoc* analyses revealed that the increase of cognitive fatigue was not significantly stronger in the cognitive fatigue condition than in the physical fatigue condition ($p = 0.141$) and that the increase in cognitive fatigue was significantly stronger in the physical fatigue condition in comparison to the control condition [$b = 4.86$, $SE = 2.34$, $t(40) = 2.08$, $p = 0.045$]. Another exploratory *post hoc* analysis revealed that subjective cognitive fatigue increased significantly in the cognitive fatigue condition [$b = -29.78$, $SE = 6.76$, $t(95) = -4.405$, $p < 0.001$] but not in the physical fatigue condition ($p = 0.137$) or in the control condition ($p = 0.999$). Finally, we explored the between-condition differences in self-reported fatigue before and after the experimental manipulations. This exploratory analysis revealed that before the manipulations, subjective cognitive fatigue was not significantly different between the three conditions (p's > 0.05). After the manipulations, subjective cognitive fatigue was significantly higher in the cognitive fatigue condition in comparison to the control condition ($b = -39.19$, $SE = 6.76$, $t = -5.796$, $p < 0.001$) but not in comparison to the physical fatigue condition ($p = 0.134$). Moreover, subjective cognitive fatigue was also significantly higher in the physical fatigue condition compared to the control condition ($b = -22.27$, $SE = 6.76$, $t = -3.294$, $p = 0.017$). These analyses provide partial support for the success of the cognitive fatigue manipulation. Within the conditions, subjective cognitive fatigue increased in the cognitive fatigue condition, and not in the other conditions. However, the increase and level of subjective cognitive fatigue did not significantly differ between the cognitive fatigue and physical fatigue condition.

Subjective Physical Fatigue

In the second manipulation check, we tested whether self-reported physical fatigue increased more in the physical fatigue condition than in the cognitive fatigue and control condition. The main effect of condition was significant [$\chi^2(2) = 10.347$, $p = 0.012$] as well as the main effect of time [$\chi^2(1) = 10.764$, $p = 0.002$]. Crucially, also the interaction term Condition x Time was significant [$\chi^2(2) = 7.818$, $p = 0.024$], meaning that the increase in subjective physical fatigue differed between the three conditions. The confirmatory *post hoc* analysis revealed that subjective physical fatigue increased significantly more in the physical fatigue condition than in the control condition [$b = 6.22$, $SE = 2.17$, $t(60) = 2.86$, $p = 0.006$]. Surprisingly, exploratory analyses showed that the increase in subjective physical fatigue was not significantly stronger in the physical fatigue condition than in the cognitive fatigue condition ($p = 0.337$) and increased significantly more in the cognitive fatigue condition in comparison to the control condition [$b = 4.24$, $SE = 1.96$, $t(40) = 2.16$, $p = 0.037$]. Another exploratory analysis revealed that subjective physical fatigue increased in the physical fatigue condition [$b = -23.19$, $SE = 6.46$, $t(95) = -3.56$, $p = 0.007$] but not in the other two conditions (p's > 0.05). Finally, we explored the between-condition differences in subjective physical fatigue before and after the experimental manipulations. These exploratory analyses revealed that before the experimental manipulations, subjective physical fatigue did not significantly differ between the three conditions (p's > 0.05). However, after the manipulations, subjective physical fatigue was significantly higher in the physical fatigue condition compared to the control condition [$b = -27.13$, $SE = 6.46$, $t(95) = -4.20$, $p < 0.001$] but not in comparison to the cognitive fatigue condition ($p = 0.508$). Subjective physical fatigue was also not significantly higher in the cognitive fatigue condition than in the control condition ($p = 0.148$). These results again partially support the success of the manipulation. Subjective physical fatigue increased in the physical fatigue condition, but not in the other conditions. However, the increase and level of subjective physical fatigue did

omnibus test was performed for clarity and yields the same results as our preregistered *post hoc* analyses.

TABLE 1 | Means and standard deviations of self-reported fatigue per condition and per measurement.

	Cognitive fatigue (0–100)		Physical fatigue (0–100)	
Condition	**Pre**	**Post**	**Pre**	**Post**
Cognitive fatigue	44.36 (23.07)	74.15 (22.39)	37.90 (23.25)	53.16 (26.74)
Physical fatigue	40.39 (29.85)	57.23 (29.96)	41.23 (26.71)	64.42 (30.01)
Control	37.66 (26.74)	34.96 (23.93)	38.96 (27.49)	37.28 (26.81)

N = 20. Standard deviations are presented in parentheses.

not significantly differ between the physical fatigue or cognitive fatigue condition.

From these manipulation checks, it follows that the fatigue manipulations were partially effective at inducing subjective fatigue. Significant increases in subjective fatigue were observed within the relevant conditions but the increases and post-measures did not significantly differ between the cognitive fatigue and physical fatigue conditions. Assuming that the single-item VAS-scales are valid, it is thus debatable whether we can test the impact of domain-specific fatigue on the decision to exert physical effort. To further evaluate the validity of the experimental tasks, we additionally looked into the (domain-specific) effort, frustration, boredom and stress participants reported (before and) after the experimental manipulations. Descriptive data of these experiences can be found in **Supplementary Tables 1, 2**. While we cannot be completely certain that the fatigue manipulations evoked the appropriate fatigue experiences, these data suggest that our manipulations did evoke the appropriate domain-specific demand experiences. As such, our findings will at the very least inform us about the impact of exerting (physical or cognitive) effort on subsequent physical effort-based decision-making.

Main Analyses

In the main analysis, we tested whether the probability to accept physically effortful offers in the decision-making task was influenced by the physical effort requirements of offers, by the fatigue conditions and the interaction term Condition × Physical Effort Requirement. As expected, and confirming hypothesis 1, the analysis showed a significant effect of the physical effort requirements [$\chi^2(1) = 37.536$, $p = 0.001$]. *Post hoc* comparisons revealed that with each increase in physical effort requirement, the probability to accept offers was significantly lower (all *p*'s < 0.001). This replicates the longstanding law of least effort (Hull, 1943) which states that individuals tend to avoid effort when possible. Unexpectedly, no significant effect of condition was found ($p = 0.253$). Participants were not significantly less likely to accept offers after the cognitively or physically demanding task. Hypothesis 2 was thus rejected.

Most important, the interaction term Condition x Physical Effort Requirement significantly predicted the probability to accept physically effortful offers [$\chi^2(2) = 0.017$, $p = 0.028$]. In line with the prediction (hypothesis 3b), *post hoc* analyses as well as visual inspection of the interaction-effect (see **Figure 2**) revealed that the effort slope was significantly steeper in the physical fatigue condition than in the control condition (OR = 0.369, 95% CI [0.166, 0.737], $p = 0.012$). However, and against hypothesis 3a, the effort slope did not significantly differ between the cognitive fatigue condition and the control condition ($p = 0.328$). In fact, an exploratory analysis revealed that the effort slope was significantly steeper in the physical fatigue condition than in the cognitive fatigue condition (OR = 0.266, 95% CI [0.093, 0.663], $p = 0.008$). Between-condition comparisons at the specific levels of physical effort requirement did not reach significance (*p*'s > 0.05).

See **Supplementary Material** for additional data and analyses on reward sensitivity and performance on the execution task.

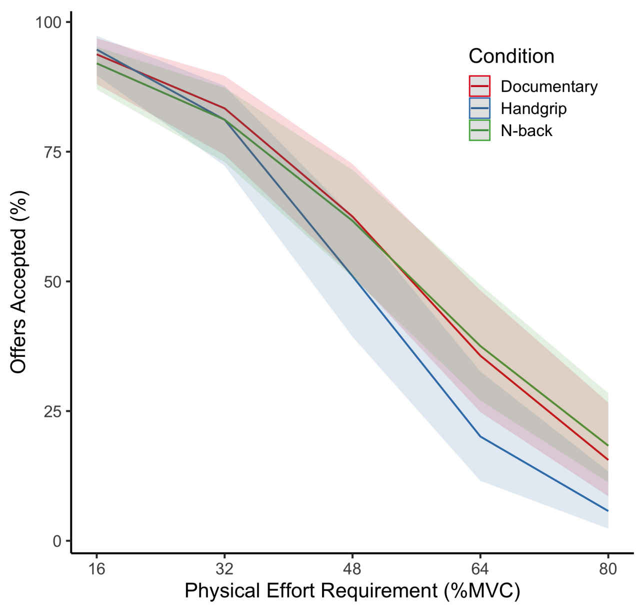

FIGURE 2 | Effects of physical effort requirements and condition on percentage of offers accepted. *N* = 20. MVC, Maximum Voluntary Contraction. Shaded areas represent 95% confidence intervals. The overall percentage of effortful offers accepted did not differ significantly between the experimental conditions. However, the effort slope was significantly steeper after performing the handgrip exercise than after documentary watching or after performing the N-back task. Between-condition comparisons at the specific levels of physical effort requirement did not reach significance.

DISCUSSION

We aimed to investigate the impact of cognitive and physical fatigue on the decision-making process for exerting physical effort. We expected that cognitive and physical fatigue would increase the weight of effort costs within the cost-benefit analysis for the decision to exert physical effort. Despite thorough attempts to specifically manipulate cognitive and physical fatigue, the evidence for effective domain-specific fatigue manipulations was weak. While participants reported stronger increases in cognitive and physical fatigue in the fatiguing conditions than in the control condition, these increases did not differ between the two fatiguing conditions. In the remainder of this discussion, we will therefore be conservative and make no claims about the consequences of any specific forms of fatigue but rather focus on the impact of cognitive or physical effort exertion on subsequent effort-based decision-making.

The present experiment shows that people are sensitive to effort requirements, which validates the effort-based decision task. More important, exerting cognitive effort did not reduce the likelihood to accept physically effortful offers or strengthen the negative effort slope. This pattern of findings does not support Müller and Apps's (2019) suggestion that prior effort exertion has domain-general effects on subsequent effort-based decision-making. Specifically, Müller and Apps (2019) argue that individuals would assign more weight to effort-costs after exerting effort, irrespective of the effort domain (i.e., physical or cognitive). If that were the case, performing a cognitively demanding task would increase the physical effort slope. The current study was the first to directly test this assumption and our findings do not support such a domain-general impact. Possibly, the cognitively demanding task was not sufficiently demanding to evoke domain-general effects on effort-based decision-making. Multiple studies have shown that the fatiguing effects of exerting cognitive effort depend on task difficulty rather than on time-on-task (Boksem and Tops, 2008; Chatain et al., 2019). While the 2-back task draws on multiple cognitive capacities such as working memory processing, updating and vigilance, the fixed task characteristics might not have been sufficiently demanding for our university sample to elucidate an impact on effort-based decision-making. Higher cognitive demands might be needed to evoke domain-general effects of cognitive effort exertion. A promising approach would be to adapt task difficulty to participant performance on a trial-by-trial basis, which has been shown to effectively evoke the phenomenology of cognitive effort (Lin et al., 2020). Establishing strong cognitive effort manipulations will be crucial to understand the impact of cognitive effort exertion on subsequent (physical) effort-based decision-making.

Interestingly, our results show that while physical effort exertion does not reduce the overall likelihood to accept physically effortful offers, it strengthens the effort slope. In line with findings of Iodice et al. (2017), participants weighted physical effort costs more heavily in the effort-based decision-making task after a demanding physical task. Importantly, we show that this effect not only applies to decisions about cycling duration but also about delivering physical force (i.e., squeezing). An important asset of the present study is that variation in effort levels of the high-effort options was not contaminated by the duration of physical effort requirements. While Iodice et al. (2017) manipulated effort levels by varying the duration of the high-effort options (i.e., 10–40 min of cycling), in the present study, exclusively the physical force requirements of high-effort options varied (i.e., 16–80% of participants' MVC). As such, our study provides new evidence for the impact of physical effort exertion on the weighting of future physical effort-costs. It shows that physical effort-based decision-making is not fixed but depends on earlier bouts of physical effort exertion.

These findings provide interesting methodological and theoretical insights. Importantly, this was the first study to test the consequences of both cognitive and physical effort exertion for subsequent effort-based decision-making within a single study. Our findings show that physical effort decisions are sensitive to earlier bouts of effort exertion (i.e., a steeper effort slope after exerting physical effort). Crucially, the effect of physical effort exertion was larger than that of cognitive effort exertion, which did not significantly differ from the control condition. Our ability to show these differential consequences of effort exertion within the same experimental design is informative, even though these findings may be attributed to different reasons: It is possible that the predictions made in the neurocognitive framework of motivational fatigue (Müller and Apps, 2019) are incorrect and that cognitive fatigue does not affect the weight assigned to future effort-costs. If that is the case, an alternative explanation for lower levels of physical activity participation after cognitive exertion might be that the perception of effort increases as suggested by psychobiological models of endurance performance (e.g., Marcora, 2010; Pageaux, 2014; Martin et al., 2018), rather than the weight of effort-costs. Alternatively, the absence of evidence for a change in effort-costs could be ascribed to the methodological constraints of this study such as the relatively small sample size, the absence of clear domain-specific fatigue experiences and the very restricted form of physical effort exertion (i.e., hand squeezing), or the intensity of the cognitive task. Therefore, more research with larger samples, alternative effortful tasks and thorough manipulation checks will be needed to determine which mechanism explains the deterioration of physical performance after cognitive effort (Brown et al., 2020). The current study provides a strong methodological basis which can be drawn upon by future research to further investigate these processes.

At the same time, our findings are the first to provide direct support for Müller and Apps's (2019) proposition that exerting physical effort increases the weight assigned to future effort-costs. That is, the negative impact of effort requirements on the likelihood to accept effortful offers (i.e., the effort slope) increased after earlier physical effort exertion. This finding provides a nuanced image of the consequences of physical effort exertion for the decision-making process with regard to subsequent effort. Individuals are not unwilling to exert any effort after earlier bouts of effort exertion (Thorndike, 1914) but they weight the effort-costs more strongly in the cost-benefit analyses underlying the decision to exert further effort or not. This could very well explain why, after performing an effortful task, individuals tend to perform worse on a subsequent effortful task, unless they are motivated to perform well by additional rewards (Boksem et al., 2006; Hopstaken et al., 2015, 2016). From the current perspective, this makes perfect sense. If the perceived costs of effort increase after a period of effort exertion while at the same time, additional rewards outweigh these increased costs, individuals will still engage in a subsequent effortful task. Here too, it will be valuable to investigate whether the observed changes in effort-based decision-making occur due to a change in the weight assigned to effort-costs (Müller and Apps, 2019), or due to a change in the perception of effort (Marcora, 2010; Pageaux, 2014; Martin et al., 2018).

With regard to the manipulation checks, it is important to note that the findings do not seem to align with the definition of cognitive fatigue as a state resulting from prolonged engagement in a cognitively demanding task (Boksem and Tops, 2008; Kanfer, 2011; van der Linden, 2011). If that were true, then the cognitively demanding task would have evoked more subjective cognitive fatigue on the single-item VAS-scale than the physically demanding task. The small sample size might account for the fact that the differences between the fatiguing conditions (see **Table 1**) failed to reach statistical significance (Button et al., 2013). Another reason for the absence of domain-specific fatigue differences might be that the specific type and level of cognitive and physical demands that the experimental tasks required (i.e., working-memory and local muscle performance) did not evoke the intended fatigue experiences. Crucially, the current manipulation issues also tap into the ongoing challenge to scientifically define fatigue and its (experimental) antecedents (van der Linden, 2011; Hockey, 2013). The multifaceted nature of fatigue (i.e., behavior, emotion, motivation, and information processing) makes it very hard to pinpoint the exact nature of fatigue (van der Linden, 2011). As outlined by Müller and Apps (2019), the same phenomenological experience of fatigue can occur after exerting effort into very different domains, which can make it very hard for people to differentiate between physical and cognitive fatigue on a single-item VAS-scale. Against this background, it is less surprising that our domain-specific fatigue manipulations did not result in convincing domain-specific differences in self-reported fatigue.

An important strength of the current study is its innovative and theory-driven design, which enabled us to test some of the core assumptions of dominant fatigue theories. Specifically, this was the first study in which the decision-making process for physical effort was assessed after bouts of both cognitive and physical effort exertion, which enabled us to compare the consequences of these specific forms of effort exertion. An interesting venue for future research will be to measure cognitive effort-based decision-making in addition to physical effort-based decision-making. While the present study provides a first glimpse into the domain-specificity of effort exertion on subsequent decision-making, adding cognitive effort-based decision-making will allow researchers to test the full range of domain specific and -general effects (i.e., all possible combinations within and between the physical and cognitive domain). A noteworthy example here is a study performed by Chong et al. (2018), in which the researchers used a single task to measure both cognitive- and physical effort-based decision-making. Similar tasks could very well be applied to further disentangle the effects of effort exertion, which can improve our understanding of both the antecedents and consequences of effortful behavior.

Despite its strong design, several important limitations should be stressed. As outlined before, an important limitation of this study is the absence of clear domain-specific fatigue manipulations. Subjective cognitive fatigue did not increase significantly more in the cognitive fatigue condition than in the physical fatigue condition and vice versa. This makes it impossible to draw definite conclusions about the impact of cognitive and physical fatigue on subsequent effort-based decision-making. Regarding the manipulation of cognitive fatigue, it will be valuable to select prolonged tasks that are more cognitively demanding (Pageaux and Lepers, 2018). While the n-back task has been used to induce cognitive fatigue before (Hopstaken et al., 2015, 2016), it might be more effective to use tasks that require other cognitive processes, such as inhibition (Smith et al., 2019). Moreover, researchers could select tasks that adapt to participants' dispositional and situational cognitive capacities (for examples, see Lin et al., 2020; O'Keeffe et al., 2020), to ascertain that the task is similarly demanding for each participant. In a similar vein, it will be valuable to apply alternative tasks to induce physical fatigue in future research. We currently applied a task specifically requiring forearm muscle contraction and it might be valuable to use tasks requiring larger muscle mass (e.g., quadricep muscle contraction) or whole-body exercises (e.g., running or cycling) to induce physical fatigue. Another limitation was that we did not assess the perception of physical effort. Accumulating evidence suggests that the perception of effort plays a crucial role in explaining physical performance after cognitive effort exertion (Pageaux and Lepers, 2016, 2018; Brown et al., 2020) as well as in motor control and decision making in general (Cos, 2017; Wang et al., 2021). While participants in the present study did experience the different physical effort levels immediately after each manipulation task (i.e., within the familiarization task), including an assessment of perceived effort will be crucial for future research to obtain insight into its explanatory role for effort choices. Finally, the relatively small and homogeneous sample limits the generalizability of our conclusions. Twenty university students participated in our study and it would be interesting to see whether the current findings apply beyond this specific sample of university students. For example, it is possible that different effects would be observed in older individuals since both cognitive and physical capacities tend to deteriorate with age, which might affect the cost-benefit trade-offs people make. Moreover, inclusion of larger samples is recommended in future research. The current sample size resembled that of Iodice et al. (2017), who conducted a very similar study. However, larger samples will lead to more reliable estimates of effect sizes and increase the chances to obtain reproducible findings (Button et al., 2013).

To conclude, the present study advances our insight into the psychological mechanisms that underlie engagement in effortful behavior. While it was not possible to investigate the unique impact of cognitive and physical fatigue on subsequent physical effort-based decision-making, our study reveals very detailed consequences of effort exertion for subsequent effort decisions. Our findings confirm that individuals perceive physical effort to be costly and our results imply that individuals ascribe more weight to these physical effort-costs after prolonged exertion of physical but not cognitive effort. Individuals are thus not simply less likely to accept physically effortful offers after earlier bouts of effort exertion but this effect seems to depend upon the type of previous effort exertion as well as the specific effort levels of offers. Taking this specificity into account will help researchers to further improve our understanding of the psychology of physical activity, which could eventually contribute to the effectiveness of global physical activity promotion.

DATA AVAILABILITY STATEMENT

The datasets presented in this study can be found in online repositories. The names of the repository/repositories and accession number(s) can be found below: https://osf.io/zp7te/. This article has received the badges for Open Data, Open Materials, and Preregistration. More information about the Open Practices badges can be found at http://www.psychologicalscience.org/publications/badges.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by the Ethics Committee Social Sciences of Radboud University. The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

SA, DB, HV, MH, and SG designed the research. SA performed the research and analyzed the data. SA, DB, HV, MK, MH, and SG wrote the manuscript. All authors contributed to the article and approved the submitted version.

SUPPLEMENTARY MATERIAL

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fpsyg.2021.645037/full#supplementary-material

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Validation of Discrete and Regression-Based Performance and Cognitive Fatigability Normative Data for the Paced Auditory Serial Addition Test in Multiple Sclerosis

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Cognitive fatigability is an objective performance decrement that occurs over time during a task requiring sustained cognitive effort. Although cognitive fatigability is a common and debilitating symptom in multiple sclerosis (MS), there is currently no standard for its quantification. The objective of this study was to validate the Paced Auditory Serial Addition Test (PASAT) discrete and regression-based normative data for quantifying performance and cognitive fatigability in an Ontario-based sample of individuals with MS. Healthy controls and individuals with MS completed the 3″ and 2″ versions of the PASAT. PASAT performance was measured with total correct, dyad, and percent dyad scores. Cognitive fatigability scores were calculated by comparing performance on the first half (or third) of the task to the last half (or third). The results revealed that the 3″ PASAT was sufficient to detect impaired performance and cognitive fatigability in individuals with MS given the increased difficulty of the 2″ version. In addition, using halves or thirds for calculating cognitive fatigability scores were equally effective methods for detecting impairment. Finally, both the discrete and regression-based norms classified a similar proportion of individuals with MS as having impaired performance and cognitive fatigability. These newly validated discrete and regression-based PASAT norms provide a new tool for clinicians to document statistically significant cognitive fatigability in their patients.

Keywords: PASAT, cognitive fatigability, fatigue, normative data, regression-based norms, discrete norms, multiple sclerosis

INTRODUCTION

Cognitive impairment affects up to 70% of individuals with multiple sclerosis (MS), with impaired working memory and information processing speed abilities being fundamental cognitive deficits (Chiaravalloti and DeLuca, 2008; Grzegorski and Losy, 2017). Cognitive impairment arises in MS due to pathophysiological processes that result in lesions in the brain's white and gray matter (DeLuca et al., 2020). Those with primary progressive (PPMS) and secondary progressive (SPMS) disease subtypes demonstrate more pronounced cognitive deficits than those with the relapsing-remitting subtype (RRMS; Eijlers et al., 2018). Furthermore, cognitive impairment tends to increase with disease duration (Amato et al., 2006).

Many individuals with MS report experiencing cognitive fatigue, or a lack of mental energy required for sustained cognitive tasks (Fisk et al., 1994). Cognitive fatigue is a debilitating symptom that can result in difficulties completing tasks at work or school that require sustained cognitive effort. Additionally, cognitive fatigue has a variety of socioeconomic consequences, such as a loss of work hours, unemployment, and early retirement (Smith and Arnett, 2005; Simmons et al., 2010). Despite these functional impairments, there is currently no universally accepted method for measuring cognitive fatigue in the MS literature.

The causes of MS-related cognitive fatigue can be classified into primary and secondary mechanisms (Forwell et al., 2008; Braley and Chervin, 2010). Secondary mechanisms include other symptoms that worsen fatigue, such as depression, mobility inefficiency, respiratory problems, and sleep disorders. The relation between depression and fatigue in MS remains unclear, with several studies reporting little to no correlation between the two symptoms even though they often overlap in MS (Krupp et al., 1988, 1989; Vercoulen et al., 1996). However, other studies have found that depression is a predictor of cognitive fatigue in individuals with MS (Berard et al., 2019b) and that there is a moderate correlation between MS-related cognitive fatigue and depression (Ford et al., 1998). It has also been postulated that reduced sleep quality due to impaired slow wave sleep may contribute to cognitive fatigue in MS (Touzet, 2017).

Primary mechanisms of cognitive fatigue, in contrast, are those that are directly related to the pathogenesis of MS, such as proinflammatory cytokines, endocrine influences, axonal loss, and an altered pattern of cerebral activation (Braley and Chervin, 2010; Linnhoff et al., 2019). In particular, CF is associated with disruptions in circuits involved in attention and arousal, including the basal ganglia, frontal cortex, and thalamus (Chaudhuri and Behan, 2000). Lesions in pathways of the reticular and limbic systems and basal ganglia being particularly implicated in CF (Chaudhuri and Behan, 2004). Functional neuroimaging studies have also demonstrated differences in activation patterns in the attention network between individuals with MS and healthy controls before, during, and after a cognitively fatiguing task (Berard et al., 2019a). In addition to structural disease pathology, pro-inflammatory cytokines have been postulated to play a role in MS-related fatigue. Individuals with MS who subjectively report higher levels of fatigue show higher levels of tumor necrosis factor alpha which was correlated with daytime sleepiness (Heesen et al., 2006).

Traditionally, MS-related cognitive fatigue has been measured subjectively through self-report questionnaires and rating scales, such as the Fatigue Severity Scale (Krupp et al., 1989), the Fatigue Impact Scale (Fisk et al., 1994), the Fatigue Scale for Motor and Cognitive Functions (Penner et al., 2009), and the Wurzburg Fatigue Inventory for MS (Flachenecker et al., 2006). Because fatigue is a multidimensional construct, however, self-report measures vary in what aspect of fatigue they measure (e.g., fatigue severity, duration, momentary perceptions, chronic character, mental or physical dimensions, and/or impact on daily functioning; Beckerman et al., 2020). In addition, self-report measures of fatigue are often prone to recall bias, regression to the mean, and they have been found to correlate weakly with one another (Fiene et al., 2018; Linnhoff et al., 2019). Prior studies have also found that there is no relationship between subjective and objective measurements of cognitive fatigue (Parmenter et al., 2003; Bryant et al., 2004; Bailey et al., 2007). Given the variability of how cognitive fatigue has been measured in the MS literature, Linnhoff et al. (2019) proposed a taxonomy that distinguishes cognitive fatigue from cognitive fatigability (CF). In contrast to cognitive fatigue that is measured subjectively, CF refers to an objectively measured decrease in performance throughout the duration of a sustained cognitive task (Walker et al., 2012).

The Paced Auditory Serial Addition Test (PASAT; Gronwall, 1977; Rao, 1990) is a sensitive measure of working memory and information processing speed that is consistently used in studies examining CF in MS. During the PASAT, participants listen to a series of single digit numbers and must add each number that is heard to the number immediately prior to it. Participants must respond orally before the presentation of the next digit to receive a correct response. The interstimulus interval (ISI) can be varied, with 3- or 2-s (3″ or 2″) ISIs being the most common in the MS literature (Tombaugh, 2006). The PASAT can measure CF when performance is compared between the beginning and end of the task, with decreased performance at the end compared to the beginning of the task being an indication of CF. Prior studies have demonstrated CF in MS by comparing performance between the first and second half of the task (Walker et al., 2012; Berard et al., 2014) and the first and last third of the task (Morrow et al., 2015; Berard et al., 2018, 2019b). In both cases, individuals with MS have demonstrated greater within-task performance decrements compared to healthy controls. Agyemang et al. (2021) investigated how performance declines over time on the PASAT with the same population used in the current study. They found that individuals with MS had more cumulative errors throughout the task than the healthy controls, particularly for the 3″ PASAT. When compared to controls, the MS group had a steeper, linear performance decline from the start of the task rather than their performance breaking down at any specific point during the task. Therefore, the CF experienced by individuals with MS seems to arise from difficulties maintaining an optimal level of performance from the initial onset of a cognitively demanding task.

PASAT scores typically constitute the total number of correct responses for each trial, out of a maximum score of 60. A disadvantage of using total correct scoring is that participants may use a chunking strategy to reduce the working memory demands of the task. Namely, some participants may add two numbers, skip the next number, then add the following two numbers which reduces both the difficulty of the task and its sensitivity at detecting cognitive impairment (Snyder et al., 1993). Dyad and percent dyad scoring methods can be used to better determine whether a participant was performing the task as intended. Total dyad scores are calculated by summing the *number of times* two correct responses occur in succession, while percent dyad scores reflect the *proportion of time* that two correct responses occurred in a row. Thus, dyad and percent dyad scores reflect whether, and for what proportion of time, the participant was meeting the working memory demands of the task.

To evaluate an individual's performance on a neuropsychological test relative to demographically similar healthy individuals, clinicians consult normative data. Discrete norms are derived by dividing data into groups with certain demographic variables (e.g., age, sex, and education brackets) and computing the mean and standard deviation (SD) for each group. Typically, performance is considered impaired if an individual scores 1.5 SD or more below the normative mean. However, there are limitations to using discrete norms, including arbitrary cut-offs for age and education grouping that can affect the interpretation of an individual's impairment depending on which category they are assigned to Oosterhuis et al. (2016). Other limitations include a small number of individuals in each grouping and a lack of correction for all relevant demographic information (Berrigan et al., 2014). Regression-based norms can be used to overcome these limitations. They are derived by computing linear regressions that control for numerous demographic variables (Testa et al., 2009; Bergman and Almkvist, 2015). Moreover, smaller sample sizes can be used to obtain norms as precise as those obtained from discrete norms (Oosterhuis et al., 2016).

Berard and Walker (2021) established discrete and regression-based normative data for PASAT performance and CF using data from 178 healthy control participants. They established regression-based formulae that were demographically adjusted for sex, age, and number of years of education. Additionally, discrete normative data were established by subdividing participants by number of years of education ($\leq$15 years or $\geq$ 16 years) and age (20–35, 36–50, and 51–65 years of age). Because no significant differences were found between males and females on PASAT performance and CF, the discrete norms were not divided by sex. For both performance and CF, norms were computed for the entire task, for each half of the task, and for each third of the task for the 3″ and 2″ versions.

Although CF is known to be a debilitating symptom for individuals with MS, it was unknown how much CF was experienced by a healthy population. To date, there has not been a universally accepted standard for quantifying a normal amount of CF. Therefore, the goal of the normative data, as previously established by Berard and Walker (2021), was to establish how much CF was experienced by healthy control participants. This data was validated in the current study to determine how well the previous normative data could classify individuals with MS as having impaired performance and CF.

The first objective of the current study was to validate the PASAT discrete and regression-based normative data for quantifying performance and CF in an Ontario sample of individuals with clinically definite MS. The second objective of this study was to determine whether the discrete or regression-based norms were more sensitive to impaired performance and CF in individuals with MS. It was hypothesized that individuals with MS would perform worse than healthy controls on PASAT performance and CF measures. Secondly, it was expected that regression-based norms would classify a greater number of individuals with MS as impaired on PASAT performance and CF than the discrete norms.

MATERIALS AND METHODS

Participants

Participants consisted of 178 healthy controls previously used to establish normative data (Berard and Walker, 2021) and 186 individuals with a confirmed diagnosis of MS. Participants with MS were recruited from the Ottawa Hospital MS Clinic. They were informed about the research study by their treating team and those who indicated interest were then contacted by research staff. Healthy control participants were recruited from the community through word of mouth, posted advertisements, and newspaper and website advertisements. Inclusion criteria for all participants included being between 18 and 65 years of age and fluency in English. A confirmed diagnosis of MS was also required for the MS group for inclusion in the study. Exclusion criteria for all participants included any neurological, medical, or psychiatric condition (besides MS and depression) that might impede cognition, use of legal or illegal drugs that might impact cognition, prior head trauma, a learning disability, attention-deficit hyperactivity disorder, mild cognitive impairment (aside from that related to MS), dementia, or substance abuse.

Procedure and Measures

All participants volunteered to participate in one of three separate studies evaluating cognition in individuals with MS. All three studies contributing to the current project were approved by the Ottawa Health Science Network Research Ethics Board with one of the studies also being approved by the Sunnybrook. Prior to test administration, study procedures were reviewed with all participants, and they were given the opportunity to ask questions. Thereafter, all participants provided their full informed consent. Participants completed a comprehensive battery of neuropsychological tests, including the PASAT, that evaluated multiple cognitive domains. The PASAT was administered as either the third or fourth task in the battery in each of the three studies, such that relative time of administration was unlikely to be a significant factor in performance.

The PASAT version used in the Multiple Sclerosis Functional Composite (MSFC; Cutter et al., 1999) was utilized, with the 3″ PASAT being administered before the 2″ PASAT. Each test consisted of 60 trials. Research assistants trained by a licensed Clinical Neuropsychologist recorded oral PASAT responses at both the 3″ and 2″ ISIs. The total number of correct responses, dyad scores, and percent dyad scores were recorded. CF was measured by creating difference scores between the second and first half of the task as well as the last third and first third of the task. Halves were derived by subtracting the score of the first 30 trials from the second 30 trials (i.e., second half score—first half score). Thirds were derived by subtracting the score of the first 20 trials from the last 20 trials (i.e., last third score—first third score). In order to have an equal number of possible dyads in each portion of the task, a correct dyad was scored for a correct response on the first pair of numbers presented (Fisk and Archibald, 2001). Percent dyad scores were calculated using the following formula: (Dyad Score/Total Correct Score) × 100%. Z-scores for PASAT performance and CF were computed using the discrete norms and regression-based formulae established by Berard and Walker (2021). Participants were classified as impaired in their z-scores were ≥ 1.5 SD below the normative mean.

Analyses

First, analyses were conducted to determine if there were differences between the MS and healthy control group in sex, age, and number of years of education. A chi-square test for independence was used to examine group differences in the proportion of males and females and one-way analyses of variance (ANOVAs) compared group differences in age and number of years of education. A series of one-way analysis of covariance (ANCOVA) tests examined group differences in raw scores of the performance and CF measures of the 3″ and 2″ PASAT. Because there was a group difference in number of years of education, it was included as a covariate for all ANCOVAs. A one-way ANOVA was then performed to determine whether performance and CF scores for the MS group differed between the three studies from which the data for the current study were derived. Then, for the MS group, the proportion of performance and CF z-scores from the discrete and regression-based norms that were ≥ 1.5 SD below the normative mean were computed. Finally, chi-square tests for independence were used to test whether discrete or regression-based norms classified a greater number of individuals with MS as impaired on performance and CF.

RESULTS

Demographics and Disease Characteristics

Information on the demographics and disease characteristics for the MS and healthy control groups is shown in **Table 1**. In the MS group, there was a high proportion of those with RRMS (84.4%) compared to SPMS (11.8%) and PPMS (3.8%). The MS group also had a mean disease duration of 7.2 years and a mean Expanded Disability Status Scale (EDSS) score of 2.3, indicating that participants had minimal to mild disability on average (Kurtzke, 1983). There were no statistically significant group differences in sex or age. Given that education was different between groups, it was thereafter included as a covariate in subsequent analyses.

Study Differences

Given that the sample was comprised of individuals from three different contributing studies, potential differences between studies in PASAT performance and CF were investigated. Significant between-study differences in PASAT performance measures are shown in **Table 2** for the MS group. There were statistically significant between-study differences for total correct and dyad performance measures of the 2″ PASAT. Significant between-study differences in PASAT CF measures for the MS group are shown in **Table 3**. There were statistically significant differences between studies for percent dyad CF scores on the 2″ PASAT for both halves and thirds. There were no statistically significant study differences for the 3″ PASAT.

Group Differences

Performance

Group differences in total correct (**Figure 1**), dyad (**Figure 2**), and percent dyad (**Figure 3**) scores were examined for overall PASAT performance. There were statistically significant group differences in 3″ PASAT performance using total correct scoring [$F_{(1,358)} = 10.34$, $p = 0.004$], dyad scoring [$F_{(1,358)} = 9.38$,

TABLE 1 | Demographic information and disease characteristics.

Demographic variable	Controls	MS	χ^2	*F*	*p*
Sex. *n* (%)	*M* = 34 (19.1) *F* = 144 (80.9)	*M* = 41 (22) *F* = 145 (78)	0.48		0.488
Age (years), *M* (*SD*)	41.5 (12.1)	43.1 (9.7)		1.95	0.163
Education (years), *M* (*SD*)	15.8 (2.4)	15.1 (2.2)		8.64	0.004**
EDSS, *M* (*SD*)		2.3 (1.5)			
Disease duration (years), *M* (*SD*)		7.2 (5.4)			
MS Subtype, *n* (%)		RRMS = 157 (84.4) SPMS = 22 (11.8) PPMS = 7 (3.8)			

*M, males; F, females; EDSS, Expanded Disability Status Scale score; RRMS, relapsing remitting multiple sclerosis; SPMS, secondary progressive multiple sclerosis; PPMS, primary progressive multiple sclerosis. **p < 0.01.*

TABLE 2 | Between-study differences in 2″ PASAT performance measures for the MS group.

Measure	IPSIMS *M* (*SD*)	BICAMS *M* (*SD*)	SUNSCREEN *M* (*SD*)	*F*	*p*
Total correct	19.9 (12.0)	31.7 (12.7)	29.5 (11.0)	17.93	<0.001***
Dyad	11.0 (10.3)	20.3 (14.8)	17.4 (12.8)	9.13	<0.001***

*IPSIMS, Information processing speed in MS study; BICAMS, Brief international cognitive assessment for MS study; SUNSCREEN, Using computers to assess cognition in MS study. ***p < 0.001.*

TABLE 3 | Between-study differences in 2″ PASAT cognitive fatigability measures for the MS group.

Measure	IPSIMS *M* (*SD*)	BICAMS *M* (*SD*)	SUNSCREEN *M* (*SD*)	*F*	*p*
Percent dyad (Halves)	−30.4 (28.4)	−17.6 (17.6)	−16.0 (20.8)	7.38	0.001**
Percent dyad (Thirds)	−35.7 (29.6)	−22.7 (21.1)	−24.3 (21.3)	5.22	0.006**

*PSIMS, Information processing speed in MS study; BICAMS, Brief international cognitive assessment for MS study; SUNSCREEN, Using computers to assess cognition in MS study. **p < 0.01.*

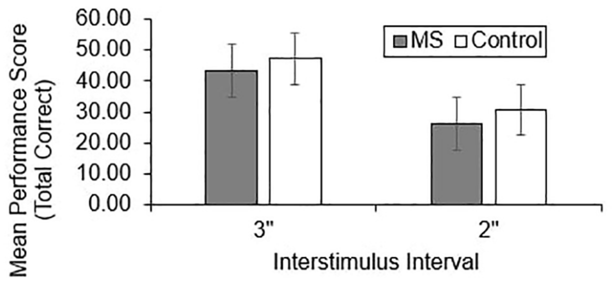

FIGURE 1 | Group difference in total correct performance scores.

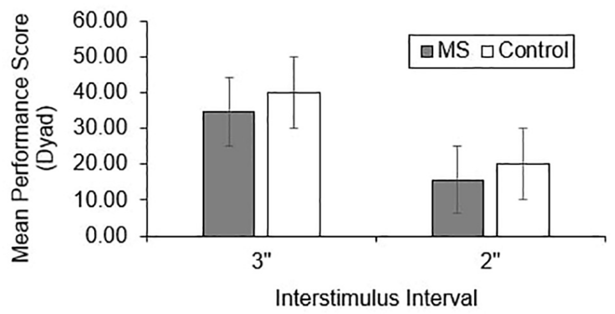

FIGURE 2 | Group difference in dyad performance scores.

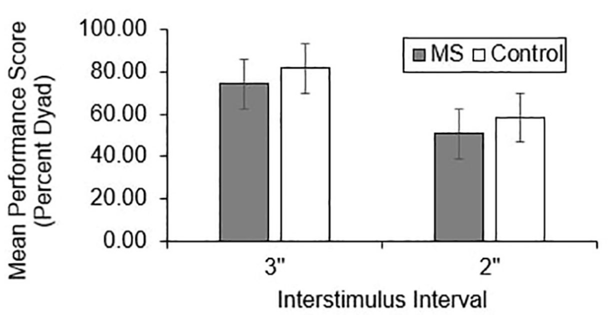

FIGURE 3 | Group difference in percent dyad performance scores.

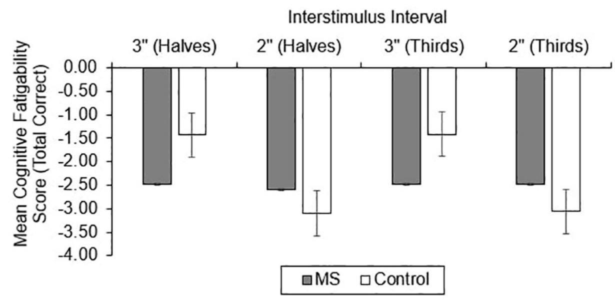

FIGURE 4 | Group difference in total correct cognitive fatigability scores.

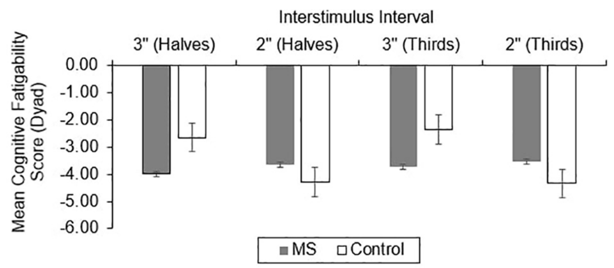

FIGURE 5 | Group difference in dyad cognitive fatigability scores.

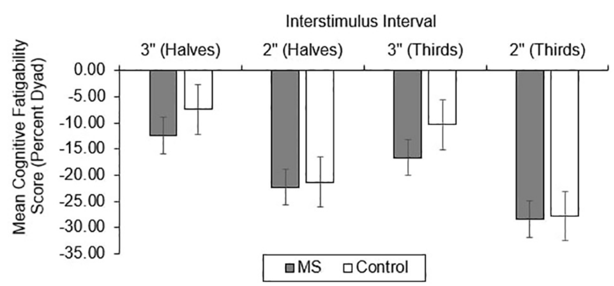

FIGURE 6 | Group difference in percent dyad cognitive fatigability scores.

$p = 0.002$], and percent dyad scoring [$F_{(1,358)} = 9.05$, $p = 0.003$]. There were also statistically significant group differences using total correct scoring [$F_{(1,351)} = 5.84$, $p = 0.016$], dyad scoring [$F_{(1,351)} = 5.92$, $p = 0.015$], and percent dyad scoring [$F_{(1,351)} = 5.94$, $p = 0.015$] for the 2″ PASAT. The MS group scored lower on all performance measures than the healthy control group.

Cognitive Fatigability

For CF, group differences in total correct (**Figure 4**), dyad (**Figure 5**), and percent dyad (**Figure 6**) scores were also examined. Individuals with MS performed significantly worse than healthy controls on all CF measures for the 3″ PASAT. Using halves, the two groups significantly differed in total correct scores [$F_{(1,\ 358)} = 6.57$, $p = 0.011$], dyad scores [$F_{(1,358)} = 4.85$, $p = 0.028$], and percent dyad scores [$F_{(1,358)} = 6.45$, $p = 0.012$]. Using thirds, the two groups also significantly differed in total correct scores [$F_{(1,358)} = 8.50$, $p = 0.004$], dyad scores [$F_{(1,358)} = 6.87$, $p = 0.009$], and percent dyad scores [$F_{(1,358)} = 7.54$, $p = 0.006$].

There were no statistically significant group differences in CF for the 2″ PASAT when halves were used to calculate total correct scores [$F_{(1,350)} = 1.01$, $p = 0.316$], dyad scores [$F_{(1,350)} = 1.46$, $p = 0.228$], or percent dyad scores [$F_{(1,350)} = 0.00$, $p = 0.959$]. In addition, there were no statistically significant group differences in CF on the 2″ PASAT when thirds were used to calculate total scores [$F_{(1,350)} = 1.74$, $p = 0.189$], dyad scores [$F_{(1,350)} = 3.08$, $p = 0.080$], or percent dyad scores [$F_{(1,350)} = 0.02$, $p = 0.898$]. Thus, the MS group performed significantly worse than healthy controls on all CF measures only when a 3″ ISI was used.

Impairment

The proportion of individuals with MS who demonstrated impaired performance (**Table 4**) and CF (**Table 5**) using the discrete and regression-based norms was determined for all scoring methods. In contrast to the hypothesis that regression-based norms would be more sensitive to impaired performance and CF than the discrete norms, there were no statistically significant differences in the number of individuals with MS who were classified as impaired.

DISCUSSION

CF is a common and challenging symptom for individuals with MS that negatively affects daily functioning, increases the likelihood of unemployment, and reduces quality of life. Although CF is known to be a debilitating symptom for individuals with MS, there is currently no universally accepted method for quantifying it. Therefore, the goal of this study was to validate discrete and regression-based normative data established by Berard and Walker (2021) to detect impaired performance and CF in an Ontario sample of individuals with MS. The second objective of this study was to determine whether discrete or regression-based norms were more sensitive to impaired performance and CF in individuals with MS. The results validated both the discrete and regression-based PASAT norms for use with individuals with MS. However, there was no advantage of using regression-based norms to detect impaired performance or CF compared to using discrete norms.

TABLE 4 | The proportion of the MS group impaired on PASAT performance measures.

Measure	Discrete norms,% impaired	Regression-based norms,% impaired
3″ PASAT		
Total correct	18.3	18.3
Dyad	19.9	21.5
Percent dyad	18.8	16.7
2″ PASAT		
Total correct	13.4	14.5
Dyad	1.6	4.8
Percent dyad	15.1	14.0

TABLE 5 | The proportion of the MS group impaired on PASAT cognitive fatigability measures.

Measure	Discrete norms, % impaired	Regression-based norms, % impaired
3″ PASAT (Halves)		
Total correct	12.9	12.9
Dyad	8.6	9.7
Percent dyad	14.5	13.4
2″ PASAT (Halves)		
Total correct	5.9	4.8
Dyad	7	5.4
Percent dyad	9.1	7
3″ PASAT (Thirds)		
Total correct	12.4	9.1
Dyad	10.2	9.7
Percent dyad	15.1	12.9
2″ PASAT (Thirds)		
Total correct	4.3	4.3
Dyad	4.3	4.3
Percent dyad	10.2	8.1

Group Differences

Performance

Individuals with MS demonstrated significantly lower raw PASAT scores than healthy controls regardless of the scoring method or ISI that was used. These results are consistent with previous studies that have demonstrated worse PASAT performance in individuals with MS compared to healthy controls using total correct (Kujala et al., 1995; Rosti et al., 2006; Solari et al., 2007), dyad (Kujala et al., 1995; Snyder and Cappelleri, 2001; Snyder et al., 2001; Rosti et al., 2006; Solari et al., 2007), and percent dyad scoring methods (Fisk and Archibald, 2001; Rosti et al., 2006). Thus, the performance of an Ontario-based early sample is in keeping with what has been documented in other studies.

Cognitive Fatigability

Consistent with our expectations, the results showed that individuals with MS demonstrated greater CF than healthy controls using all scoring methods on the 3″ PASAT. This was true whether halves or thirds were used, suggesting that both methods detected significantly more CF in the MS group than the healthy control group. However, there were no significant group differences in CF for the 2″ PASAT when either halves or thirds were used. This might be explained by the increased difficulty of the 2″ PASAT which likely challenged the cognitive capacity of both groups from the onset of the task. As a result, CF scores did not differ between the two groups. Because the 2″ PASAT was always administered after the 3″ version, another possibility is that participants were already fatigued by the time they completed the 2″ version. Given the effort required to maintain performance at the prior 3″ ISI, it may have resulted in impaired performance from the onset of the 2″ task.

Impairment

Performance

Across all scoring methods, both the discrete and regression-based norms classified a greater proportion of individuals with MS as impaired using the 3″ compared to the 2″ PASAT. As previously discussed, a shorter ISI likely increased the difficulty of the PASAT even for the healthy control participants, resulting in decreased sensitivity at detecting impaired performance in individuals with MS, given the difficulties experienced by both groups. For the 3″ PASAT, all scoring methods, for both types of norms, classified a similar proportion of participants (all over 16%) as having impaired PASAT performance (**Table 4**). For the 2″ PASAT, dyad scoring classified a much smaller proportion of individuals with MS as impaired (1.6–4.8%) compared to total correct and percent dyad scoring (13.4–15.1%; **Table 4**). This lower proportion of impairment is expected given the increased difficulty of the task and the fact that participants are less likely to obtain two correct scores in a row. This again justifies the use of the 3″ PASAT in the MSFC over the 2″ version given its greater sensitivity to impairment.

Cognitive Fatigability

Similar to the performance impairments, both the discrete and regression-based norms classified a greater proportion of individuals with MS as having impaired CF using the 3″ compared to the 2″ PASAT. This suggests that the 2″ PASAT was difficult and fatiguing for both groups and likely resulted in floor effects. The 2″ PASAT may be too difficult for participants from the beginning of the task thereby making it less likely to detect a breakdown in performance over the course of the task (i.e., as many errors occur at the beginning of the task as at the end of the task). However, for both ISIs, halves and thirds were equally sensitive to impaired CF. This suggests that comparing scores between the first half and last half of the PASAT, or the first and last third of the PASAT, are both effective methods for detecting impaired CF in MS. Additionally, for both ISIs and norm types, percent dyad scoring classified the largest proportion of individuals with MS as having impaired CF compared to total correct and dyad scoring methods.

The results of the current study support prior research by Walker et al. (2012) who found that percent dyad scoring was most sensitive to CF for the 3″ PASAT, while both groups had difficulties meeting task demands for the 2″ PASAT. Thus, regardless of whether one is interested in detecting group differences or impairment in performance and CF, a decrease over time in the proportion of time spent appropriately meeting the working memory demands of the PASAT appears to be the most sensitive manner of CF detection. As the task progresses, those with MS are less able than healthy controls to meet the working memory demands of the task and also demonstrate the highest rate of impairment when this scoring method is used for the 3″ PASAT (12.9–15.1%; **Table 5**).

Discrete and Regression-Based Norms

In contrast to our hypothesis, there were no statistically significant differences in the proportion of individuals with MS who were classified as having impaired performance or CF using the regression-based norms compared to the discrete norms. Therefore, the addition of sex in the regression-based formulae (over and above the age and number of years of education accounted for in the discrete norms) did not improve the sensitivity at detecting impairment for either performance or CF in the MS group. This is in line with previous research demonstrating that performance on the PASAT is not affected by sex or gender (Johnson et al., 1988; Roman et al., 1991; Wingenfeld et al., 1999; Fluck et al., 2001) or that the effect of sex was very small and not clinically significant (Brittain et al., 1991; Wiens et al., 1997; Diehr et al., 1998).

That regression-based norms did not detect higher rates of impairment was unexpected given that prior research has shown that regression-based norms are typically more sensitive than discrete norms for capturing cognitive functioning in MS (Smerbeck et al., 2012; Burggraaff et al., 2017). However, the lack of similar findings in the current study might be explained by the fact that additional variables, such as ethnicity, were not accounted for. Age, education, and ethnicity have been found to be significant predictors of PASAT performance in prior studies (Diehr et al., 1998, 2003). Discrepancies in neuropsychological test performance between different ethnicities have been explained by socioeconomic status, which has been found to correlate highly with neuropsychological test scores and the risk of disability from MS (Gasquoine, 2009; Calocer et al., 2020). Separate from the number of years of education, there have also been historical differences in the quality of education afforded to different ethnicities (Manly et al., 2002; O'Bryant et al., 2007) which might impact neuropsychological test performance. Thus, ethnicity is an important variable to control for in future PASAT normative data.

Limitations and Future Directions

The present study was the first to validate PASAT discrete and regression-based normative data for measuring CF in an early Ontario sample of individuals with MS. Despite the important implications of the results, the study is not without limitations. One limitation is that the data were analyzed retrospectively and were derived from three prior studies. Measures that were used in the neuropsychological evaluations varied between the three studies, resulting in the inability to examine potential correlations between CF and subjective fatigue or depression. In addition, medication use was not examined. It is possible that some participants were taking medications that improved their fatigue, such as fampridine-SR and/or antidepressants. Therefore, future research should examine medication use or exclude participants taking medications that might impact their CF results.

Another limitation of this study was the characteristics of the sample. Because the sample was exclusively from Ontario, it is unclear how the impairment classification rates would differ in other regions given potential differences in demographics. Additionally, the majority of participants in the study had RRMS, a low disease duration, and low levels of disability, reflected by a low mean EDSS score. These characteristics might have impacted the type and extent of cognitive deficits that were observed since individuals with RRMS typically demonstrate milder information processing speed deficits than those with progressive disease subtypes (De Sonneville et al., 2002; Benedict et al., 2006; Potagas et al., 2008). Furthermore, individuals with MS with shorter disease durations tend to show more subtle cognitive impairments than those with longer disease durations (Amato et al., 1995, 2001; Achiron et al., 2013). The fact that rates of CF impairment up to 15% were detected in this sample despite the low disease duration and level of disability speaks to the sensitivity of the norms at detecting even subtle cognitive changes early in the disease course. CF is correlated with MS pathophysiology (Manjaly et al., 2019), with the pathophysiology being more pronounced in progressive subtypes compared to RRMS (Dutta and Trapp, 2014). Future work should, therefore, validate the PASAT normative data for performance and CF in a sample with a longer disease duration and in more individuals with progressive disease subtypes since this sample may be more likely to show evidence of CF.

Another direction for future research is to develop CF norms for other neuropsychological tests of information processing speed and working memory and to validate these norms in a sample of individuals with MS. In prior studies, CF has been investigated using the Blocked Cyclic Naming Task (Cehelyk et al., 2019), the Stroop task (Chinnadurai et al., 2016), the Symbol Digit Modalities Test (DeLuca et al., 2008; Chinnadurai et al., 2016), and the *n*-back task (Bailey et al., 2007). In particular, future research should aim to establish concurrent validity with these measures.

Future research should also aim to establish effective interventions to target CF. There is currently a lack of clear recommendations on how CF should be measured and treated (Walker et al., 2019). Past treatments have included a pharmacological intervention using fampridine-SR (Morrow et al., 2017) and a procedural intervention using transcranial direct current stimulation (Fiene et al., 2018), with only the procedural intervention demonstrating efficacy in treating CF. There is a need to investigate whether or not behavioral interventions are efficacious. Given that such interventions have been successful in targeting subjective fatigue (Plow et al., 2019), it would be prudent to see if modifications of such techniques could prove beneficial for CF as well.

CONCLUSION

In conclusion, the current study validated the PASAT discrete and regression-based norms for performance and CF in a sample of individuals with MS. These results have important implications, given that the inability to maintain optimal cognitive performance over a long period of time may limit an individual's productivity and ability to concentrate at work or school. For clinicians, these results provide a new tool for documenting statistically significant CF in their patients. Overall, the results revealed that the 3′′ PASAT is sufficient for detecting impaired performance and CF. Scoring CF using halves and thirds were found to be equally effective methods for detecting impairment. Finally, both the discrete and regression-based norms were equally effective at detecting impaired performance and CF in this sample.

DATA AVAILABILITY STATEMENT

The datasets presented in this article are not readily available because participants of this study did not consent for their data to be shared publicly. Requests to access the datasets should be directed to corresponding author.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by the Ottawa Health Science Network Research Ethics Board. The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

CW contributed to the design of the study, conducted analyses, and wrote and edited the article. JB conceived and designed the study, conducted analyses, and edited the article. LW conceived and designed the study as well as edited the article. All authors contributed to the article and approved the submitted version.

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Training Willpower: Reducing Costs and Valuing Effort

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The integrative model of effortful control presented in a previous article aimed to specify the neurophysiological bases of mental effort. This model assumes that effort reflects three different inter-related aspects of the same adaptive function. First, a mechanism anchored in the salience network that makes decisions about the effort that should be engaged in the current task in view of costs and benefits associated with the achievement of the task goal. Second, a top-down control signal generated by the mechanism of effort that modulates neuronal activity in brain regions involved in the current task to filter pertinent information. Third, a feeling that emerges in awareness during effortful tasks and reflects the costs associated with goal-directed behavior. The aim of the present article is to complete this model by proposing that the capacity to exert effortful control can be improved through training programs. Two main questions relative to this possible strengthening of willpower are addressed in this paper. The first question concerns the existence of empirical evidence that supports gains in effortful control capacity through training. We conducted a review of 63 meta-analyses that shows training programs are effective in improving performance in effortful tasks tapping executive functions and/or self-control with a small to large effect size. Moreover, physical and mindfulness exercises could be two promising training methods that would deserve to be included in training programs aiming to strengthen willpower. The second question concerns the neural mechanisms that could explain these gains in effortful control capacity. Two plausible brain mechanisms are proposed: (1) a decrease in effort costs combined with a greater efficiency of brain regions involved in the task and (2) an increase in the value of effort through operant conditioning in the context of high effort and high reward. The first mechanism supports the hypothesis of a strengthening of the capacity to exert effortful control whereas the second mechanism supports the hypothesis of an increase in the motivation to exert this control. In the last part of the article, we made several recommendations to improve the effectiveness of interventional studies aiming to train this adaptive function.

"Keep the faculty of effort alive in you by a little gratuitous exercise every day."
James (1918, p. 127)

Keywords: cognitive training, effort, executive functions, transfer, exercise training, effortful control, self-control, mindfulness training

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INTRODUCTION

In daily life, our behavior mainly depends on routinized, automatic and unconscious processes (Ouellette and Wood, 1998; Kahneman, 2011). However, in some cases, effortful control is required to perform a more demanding task, such as maintaining concentration on complex problem solving (e.g., academic tasks), sustaining attention on infrequent cues (e.g., vigilance tasks), repressing immediate impulses to secure delayed benefits or avoid expected costs (e.g., self-control situations), or exercising at an uncomfortable intensity (e.g., sport and rehabilitation situations). Effortful control is deliberate, costly and exerted over brain areas involved in the achievement of a task goal (André et al., 2019; Müller and Apps, 2019). Effortful control is the product of the activity of the mechanism of effort anchored in the salience network (for more details, see André et al., 2019), which includes the dorsal anterior cingulate cortex and the anterior insula (Seeley et al., 2007). The metaphor of the steering wheel (Bargh, 1997; Baumeister and Sommer, 1997) is relevant and illustrates the importance of effortful control in behavior: Even if a car is driven straight-ahead 95% of the time (thus no need for steering), a car without a steering wheel is not 95% as good as a car with one.

People who have a high capacity to exert effortful control are more likely to perform better in work, school and sport situations that require controlled attention or self-control. On the contrary, people who have a low dispositional capacity to exert effortful control, such as individuals with addictions, obsessive-compulsive disorder or attention-deficit hyperactivity disorder, generally present difficulties to regulate intrusive thoughts and emotions and to delay rewards (Pinto et al., 2014; Lugo-Candelas et al., 2017; Eichholz et al., 2020; Lim et al., 2020). In fact, developmental studies have shown that effortful control capacity in childhood predicts academic achievement, physical health, substance dependence, personal finances, antisocial behaviors and criminal offending outcomes later in life (Tarter et al., 2003; Moffitt et al., 2011; Liew, 2012; Fergusson et al., 2013; Daly et al., 2015; Holmes, 2018; for a review see Robson et al., 2020). Strengthening the capacity to exert effortful control through training could be a good way to improve quality of life and well-being of individuals, particularly those who have a low capacity. The term 'capacity' refers here to the ability or skill to exert effortful control rather than the maximum amount of resources devoted to effortful control. The aim of the present article is to show that the capacity to exert effortful control is trainable and to propose two plausible neurophysiological mechanisms supporting these durable changes in capacity.

The strength model of self-control (Baumeister et al., 2007, 2018) proposes that self-control could be strengthened through training. Taking the metaphor of the muscle, this model assumes that regular exertions of self-control can improve willpower strength and stamina, just as exercise training can strengthen muscles. The mechanisms underlying these gains in self-control would be an improvement in the self-regulatory general core capacity, i.e., increasing available self-control resources (Oaten and Cheng, 2006). Another important prediction of this model is that the improvements in the general capacity induced by the training program can be extended to other spheres of self-regulation unrelated to what had been practiced (Baumeister et al., 2006). In support of this model, two recent meta-analyses showed that self-control training is effective at strengthening the ability to self-regulate (Friese et al., 2017; Beames et al., 2018).

The strength model of self-control makes a last important prediction: the capacity to exert effortful control can be temporarily weakened after the performance of a first effortful task. This phenomenon called 'ego depletion effect' was recently challenged regarding its actual existence (Carter et al., 2015; Vadillo, 2019), and replicability (Xu et al., 2014; Hagger et al., 2016; Lurquin et al., 2016; Osgood, 2017; Alós-Ferrer et al., 2019; Vohs et al., 2021). This debate, which certain researchers considered as closed, is beyond the scope of this paper. But, what does it mean for the present theory if the ego depletion effect is so small that it is practically impossible to study? Strengthening willpower through training should increase the ability to compensate for a temporary weakening of the capacity to exert effortful control (i.e., an ego depletion effect). Consequently, any reader could think that it would be useless to study the possible strengthening of willpower through training if the ego depletion effect does not exist or is negligible.

Three arguments justify the pertinence of studying the improvement of the capacity to exert effortful control through training in spite of this questioning about ego depletion. First, denying a possible transient weakening of the capacity to exert effortful control after a long and intense use of this capacity is ignoring all the literature on cognitive, mental and central fatigue. Cognitive fatigue is generally evidenced in vigilance tasks by a decrement of performance with time-on-task (Mackworth, 1964; Boksem et al., 2005; Ackerman, 2011). In other respect, sport sciences are interested in the impact of mental or central fatigue induced by long and highly demanding cognitive tasks on sport performance. Two systematic reviews conducted on this topic showed a consistent effect of cognitive fatigue on endurance performance (Van Cutsem et al., 2017; Pageaux and Lepers, 2018). Moreover, a recent study showed that performance decreased with time-on-task during a classical depleting task; i.e., the 'e' letter task (Arber et al., 2017).

Second, willpower is the capacity to exert effortful control in spite of high costs (Baumeister and Tierney, 2011). As we will see further, different categories of costs are involved in effort-based decision making (i.e., decision about the amount of effortful control dedicated to the achievement of the task goal). Ego depletion and cognitive fatigue belong to the same category of costs: a temporary weakening of the capacity to exert effortful control that requires a compensatory investment in effortful control to maintain performance (André et al., 2019). Other categories of costs can modulate the effort-based decision making, such as the pain associated with the achievement of the task goal (e.g., muscle pain while carrying out a resistance exercise) or the risk of repeated failures (e.g., ego threat or threat to the physical integrity associated with task failures). In this perspective, strengthening willpower allows to cope with a large variety of stressful situations, including fatiguing tasks, painful tasks and risky tasks. For instance, a long-distance runner (e.g., ultra-marathon) has to cope with cognitive fatigue, muscular fatigue, and muscular pain; i.e., the athlete has to maintain an effortful control in spite of these costs if he/she wants to succeed. Consequently, even if the cognitive fatigue associated to the task is negligible, a successful coping with the other constraints of the task justify to train willpower.

Third, the transient weakening and the durable strengthening of the capacity to exert effortful control rely on two distinct neurobiological mechanisms that can be studied separately. As suggested by several authors, the temporary weakening of the capacity to exert effortful control relies on a short-term synaptic mechanism induced by an accumulation of adenosine in prefrontal brain regions involved in the ongoing task (Martin et al., 2018; André et al., 2019). By contrast, as we will see further, the durable strengthening of the capacity to exert effortful control relies on long-term synaptic mechanisms modifying the efficacy of glutamatergic synapses involved in the circuitry connecting the anterior cingulate cortex with brain structures computing costs and benefits (see the section "Neural Bases of Gains in Effortful Control Capacity through Training"). These two phenomena relying on two distinct neurobiological mechanisms, the existence or non-existence of the former does not in any way affect the existence or non-existence of the later, and reciprocally.

The concept of 'resources' applied to self-control and ego depletion has also been criticized and some authors, such as Michael Inzlicht, preferred to develop a non-resource-based account of the short- and long-term dynamic characteristics of self-control (Inzlicht et al., 2014b). Evidence for this model has not been forthcoming, and indeed the central prediction — that ego depletion manipulations reduce motivation to exert self-control on the dependent measure — has failed repeatedly (see Baumeister and Vohs, 2016).

Concerning the trainability of the capacity to exert effortful control, the alternative theory proposed by Inzlicht et al. (2014a,b) emphasizes that the motivation to exert effortful control can be increased using motivational techniques, such as implementation intentions and motivational interviewing (for a review, Berkman, 2016). In contrast to this model, we make a clear distinction between the capacity to exert effortful control and the motivation to exert effortful control. People can have the ability without being motivated (and vice versa). For example, some studies clearly showed that individuals are sometimes able to engage in effortful control (i.e., a capacity) but decide not to engage (i.e., a motivation) (e.g., Treadway et al., 2009). Therefore, the decision is not made toward the desired rewards but in order to escape the cost of the effort. As mentioned above, capacity refers to the ability to mobilize brain resources dedicated to effortful control, whereas motivation refers to the motive to mobilize these resources. Generally, training programs aim to develop capacities, and motivational techniques help researchers and practitioners increase the motivation and volition of individuals to engage in these effortful interventions and training programs. Michie et al. (2013) identified up to 93 theory-based behavior change techniques (BCTs) aiming to improve adherence to interventions. The use of these techniques is a prerequisite for the success of an intervention, but they are not the heart of the intervention and do not fully explain the improvement in trained capacity. Generally, the tasks and exercises repeatedly practiced by the participants constitute the true active element leading to a change in the capacity to be improved.

The integrative model of effortful control published by the authors in 2019 (André et al., 2019) proposed a theoretical framework based on recent findings in the field of neuroscience that define clearly what is effort and effortful control and which neuronal network underpins the capacity to exert effortful control. It particularly invokes the following contributions from neuroscience: the theory of attentional effort regarding the role of the cholinergic pathway in the generation and maintenance of the effort signal (Sarter et al., 2006), the theory of the dissociation between the salience network and the executive control network (Seeley et al., 2007; Seeley, 2019), the theory of the dynamic network connectivity regarding the short-term neuroplastic mechanisms that can explain a reduction in prefrontal activity following an exposure to stress or fatigue (Arnsten, 2009; Arnsten et al., 2010, 2012), and the theory of the expected value of control concerning the role of the anterior cingulate cortex in effort-based decision making (Shenhav et al., 2013, 2017).

The main proposal of this model is that effort designates three functional parts: (1) a mechanism anchored in the salience network (i.e., the mechanism of effort), which specializes in perceiving and responding to homeostatic and allostatic demands (Seeley, 2019), (2) a control signal (i.e., the effort signal) that is the main product of the mechanism of effort, that oscillates in the theta band (Onton et al., 2005; Sauseng et al., 2007; Kao et al., 2013), and whose the function is to filter information in the brain regions receiving this control signal, (3) a perception that emerges in awareness during effortful tasks (i.e., the perception of effort), which is a secondary product of the mechanism of effort and reflects the costs associated with the goal-directed behavior. Exerting effortful control, i.e., generating the control signal, is the main function of the mechanism of effort.

The strength model of self-control and the integrative model of effortful control share two important predictions: (1) the capacity to exert effortful control can be temporarily weakened when it is overloaded and used during a long period; (2) the capacity to exert effortful control can be durably improved through extensive and adapted training. However, our integrative model of effortful control differs from the strength model of self-control in three important points: (1) the mechanism underpinning the transient decrease in effortful control capacity (i.e., ego depletion effect or cognitive fatigue effect) is not viewed as the depletion of a resource, but as the weakening of the capacity of a neural system to generate a control signal because of a short-term synaptic mechanism induced by an accumulation of adenosine in prefrontal brain regions involved in the ongoing task, (2) predictions are made at the behavioral and neurophysiological levels and not only at the behavioral level (e.g., durable increase of performance accompanied by a durable increase in between-network connectivity with training), (3) the general core capacity that can be temporarily weakened through intensive use and durably strengthened through training is anchored in the salience network and not in the executive control network that underpins inhibitory control.

The present article focuses on the mechanisms leading to improvements in the capacity to exert effortful control.

Motivational techniques are viewed here as moderators that facilitate the engagement of effortful control in training tasks throughout the entire duration of the intervention. The modulatory influence of these moderators on mechanisms leading to an increase in the engagement of effortful control is beyond the scope of this paper.

More specifically, the present paper aims to describe the hypothetical neurophysiological mechanisms that could underpin improvements in the capacity to exert effortful control. Arguing that training increases the amount of available resources (i.e., the capacity of a tank) is not a sufficient level of explanation to improve the methodology, efficacy, and effectiveness of effortful control interventions. This paper tries to answer the two following questions: Is there clear evidence for improvements in effortful control capacity with training? And if so, which durable changes in brain functioning explain these increments in effort capacity?

The following sections provide answers to these questions. In the first section "Definitions," we present several interrelated concepts that are the object of the training. In the second section "Improvements in Effortful Control with Practice: An Umbrella Review of Meta-Analytic Reviews," we summarize the main results of several meta-analytic reviews examining the effects of training on the capacity to exert effortful control. We discuss the significance and the size of this effect as a function of several moderators, such as the duration of the intervention and the type of exercises used to train the capacity to exert effortful control. We also address the issue concerning the generalizability/transferability of gains in effortful control capacity. Then, in the third section "Neural Bases of Gains in Effortful Control Capacity through Training," we describe two brain mechanisms that could explain these training effects. Finally, in the last section "Challenging the Trainability of Effortful Control Capacity," we formulate a series of recommendations to examine these training effects in the future.

DEFINITIONS

As mentioned in the previous section, a gain in capacity in effortful control can be very beneficial for an individual to increase his/her likelihood of success in personal achievement. In this section, we present the main concepts that constitute the target of the training interventions.

Two broad categories of training programs that are able to improve effortful control capacity have been identified (Beames et al., 2018). The first category of training programs aims to improve executive functions, whereas the second category aims to strengthen self-control, willpower or self-regulation. The following paragraphs will help the reader to disentangle the links between all these closely related concepts and then to understand more clearly how they fit together.

The concept of executive functions (EFs) comes from cognitive psychology and designates high-level cognitive functions anchored in the executive control network, which is a frontoparietal network bilaterally involving the dorsolateral prefrontal cortex (DLPFC) and the posterior parietal cortex (Seeley et al., 2007). Executive control must be distinguished from effortful control that is exerted by another large-scale network: the salience network (Seeley et al., 2007; Seeley, 2019). These two networks are both activated as soon as someone is engaged in a cognitive or physical task (i.e., they are task-positive networks) but ensure different functions. The level of activation of these two networks depends, among other things, on the cognitive load of the ongoing task (Paus et al., 1998). Executive control allows individuals to mentally shift through ideas, to reason before acting, to cope with novel and unexpected challenges, to resist temptations and to stay focused on a specific goal (Diamond, 2013), whereas effortful control helps targeted brain regions involved in the ongoing task to keep the focus on relevant task features (André et al., 2019). The salience network and the executive control network are bidirectionally interconnected. Effortful control enhances executive functioning whereas executive functions send cost signals to the salience network that generates effortful control according to a cost/benefit decision-making.

Miyake et al. (2000) identified three main separable EFs that share commonalities: (a) shifting between tasks or mental sets, (b) updating and monitoring of working memory representations, and (c) inhibition of dominant or prepotent responses. The first component of EFs is also called 'cognitive flexibility' and corresponds to the ability to shift from one mental set to another mental set, from one set of guidelines for action to a different set (e.g., shifting from a status of an offensive player to a status of defender in basket-ball as soon as the ball is caught by the opponents). The second component is the ability to maintain, refresh and manipulate relevant information in working memory (e.g., performing the mental rotation of the representation of an object). The third component, also called 'inhibitory control,' 'intentional inhibition,' or 'controlled inhibition,' is the ability to repress or stop prepotent impulses, unwanted and intrusive thoughts, embarrassing emotions, or automatic motor responses.

More recently, Zelazo and Carlson (2012) introduced a new taxonomy of EFs, taking into account the context in which participants exert executive control. These authors proposed distinguishing cool EFs solicited and assessed in emotionally neutral contexts, such as laboratory settings, and hot EFs involved in motivationally and emotionally significant high-stakes situations, such as multiplayer online role-playing games or real social situations in daily life. As discussed later, these two categories of EFs are used in different types of interventions aiming to develop effortful control capacity.

Inhibitory control presents many similarities with the concept of self-control used in social psychology when the latter is more restrictively designated as the ability to follow rules or inhibit immediate desires so as to delay gratification (e.g., Muraven and Baumeister, 2000, p. 247), as well as to interrupt undesired behavioral tendencies and refrain from acting on them (e.g., Tangney et al., 2004, p. 274). However, the concept of self-control has a larger meaning when it is used interchangeably with the concept of self-regulation (Baumeister and Vohs, 2016, p. 70). Based on this larger meaning, it refers to the ability to voluntarily regulate attention, emotion, and behavior in the service of more highly valued goals and represents the deliberate, conscious, effortful subset of self-regulation (Baumeister et al., 2007, p. 351).

Willpower is a folk term referring to mental energy that is expended in difficult acts of self-control, such as resisting temptation and delaying gratification (Baumeister and Tierney, 2011). It is often used in connection with making current sacrifices for the sake of long-term benefits and goals. In the same way, effortful control invokes executive functions and helps to inhibit behavioral impulses so as to regulate emotions and behaviors, thereby enabling people to adjust to situations in flexible, adaptive fashion (André et al., 2019). The common theme is that the Self exerts effort to regulate its own responses to produce preferred outcomes. Philosophers have identified a set of virtues or skills associated with a strong willpower, such as persistence, endurance, perseverance, resoluteness and patience (Roberts, 1984; Steutel, 1999; Szutta, 2020). All these virtues help an individual to remain focused on his/her intended goals and to facilitate their achievement. In the framework of the integrative model of effortful control, we assume that willpower is the capacity to exert effortful control in difficult situations, such as sustaining attention in boring vigilance task or maintaining a high intensity of exercise in spite of fatigue and pain.

One important commonality between EFs and self-control, in both its more restrictive and larger meaning, is that all these high-level cognitive functions require effortful control. Based on the framework of the integrative model of effortful control (André et al., 2019), we assume here, that the self-regulatory general core capacity, which can be temporarily weakened through intensive use and durably strengthened through training, corresponds to the effortful control capacity ensured by the salience network. In addition, we assume that the good functioning of the executive control network, which underpins EFs, depends directly on the effortful control exerted by the salience network.

Effortful control is not conceived here as a depletable resource but as a control signal that can be weakened and/or deteriorated under the effect of fatigue (for more information about the mechanisms underpinning the possible weakening of this control signal, see André et al., 2019). In the same way, effortful control capacity can be conceived as the function of the mechanism of effort to generate this control signal, which can be directly assessed by measuring spectral power of theta-wave activity above prefrontal areas (e.g., Cavanagh and Franck, 2014; Fairclough and Ewing, 2017). Higher the density of prefrontal theta-wave activity is, higher the engagement in effortful control. Exerting effortful control means that the organism needs to mobilize energy, and the activation of the sympathetic system is closely linked to the exertion of effortful control (Critchley, 2005). In that way, indexes of sympathetic activity, such as pupil size and pre-ejection period, are used as indirect measures of effortful control (Richter et al., 2008; van der Wel and van Steenbergen, 2018).

Based on the above, we can make a series of hypotheses: (1) the capacity to maintain a high level of effortful control over time in spite of fatigue or pain (i.e., a high level of concentration or effort engagement) can be strengthened through training programs involving effortful activities; (2) training programs targeting self-control or EFs stimulate effortful control and can strengthen this general capacity; (3) training programs more specifically targeting EFs lead to several synergistic effects: a strengthening of the effortful control capacity through durable changes within the salience network, a strengthening of the EFs through durable changes within the executive control network and a strengthening of the connectivity between these two networks.

Finally, the notion of transfer is central in the cognitive training literature and related to the generalizability of the gain obtained through extensive practice. Transfer distance refers to the similarities between the trained tasks and the tasks used to demonstrate a gain in performance at the end of the intervention (i.e., the principal outcome). Two types of transfer can be distinguished: (a) 'near-transfer' effects when trained tasks and postintervention untrained tasks are similar, (b) 'far-transfer' effects when trained tasks and postintervention tasks are dissimilar. The ultimate goal of interventions targeting effortful control capacity is to promote far-transfer effects because the gain in this general capacity should ideally be transferable to a broad range of everyday functional activities.

IMPROVEMENTS IN EFFORTFUL CONTROL WITH PRACTICE: AN UMBRELLA REVIEW OF META-ANALYTIC REVIEWS

In this section, we summarize the main results of meta-analyses focusing on the long-term effects of different types of training methods stimulating effortful control. As mentioned earlier, Beames et al. (2018) distinguished two main categories of training methods: methods focusing on improving executive functions and methods focusing on strengthening self-control. Each following subsection addresses three important issues: the effectiveness of the training method to increase performance in effortful tasks that engage EFs or self-control, the stability of these gains once training stops and the generalizability/transferability of these gains. The method used to select, extract information and evaluate for risk of bias in these meta-analyses is detailed in the **Supplementary Material**.

Interventions Targeting Executive Functions

A very large number of studies have examined the effectiveness of miscellaneous training methods on EFs. Four main categories of training methods can be distinguished: process-based cognitive training, physical training, video-game training, and mindfulness training. Process-based cognitive training aims to directly increase the efficiency of specific cognitive processes, such as core EFs, through extensive repeated practice of affectively neutral computerized and/or manual cognitive tasks tapping the targeted cognitive process. Physical training aims to improve higher cognitive functions, such as EFs and episodic memory, through the regular practice of aerobic, resistance and/or coordinative exercises. Video game training stimulates miscellaneous cognitive functions, such as hot EFs, through video games, exergames or serious games that generally involve motivationally salient contexts or simulated social contexts generating heightened emotion. Mindfulness training is the regular practice of exercises maintaining attention to the current situation while concurrently acknowledging any thoughts or feelings that arise in consciousness (Bishop et al., 2004).

Process-Based Cognitive Training Interventions

Table 1 summarizes the results of sixteen meta-analyses published from 2011 to 2021, which focused on the effect of process-based cognitive training on EFs (near-transfer effects) and other far-transfer outcomes. Strategy-based training methods were not taken into consideration because they focus more heavily on compensatory rather than restorative methods, bypassing deficient cognitive processes and teaching alternative approaches to achieving goals (Mowszowski et al., 2016). For instance, strategy-based training programs aiming to compensate for memory deficits typically include internal techniques (e.g., categorizing or visualizing information to be remembered, encoding through multiple sensory channels) and external techniques (e.g., using environmental cues, calendars or memory notebooks).

The methods used to calculate the effect sizes varied greatly across meta-analyses. The most commonly used methods were Cohen's *d* (Cohen, 1988) and Hedge's *g* (Hedges, 1981), but alternative methods to calculate standardized mean difference (SMD) have also been used (e.g., Morris, 2008).

The 16 meta-analyses included in **Table 1** principally targeted three populations: children, adolescents and older adults. Ten out of 16 meta-analyses showed a significant and small to moderate effect of process-based cognitive training on near-transfer outcomes (i.e., performance in tasks different from trained tasks but tapping the same cool EFs). By contrast, only four meta-analyses reported a significant effect of process-based cognitive training on far-transfer outcomes (Rapport et al., 2013; Nguyen et al., 2019; Basak et al., 2020; Scionti et al., 2020). However, several categories of far-transfer outcomes must be distinguished. Performance in tasks tapping untrained EFs belongs to the first category of far-transfer outcomes, for instance, the effect of a working-memory training program using n-back tasks on inhibitory control assessed with a Stroop task. Performance in academic or everyday functioning tasks belongs to the second category of far-transfer outcomes (e.g., literacy tasks, calculation tasks). Performance in emotional and social self-regulation tasks (i.e., hot executive functions) belongs to the third category of far-transfer outcomes. Finally, blinded or unblinded subjective ratings of problem behaviors (e.g., inattention, hyperactivity, quick-temperedness and disruptiveness) by a relative, a teacher or a caregiver belong to the fourth category of far-transfer outcomes.

Rapport et al. (2013) showed that programs designed to train working memory, EFs, and attention in children with attention-deficit hyperactivity disorder (ADHD) lead to significant, small magnitude improvements in the first category of outcomes, but non-significant changes for the second and fourth categories of outcomes (i.e., academic achievement measures and blinded behavior ratings, respectively). In the same way, the meta-analysis conducted by Scionti et al. (2020) in preschool children showed that process-based cognitive training programs lead to significant far-transfer benefits in the first category, but not to outcomes belonging to the three other categories. The meta-analysis of Nguyen et al. (2019) focused on far-transfer effects in the first category only and confirmed that these gains can be observed in older adults. Finally, the meta-analysis conducted by Basak et al. (2020) in older adults showed overall significant net gains of process-based cognitive training versus the control conditions on everyday functional outcomes, but these gains were obtained through training programs targeting processing speed.

To sum-up, process-based cognitive training successfully improve EFs with a small to moderate effect size on near-transfer outcomes. However, they generally fail to induce far-transfer outcomes, such as performance in everyday tasks involving EFs or self-control. This last result suggests that process-based cognitive training methods induce gains in cognition that are not sufficiently generalizable and transferable to train willpower.

Physical Training Interventions

Table 2 summarizes the results of 28 meta-analyses published during the period 2003–2021, which reported the effect sizes of chronic exercise on EFs. These meta-analyses targeted children and adolescents (7 meta-analyses), young and middle-aged adults (7 meta-analyses), and older adults (14 meta-analyses). Seven meta-analyses focused on symptomatic populations (AD, ADHD, chronic brain disorders, and MCI). A large majority of meta-analyses (26 out of 28) showed a significant effect of exercise training on EFs. Among the four meta-analyses with the highest quality score (Karr et al., 2014; Alvarez-Bueno et al., 2017; Biazus-Sehn et al., 2020; Ludyga et al., 2020; $M = 13.75/16$; SD = 0.5), three clearly showed a significant effect of exercise training on EFs. None of these meta-analyses examined the effect of exercise interventions on other secondary effortful control domains.

Two meta-analyses focusing on the effect of interventional studies combining physical and process-based cognitive training on EFs were selected for the present systematic review (Zhu et al., 2016; Guo et al., 2020; see **Supplementary Table 3.1**). Both of them showed a significant but small effect of these combined interventions on EFs.

Video Game Training Interventions

Three meta-analyses examining the effect of video game training on EFs (Stanmore et al., 2017; Mura et al., 2018; Mansor et al., 2020) have been selected for the present systematic review. The meta-analysis of Stanmore et al. (2017) reported the results of 17 studies conducted in adults ranging from 17 to 85 years of age. These authors observed a significant effect of exergames on global EFs ($g = 0.256$, 13 studies), cognitive flexibility ($g = 0.348$, 8 studies), and inhibitory control ($g = 0.90$, 5 studies), but a non-significant effect on working memory (4 studies) and problem solving (3 studies). The meta-analysis of Mura et al. (2018) reported the results of 13 intervention studies in persons suffering from neurological disabilities (multiple sclerosis, poststroke hemiparesis, Parkinson's disease, dementia, dyslexia, and Down syndrome). They showed a significant and positive effect of exergames on EFs (SMD = 0.53, eight studies) but not on attention (seven studies). The meta-analysis

TABLE 1 | Meta-analyses reporting effect sizes of process-based cognitive training on executive functions and other far-transfer outcomes.

References	Trained functions	NO studies (A/B)	Population	Duration of interventions	Results
Karch et al., 2013	Attention, executive functions, long-term memory, visuospatial abilities, working memory	NT: 11/22 FT: 4/22	Children and adolescents (4–20 years)	4–15 weeks *M* = 8.7 weeks	NT: *d* = 0.17 ns FT: *d* = 0.29 ns
Rapport et al., 2013†	Attention, executive functions	NT: 3/17 FT: 9/17	Children and adolescents with ADHD	2–16 weeks *M* = 7.3 weeks	NT: *d* = 0.06 ns FT: *d* = 0.28*
Lampit et al., 2014	Attention, multidomain, processing speed, video game, working memory	29/51	Healthy older adults (≥60 years)	2.5–16 weeks *M* = 7.4 weeks	*g* = 0.09 ns
Cortese et al., 2015	Attention, executive functions, memory, multidomain, working memory	EFR: 6/16 WMvi: 5/16 WMve: 8/16 IC: 6/16	Children and adolescents with ADHD (3–18 years)	4–20 weeks *M* = 7.5 weeks	EFR: SMD = 0.35* WMvi: SMD = 0.47* WMve: SMD = 0.52* IC: SMD = 0.07 ns
Lawrence et al., 2017	Attention, executive functions, memory, psychomotor speed, visuospatial abilities, working memory	8/11	Older adults with Parkinson's disease	1–7 weeks *M* = 4.7 weeks	*g* = 0.42*
Sherman et al., 2017†	Memory, multidomain, processing speed, strategy-based training, working memory	13/26	Older adults with MCI (mean age = 72.6 years)	2–24 weeks *M* = 12.1 weeks	*g* = 0.575*
Soveri et al., 2017	Updating of working memory	33/33	Young, middle-aged and older adults (18–84 years)	1–15 h *M* = 6.4 h	N-back: *g* = 0.62* WM: *g* = 0.24* CC: *g* = 0.16* Gf: *g* = 0.16*
Webb et al., 2018	Attention, multidomain, processing speed, video game, working memory	EF: 29/51 UWM: 7/51 CF: 22/51 IC: 19/51	Healthy older adults (≥60 years)	2–16 weeks *M* = 7.5 weeks	EF: *g* = 0.17* UWM: *g* = 0.005 ns CF: *g* = 0.17* IC: *g* = 0.16*
Lampit et al., 2019†	Attention, executive functions, processing speed, memory	14/20	Middle-aged adults with multiple sclerosis (mean age = 46.9 years)	4–12 weeks *M* = 8.2 weeks	*g* = 0.29*
Nguyen et al., 2019†	Executive functions, working memory	TO: 24/64 NT: 55/64 FT: 57/64	Healthy older adults (53–95 years)	1–27 weeks *M* = 7.0 weeks	TO: *g* = 1.00* NT: *g* = 0.26* FT: *g* = 0.22*

(Continued)

TABLE 1 | (Continued)

References	Trained functions	NO studies (A/B)	Population	Duration of interventions	Results
Takacs and Kassai, 2019†	Attention, executive functions, long-term memory, reasoning, working memory	WM: 34/90 IC: 31/90 CF: 20/90	Children (≤12 years)	1–12 weeks *M* = 5.4 weeks	WM: $g = 0.451$* IC: $g = 0.213$* CF: $g = 0.31$*
Zhang et al., 2019	Attention, long-term memory, multidomain, processing speed, working memory	11/18	Older adults with MCI (mean age = 73.4 years)	2–26 weeks *M* = 10.5 weeks	$g = 0.20$ ns
Basak et al., 2020	Executive functions, memory, multidomain, processing speed, reasoning	MCI: 33/54 HA: 116/161 NT: 41/215 FT: 38/215 AO: 8/215	Older adults with or without MCI (≥60 years)	0.5–270 h *M* = 23.3 h 1–90 weeks *M* = 8.3 weeks	MCI: $g = 0.29$* HA: $g = 0.27$* NT: $g = 0.44$* FT: $g = 0.31$* AO: $g = 0.18$ ns
Pauli Pott et al., 2020	Inhibitory control, cognitive flexibility, working memory	WM: 23/35 cool IC: 26/35 hot IC: 4/35 CF: 12/35	ADHD children (mean age = 5.0 years)	1–52 weeks *M* = 11.3 weeks	WM: $d = 0.46$* cool IC: $d = 0.30$* hot IC: $d = 0.33$* CF: $d = 0.47$*
Scionti et al., 2020	Executive functions, reasoning, working memory	NT: 30/32 FT: 16/32 AO: 13/32	Children (3–6 years)	2.5–54.8 h *M* = 11.4 h	NT: $g = 0.352$* FT: $g = 0.318$* AO: $g = 0.10$ ns
Nguyen et al., 2021	Commercial multidomain cognitive training programs	25/43	Healthy older adults (mean age = 70.6 years)	6.7–80 h *M* = 18.3 h 2–16 weeks *M* = 7.4 weeks	$g = 0.19$*

**Significant effect. †The meta-analysis calculated effect sizes for follow-up data. The third column expresses the ratio A/B. The denominator B designates the total number of studies included in the meta-analysis whereas the numerator A designates the number of intervention studies including at least one measurement of executive functions that was used to compute the effect size concerning executive functions. The range and average of intervention durations have been calculated exclusively from studies aiming to train EFs. ADHD, attention-deficit hyperactivity disorder; AO, additional outcomes; CC, cognitive control; CF, cognitive flexibility; CT vs. AC, cognitive training versus active control; CT vs. NI, cognitive training vs. no intervention; EA, executive attention; EF, executive function; EFR, executive function rating; FT, far-transfer effect; Gf, fluid intelligence; HA, healthy aging; IC, inhibitory control; MCI, mild cognitive impairment; NT, near-transfer effect; SMD, standardized mean difference; TO, trained outcomes; UWM, updating of working memory; WM, working memory; WMve, verbal working memory; WMvi, visual working memory; ns, non-significant effect.*

conducted by Mansor et al. (2020) included 27 intervention studies and examined the effect of video game training on EFs in older adults. Video game training had no significant effects on attention (8 studies), reasoning (10 studies), cognitive flexibility (15 studies), and inhibitory control (15 studies). By contrast, video game training led to a significant and moderate effect on working memory updating (g = 0.37, 19 studies). The duration of video game interventions ranged from 2 to 24 weeks, with an average of 9.4 weeks for the three meta-analyses. The three meta-analyses did not report any other far-transfer outcomes. **Supplementary Table 3.2** describes the main characteristics of these three meta-analyses.

Mindfulness Training Interventions

Finally, eight meta-analyses including randomized controlled studies reporting mean effect sizes of mindfulness training interventions on EFs have been selected in the present systematic review (Chan et al., 2019; Dunning et al., 2019; Cásedas et al., 2020; Poissant et al., 2020; Im et al., 2021; Millett et al., 2021; Verhaeghen, 2021; Yakobi et al., 2021). The characteristics of these meta-analyses are detailed on **Supplementary Table 3.3**. Two meta-analyses focused on specific populations: the meta-analysis of Chan et al. (2019) on older adults and the meta-analysis of Dunning et al. (2019) on children and adolescents. All the six other meta-analyses mainly concerned young and middle-aged adults. Seven out of these eight meta-analyses showed a significant and small to moderate effect of mindfulness training on EFs (Dunning et al., 2019; Cásedas et al., 2020; Poissant et al., 2020; Im et al., 2021; Millett et al., 2021; Verhaeghen, 2021; Yakobi et al., 2021). The eight meta-analyses shared 31.6% of duplicates. Mindfulness-based programs reported in these meta-analyses were in average shorter than exercise training programs listed in **Table 2** (6.6 weeks vs. 23.5 weeks, respectively), but as exercise training programs they provide additional benefits on mental health and well-being (e.g., reduction of anxiety, depression and reactivity to stress).

Interventions Targeting Self-Control

A few interventions have explored the beneficial effects of self-control training on self-control capacity. Self-control interventions do not focus specifically on inhibitory control but generally use a large variety of training tasks involving one or several spheres of self-control described by Hagger et al. (2010), such as volition and social processing. Four meta-analyses examined the effects of self-control training in young adults (Hagger et al., 2010; Inzlicht and Berkman, 2015; Friese et al., 2017; Beames et al., 2018). These meta-analyses have included 33 intervention studies, 11 of which are unpublished. Two other meta-analyses focused on children and adolescents (Piquero et al., 2016; Pandey et al., 2018). Together, they included 41 intervention studies, seven of which were in common and 16% were unpublished studies. All these meta-analyses showed a significant effect of training on self-control capacity. The mean effect size ranged from small (d = 0.17, Inzlicht and Berkman, 2015) to large (d = 1.07, Hagger et al., 2010) in young adults and was moderate for children and adolescents (d = 0.32, Piquero et al., 2016; d = 0.42, Pandey et al., 2018). Interestingly, Friese et al. (2017) showed that training effects were significantly larger when the task showing the training effect was preceded by a depleting effortful task (g = 0.60) rather than when it was not (g = 0.21). This last result suggests that benefits from self-control training are more pronounced for the capacity to maintain effortful control over time (i.e., stamina or resistance to cognitive fatigue) rather than the capacity to exert strong effortful control during a short period of time (i.e., strength of effortful control).

In young adults, the interventions included a large variety of training tasks, such as using a non-dominant hand, maintaining good posture, avoiding sweets, performing inhibitory control tasks (e.g., Stroop task) or practicing physical exercises. In preschool and kindergarten children, half of the interventions used a curriculum-based approach implemented in classrooms including circle-time games, storytelling, book reading, and self-talk. In preadolescents and adolescents, the training strategies mainly included activities such as role-playing, cognitive modeling, psychoeducational group therapeutic lessons, physical exercises, and mindfulness and/or yoga exercises. Nevertheless, the amount of effortful control required by this large diversity of activities is rarely assessed.

Regarding the transferability of gains in self-control, intervention studies with children and adolescents showed a main positive effect on far-transfer outcomes, such as academic achievement, mental health, social skills, frequency of school suspensions, and educational attainment, but a weaker effect on substance abuse when comparing the treatment group with the control group. In young adults, the effect of self-control training on far-transfer outcomes was not conclusive. The two most recent meta-analyses showed contradictory results. The meta-analysis of Beames et al. (2018) found that the effect sizes for health and well-being outcomes were small-to-medium and significantly different from zero whereas the meta-analysis of Friese et al. (2017) failed to show significant effects for the same outcomes.

What Did We Learn From These Meta-Analytic Reviews?

In the present umbrella review of meta-analytic reviews, we analyzed the results from 63 meta-analyses interested in the effect of miscellaneous interventions aiming to durably improve EFs and self-control efficiency. A large majority of these meta-analyses (i.e., 79.37%, 50/63) showed that training programs are effective in improving performance in tasks tapping EFs and/or self-control with a small to large effect size. The transferability of these gains is more nuanced. Process-based and video game interventions failed to show far-transfer effects on academic or everyday functioning tasks. By contrast, self-control interventions seem more effective in producing far-transfer gains in other domains of self-control than trained domain. Intervention studies based on physical training listed in **Table 2** and those based on mindfulness exercises rarely assess secondary outcomes, such as performance in academic or everyday functioning tasks. Consequently, it is difficult to assess the generalizability of these two types of interventions in the different domains of self-control. However, training effortful control through physical exercises or mindfulness exercises and observing gains in EFs could be considered far-transfer effects.

TABLE 2 | Meta-analyses reporting an effect of chronic exercise on executive functions.

References	Type of intervention	NO studies (A/B)	NO effects	Duration of interventions	Population	Results
Colcombe and Kramer, 2003	Exercise training	18/18	37	8–144 weeks *M* = 25.3 weeks	Older adults (≥55 years)	*g* = 0.68*
Smith et al., 2010	Exercise training	19/29	19	6–72 weeks *M* = 23.7 weeks	Young and middle-aged adults (≥18 years)	*g* = 0.123*
Hindin and Zelinski, 2012	Extended cognitive training and Aerobic training	17/42	90	8–144 weeks *M* = 29.3 weeks	Older adults (≥55 years)	*d* = 0.459*
Karr et al., 2014	Exercise training	EA: 13/22 PS: 5/22 WM: 8/22 IC: 11/22 VF: 8/22	EA: 20 PS: 6 WM: 14 IC: 17 VF: 11	4–52 weeks *M* = 22.2 weeks	Older adults (≥65 years)	EA: *d* = 0.15* PS: *d* = 0.12 ns WM: *d* = 0.13 ns IC: *d* = 0.06 ns VF: *d* = 0.12 ns
Jackson et al., 2016	Exercise training	8/8	8	8–52 weeks *M* = 27.8 weeks	Children (6–12 years) *M* = 9.4 years	*d* = 0.20*
Alvarez-Bueno et al., 2017	Exercise training	24/36	42	1.5–54 weeks *M* = 22.9 weeks	Children and adolescents (4–18 years)	*d* = 0.20*
Barha et al., 2017	Aerobic training: AT Resistance training: RT Multimodal training: MT	AT: 14/39 RT: 7/39 MT: 11/39	AT: 44 RT: 34 MT: 26	8–96 weeks *M* = 28.8 weeks	Middle-aged adults (≥45 years)	AT: *g* = 2.064* RT: *g* = 0.639* MT: *g* = 0.494*
de Greeff et al., 2018	Exercise training	12/31	15	6–36 weeks *M* = 22.7 weeks	Children (6–12 years)	*g* = 0.24*
Northey et al., 2018	Exercise training	36/39	94	6–52 weeks *M* = 24.5 weeks	Older adults (≥50 years)	SMD = 0.34*
Zhang et al., 2018	Mind-body training	11/19	40	8–40 weeks *M* = 20.2 weeks	Older adults (≥60 years)	0.25 ≤ *g* ≤ 0.65*
Landrigan et al., 2020	Resistance training	16/24	16	4–96 weeks *M* = 28.3 weeks	Young and middle-aged adults (≥18 years)	SMD = 0.39*
Falck et al., 2019	Exercise training	40/47	174	8–104 weeks *M* = 25.1 weeks	Older adults (≥60 years)	*g* = 0.19*
Sanders et al., 2019	Exercise training	22/36	39	4–52 weeks *M* = 24.1 weeks	Young and middle-aged adults with and without MCI (≥18 years)	*d* = 0.25*
Takacs and Kassai, 2019	Exercise training	21/22	22	6–44 weeks *M* = 18.5 weeks	Children (4–12 years)	*g* = 0.16*

(Continued)

TABLE 2 | (Continued)

References	Type of intervention	NO studies (A/B)	NO effects	Duration of interventions	Population	Results
Wu et al., 2019	Exercise training	17/32	CF: 13 WM: 10	7–48 weeks *M* = 21.4 weeks	Older adults (*M* = 71.1 years)	CF: MD = 8.80* WM: MD = 0.32*
Xue et al., 2019	Exercise training	18/19	33	5–54 weeks *M* = 24.7 weeks	Children and adolescents (6–17 years)	SMD = 0.20*
Zou et al., 2019	Mind-body training	8/12	9	8–52 weeks *M* = 22.6 weeks	Older adults (≥50 years)	SMD = 0.42*
Biazus-Sehn et al., 2020	Exercise training	15/27	19	6–52 weeks *M* = 24.3 weeks	Older adults with MCI (Mean age = 72.5 years)	SMD = 0.213*
Cai et al., 2020	Taijiquan training	9/19	18	10–52 weeks *M* = 32.6 weeks	Older adults with MCI (Mean age = 71.6 years)	SMD = 0.33*
Chen et al., 2020	Exercise training	33/33	107	4–52 weeks *M* = 25.7 weeks	Older adults (≥50 years)	*g* = 0.21*
Liu et al., 2020	Exercise training	22/22	IC: 15 WM: 14 CF: 8	8–24 weeks *M* = 13.5 weeks	Children and adolescents (5–15 years)	IC: SMD = 0.30* WM: 0.54* CF: SMD = 0.34*
Ludyga et al., 2020	Exercise training	68/80	80	4–52 weeks *M* = 21.4 weeks	Middle-aged and older adults *M* = 47.9 years	*g* = 0.164*
Zhu et al., 2020	Exercise training	12/16	12	12–48 weeks *M* = 20.0 weeks	Older adults with AD (*M* = 76.7 years)	SMD = 0.42*
Dauwan et al., 2021	Exercise training	14/36	14	4–52 weeks *M* = 20.5 weeks	Middle-aged adults with chronic brain disorders (*M* = 55.1 years)	*g* = 0.151*
Huang et al., 2021	Exercise training	26/71	26	6–93 weeks *M* = 26.1 weeks	Older adults with MCI or AD (*M* = 74.3 years)	SMD = 0.39*
Ren et al., 2021	Mind-body training	29/29	29	4–52 weeks *M* = 20.4 weeks	Middle-aged and older adults (*M* = 67.5 years)	SMD = 0.28*
Welsch et al., 2021	Exercise training	9/12	9	8–78 weeks *M* = 17.3 weeks	Children with ADHD (*M* = 9.7 years)	SMD = 0.57 ns
Xiong et al., 2021	Exercise training	25/25	WM: 19 CF: 15 IC: 15	4–56 weeks *M* = 25.4 weeks	Older adults (*M* = 69.9 years)	WM: *g* = 0.127* CF: *g* = 0.511* IC: *g* = 0.136*

**Significant effect. The third column expresses the ratio A/B. The denominator B designates the total number of studies included in the meta-analysis whereas the numerator A designates the number of intervention studies including at least one measurement of executive functions that was used to compute the effect size concerning executive functions. The meta-analysis of Hindin and Zelinski includes 25 extended process-based cognitive training programs and 17 aerobic exercise programs. AD, Alzheimer's disease; AUS, autism spectrum disorder; EA, executive attention; IC, inhibitory control; MCI, mild cognitive impairment; NO studies, Number of studies included in the calculation of effect size for executive functions/Total number of studies included in the meta-analysis. PS, problem solving; VF, verbal fluency; WM, working memory; SMD, standardized mean difference.*

The interventions listed in the 63 meta-analyses mainly focused on children, adolescents and older adults, with the exception of mindfulness-based interventions. These three populations share a common characteristic: their EFs undergo drastic and quick changes in efficiency. Indeed, EFs are still developing in children and adolescents (De Luca et al., 2003; Blakemore and Choudhury, 2006) and declining in older adults (Spreng et al., 2017). Consequently, these populations situated at the two extremes of the lifespan are likely more sensitive to the effects of moderators, such as training and chronic stress, which improve or impair these high-level cognitive functions, respectively. For that reason, researchers should focus on these three populations when examining the effects of training on EFs and effortful control, because they would increase the likelihood to observe a significant effect.

For the same reason, it would be very interesting to examine the sensitivity to training for different symptomatic populations suffering from a recurrent mental fatigue (e.g., fragile older adults, multiple sclerosis patients, traumatic brain injured people or cancer patients treated with chemotherapy) or having a low dispositional capacity to exert effortful control (e.g., individuals with addictions, depression, obsessive-compulsive disorder or attention-deficit hyperactivity disorder). Few intervention studies targeting effortful control have been conducted in these populations.

If gains in EFs and self-control through training programs are based on durable changes taking place within large-scale networks, we can hypothesize that the stability of improvement in EFs and/or self-control over time could be an important index of training success. Consequently, intervention studies assessing near- and far-transfer effects in several follow-up assessments after program cessation are very good arguments for real durable changes.

Process-based cognitive interventions reported follow-up measurements in only 26.4% of the studies, whereas self-control, physical exercise, video game and mindfulness interventions rarely reported this type of information. The duration between the postintervention and the follow-up varied greatly among the studies reporting a follow-up: from 3 weeks to 10 years. When reported, effects on follow-up outcomes were significant with small to moderate size (Rapport et al., 2013; Nguyen et al., 2019; Takacs and Kassai, 2019), or non-significant (Lampit et al., 2019). However, several confounding factors, such as regular effortful activities practiced by participants in continuation of the training program or completely independent of the training program (e.g., playing chess outside of engaging with an aerobic exercise program), can moderate the outcomes associated with self-control and EF efficiency that are measured at follow-up, and these must be more rigorously controlled for in the future.

The quality of the 63 selected meta-analyses (see section S5 in **Supplementary Material**) is globally low. According to the AMSTAR2 risk of bias assessment scale (Shea et al., 2017), 54 meta-analyses (85.71%) are of critically low quality (M = 10.48/16; SD = 2.17), i.e., present more than one critical weakness. The three more frequent critical flaws are: (a) not providing a list of excluded studies with reasons of exclusion (87.30%), (b) not pre-registering the review methods prior to the conduct of the review (71.43%) and (c) not accounting for risk of bias in individual studies when discussing the results of the review (65.08%). Future meta-analyses on this topic will have to address these issues. However, a majority of the selected meta-analyses used a satisfactory technique for assessing the risk of bias in individual studies (83.13%), provided a satisfactory explanation for the heterogeneity observed in the results of the review (79.37%) and carried out an adequate investigation of publication bias with a discussion of its likely impact on the results of the review (77.78%).

This section clearly shows that all the above-mentioned training methods allow improving EFs and strengthening self-control. The generalizability of these gains seems more evident and robust in self-control training interventions. Which mechanisms can explain these gains and their transferability? The aim of the next section is to propose plausible and rational neurobiological mechanisms to explain the effects of effortful control training. A recent meta-analysis on the topic mentions that the mechanisms underlying these effects are poorly understood (Friese et al., 2017).

NEURAL BASES OF GAINS IN EFFORTFUL CONTROL CAPACITY THROUGH TRAINING

The aim of this section is to clarify the neurophysiological mechanisms underpinning the improvements in effortful control capacity through training programs. We assume that the improvements in the capacity to exert effortful control results in learning processes based on long-term synaptic plasticity, which take place in specific regions of the central nervous system involved in the engagement of effortful control. The description of these mechanisms requires the use of a theoretical framework proposing several neuronal networks as possible targets of these durable changes in activity and/or connectivity with training. We will use the integrative model of effortful control proposed by André et al. (2019) as a model of reference.

According to this model, effortful control is a top-down oscillatory control signal generated by a large functional neuronal network called the salience network (Seeley et al., 2007; Seeley, 2019). Converging empirical evidence from neuroscience suggests that different brain structures involved in the salience network, such as the dorsal anterior cingulate cortex, integrate costs and benefits associated with the achievement of the ongoing task to make decisions about the amount of effortful control dedicated to this task (e.g., Kennerley et al., 2006; Shenhav et al., 2013, 2017; Klein-Flügge et al., 2016).

On the one hand, benefits are the immediate or delayed positive consequences associated with the achievement of the task goal. They include all types of rewards (e.g., food, money, pleasure, social rank). On the other hand, costs are associated with the detrimental consequences an individual has to cope with while attempting to achieve an intended goal, such as expending limited resources or feeling pain. They depend on task constraints (i.e., the higher the constraints are, the higher the costs are) and participant characteristics (i.e., the lower the capacity to exert effortful control is, the higher the cost of effort). They include different categories of costs that are detailed hereafter and summarized in **Table 3**.

André et al. (2019) distinguished metabolic or energetic costs (e.g., muscular and brain glucose necessary to reach the task goal) and computational costs (e.g., number of effort-dedicated processing units devoted to the task). However, three other main categories of cost computed by different cortical areas have been described in neuroscience (see **Figure 1**).

The first and certainly most studied category includes costs related to the physical activity necessary to achieve the task goal. These motor costs encompass energetic costs associated with energy expenses made by the muscles (i.e., intensity of muscle contraction) as well as computational costs associated with the complexity of the movement (e.g., number of motor units involved, complexity of the coordination timing between these motor units). Several fMRI and transcranial magnetic stimulation (TMS) studies conducted in humans have suggested that the supplementary motor area (SMA) is involved in the coding of these motor costs (Pessiglione et al., 2007; Kurniawan et al., 2010; Burke et al., 2013; Zénon et al., 2015; Bonnelle et al., 2016).

The second category of costs is related to the degree of engagement of brain regions subserving EFs, such as working memory updating, inhibitory control and planning (Duncan and Owen, 2000; McGuire and Botvinick, 2010; Baumgartner et al., 2011). These executive control costs encompass energetic costs (i.e., brain glucose expended by each processing unit involved in executive control) and computational costs (i.e., number of processing units allocated to task performance relative to the limited number of available processing units). The dorsolateral prefrontal cortex (DLPFC), which lies in the middle frontal gyrus, is an important hub in the executive control network (Menon, 2011) and its activity is associated with the executive control costs. For instance, several fMRI and functional near-infrared spectroscopy (fNIRS) studies have shown that activation in the left DLPFC scales linearly with working memory load (Barch et al., 1997; Braver et al., 1997; Jansma et al., 2000; Veltman et al., 2003; Fishburn et al., 2014; McKendrick and Harwood, 2019), indicating load-dependent recruitment of the DLPFC. In addition, transcranial direct current stimulation of the left DLPFC, which facilitates neural activity within this cortical area, reduces the cost of performing a cognitive task on gait and postural control (Zhou et al., 2014). Finally, a more recent study showed that executive control costs are anticipated by the DLPFC (Vassena et al., 2019). Other cortical areas, such as the ventrolateral prefrontal cortex (VLPFC), which is located in the inferior frontal gyrus and ensures inhibitory control (Aron, 2007; Berkman et al., 2009; Aron et al., 2014a,b), could also participate to the computation in executive control costs.

The third and last category of costs includes both risk- and pain-related costs. Three types of risk-related costs have been identified: (1) the risk of not obtaining a reward, (2) the risk of losing an already obtained reward, and (3) the risk of experiencing negative consequences while obtaining a reward. A large number of studies have shown that the anterior insula computes these three types of risk (Burke and Tobler, 2011; Burke et al., 2013; for a meta-analysis, Mohr et al., 2010). This brain region is also involved in the subjective value of pain in effort-based decision-making (Talmi et al., 2009).

In this cost-benefit effort-based decision-making framework, two main mechanisms can explain a durable improvement in the capacity to exert effortful control with training: (1) a durable decrease in the effort costs; and (2) a durable increase in the value of the benefit resulting from goal-directed activities that requires effortful control (i.e., effort valuing). In the next subsections, we more precisely describe the two mechanisms that may underpin gains in effortful control capacity through exercise as well as mindfulness and self-control training.

Durable Reductions in Effort Costs Through Physical Training

According to the first mechanism, regularly practicing effortful exercises would lead to a progressive reduction in effort costs: that is, practice increases efficiency, and makes better performance possible with the same or less effort. Motor costs, executive control costs and pain-related costs are likely to decrease with physical training.

Reductions in effort costs are frequently observed in kinesiology and sport sciences with regard to physical effort. It is easy to understand this common phenomenon: individuals who take part in a physical training program that includes effortful exercises generally improve cardiorespiratory fitness as well as muscular strength, and they become increasingly efficient at practicing these exercises (Lin et al., 2015; Montero and Lundby, 2015; Lee and Stone, 2020). Consequently, the same exercise (i.e., same duration and same absolute intensity) requires more effort and energy at the beginning of the training program than at its end. Perceived exertion decreases with training (e.g., Farhat et al., 2015). In this way, sedentary or physically unfit people who start regular exercises progressively develop a higher tolerance for exercise and effort (e.g., Gomes-Neto et al., 2016). Symmetrically, people with a high cardiorespiratory fitness perceive a given absolute intensity of exercise as less effortful than people with a low cardiorespiratory fitness do (Eston and Brodie, 1986; Pfeiffer et al., 2002).

In addition, if the gain in effortful control acquired through physical training is transferable to the cognitive domain, it can be inferred that this gain in efficiency should be observed in the activation of brain areas involved in tasks tapping EFs. More precisely, a decrease of activation in brain areas involved in the salience and/or executive control networks should be observed after the end of the physical training program compared to before its beginning.

A set of six studies explored the effects of chronic exercise on gains in executive control and their brain activation correlates. The researchers used a flanker task (Colcombe et al., 2004; Voelcker-Rehage et al., 2011; Chaddock-Heyman et al., 2013; Krafft et al., 2014), an antisaccade task (Davis et al., 2011; Krafft et al., 2014), or an n-back task (Nishiguchi et al., 2015) during fMRI scans before and after the exercise program. Half of the studies involved children, and the other half involved older adults. The duration of exercise programs varied from 13 weeks to 12 months. Four studies showed a positive effect

TABLE 3 | The different categories of costs that influence effort-based decision making and determine the amount of effortful control dedicated to a task.

Category of cost	Short definition
Metabolic or energetic costs	Muscular and brain glucose expended to reach the task goal
Computational costs	Number of processing-units recruited to perform a specific task regarding the finite number of available processing units
Motor costs	Energetic and computational costs associated with the performance of a movement or a motor skill; they involve muscular and brain costs
Executive control costs	Energetic and computational costs associated with the performance of a task requiring executive control; i.e., related to the processing units devoted to executive control
Risk-related costs	Costs associated with the risk of not obtaining a reward, losing an already obtained reward, or experiencing negative consequences while obtaining a reward
Pain-related costs	Costs associated with the risk of experiencing pain while attempting to reach a goal
Opportunity costs	This term was introduced by Kurzban et al. (2013). It designates costs associated with the engagement of the effort system to perform an effort-demanding task that prevents to perform other effort-demanding tasks
Intrinsic costs	This term was introduced by Shenhav et al. (2017). It designates costs associated with the exertion of effortful control, a capacity-limited function; i.e., energetic and computational costs related to effort-dedicated processing units

of chronic exercise on behavioral performance, but two studies failed to find such an effect (Krafft et al., 2014; Nishiguchi et al., 2015). In contrast, all six studies showed a decrease in brain activity during the cognitive task at the end of the training program compared to the beginning, suggesting a higher efficiency in brain areas belonging to the salience network or the executive control network. These areas included the right dorsolateral prefrontal cortex (Voelcker-Rehage et al., 2011; Chaddock-Heyman et al., 2013; Nishiguchi et al., 2015), anterior cingulate cortex (Colcombe et al., 2004; Voelcker-Rehage et al., 2011; Krafft et al., 2014), posterior parietal cortex (Davis et al., 2011; Krafft et al., 2014), and right superior temporal gyrus (Voelcker-Rehage et al., 2011). These results suggested that physical training reduces the executive control costs associated with the performance of a cognitive task tapping EFs. In functional brain imagery, a decrease in BOLD response or blood flow in a specific brain region involved in the performance of the task and associated with a stable or better level of performance with repetition of the same task is generally interpreted as an increase in efficiency of the neuronal networks thanks to practice. In the present case, it would be a decrease in the need for top-down control and then a decrease in energetic cost associated with a lower top-down control. To our knowledge, no study examined the effect of chronic exercises on motor costs, i.e., BOLD fMRI variations, while performing a physical exercise before and after a physical training program, certainly because of the higher risk of head movement artifacts in the MRI scanner.

Durable Reductions in Effort Costs Through Extensive Practice of Motor and Cognitive Skills

A decrease in computational cost, also known as attentional cost, can be observed with learning through a process of automatization. When people repeatedly perform a motor skill or a cognitive task, they progressively reduce the computational cost of the activity. From this perspective, the acquisition of automaticity can be viewed as the gradual withdrawal of effortful control. A large number of studies using the dual-task protocol have supported the fact that throughout the process of motor skill acquisition, the involvement of effortful control (i.e., attentional control) decreases across training sessions or blocks of trials (e.g., Brown and Carr, 1989; Wulf et al., 2001; Goh et al., 2014). The tenet of these studies is that the lower the attentional cost of performing the primary task (i.e., the motor skill) while simultaneously carrying out the secondary task (i.e., a cognitive task tapping executive control) is, the higher the automaticity of the motor skill.

This reduction in computational cost can be explained within the framework of the integrative model of effortful control. As mentioned earlier, this model assumes that effort is a mechanism anchored in a large functional neuronal network called the salience network (Seeley, 2019). The 'mechanism of effort' includes a limited number of interconnected processing units that integrates information regarding the costs and benefits associated with the achievement of the task goal and generates the effort signal, which is a top-down control signal optimizing the information processing of miscellaneous brain regions involved in the task. These effort-dedicated processing units are assumed to be anchored in the cortical minicolumns belonging to several cortical areas in the salience network, such as the anterior cingulate cortex, frontal operculum and anterior insula.

A high engagement of effortful control in the initial phase of learning followed by a progressive decrease in the need for effortful control in later phases of learning should be observed at the level of effort-dedicated processing units. Two hypothetical complementary mechanisms can explain this reduction in effortful control with learning: (1) the recruitment of a lower number of effort-dedicated processing units to perform the task and/or (2) a higher efficiency of these processing units at exerting effortful control (i.e., strengthened connectivity within each processing unit). These two mechanisms should lead to a lower activation of brain regions belonging to the salience network, and other top-down control brain regions involved in the task, such as the executive control network, by the end of the acquisition phase. Overall, fMRI studies examining patterns of activation in brain areas during cognitive tasks support quite well the hypothesis of a decrease in energetic and/or computational costs following several weeks of a training program that could include cognitive tasks or motor skills.

A set of six intervention studies confirmed that process-based cognitive training and motor skill learning led to a decrease in activation in brain areas belonging to the salience and executive control networks. The authors of these studies asked their participants to practice the following tasks: a self-initiated, self-paced, memorized sequential finger motor task while performing a letter-counting task (Wu et al., 2004); a visual serial reaction time task while performing a tone-counting task (Poldrack et al., 2005); an emotion regulation task (Berkman et al., 2014); a stop-signal task involving motor response inhibition (Beauchamp et al., 2016); and an n-back task (Heinzel et al., 2016; Miró-Padilla et al., 2019). The training volume ranged from 60 min (Berkman et al., 2014; Beauchamp et al., 2016) to 540 min (Heinzel et al., 2016), and participants were mainly young adults except for one study that preferentially included older adults (Heinzel et al., 2016). The results of these six studies confirmed a decrease in BOLD activity in brain regions in the salience network (e.g., anterior cingulate cortex) but also in numerous other regions in the executive control network, such as the dorsolateral prefrontal cortex confirming a decrease in executive control cost with training.

Increase in Connectivity: A Biomarker of Efficiency

An increase in the efficiency of effort-based processing units reflecting task automatization should also be evidenced by an increase in connectivity within the salience network and/or between the salience network and other large-scale networks, such as the executive control network: the higher the between-network connectivity is, the lower the effort cost. As mentioned earlier, these changes in connectivity are generally observed by using resting-state fMRI coupled with a seed-based functional connectivity analysis.

We found five studies using this method that focused on the link between gains in automaticity or performance through process-based cognitive training and an increase in connectivity within and between top-down control networks. First, Mohr et al. (2015) showed that a higher connectivity between the salience network and the dorsal attention network correlated with practice-related efficiency gains. These authors also observed that short-term task automatization was accompanied by decreased activation in the executive control network, indicating a release of high-level cognitive control, and a segregation of the default mode network from task-related networks. Second, Chapman et al. (2015) conducted a 12-week gist reasoning training and observed that functional connectivity increased monotonically within the default mode and executive control networks, from pre-training to the end of training and from pretraining to midtraining, respectively, in the process-based cognitive training group relative to the control group. Third, Cao et al. (2016) examined training-related changes in functional connectivity within and between the default mode, executive control and salience networks 1 year after the training ended. In their experiment, healthy older adults were randomly included in a 3-month multidomain process-based cognitive training group or in a wait-list control group. The authors observed increased functional connectivity within the executive control network after training compared with the baseline. Fourth, Thompson et al. (2016) examined functional connectivity within and between the executive control and dorsal attention networks in young adults during task performance before and after 20 days of training on either a dual n-back working memory task or a demanding visuospatial attention task involving multiple object tracking. Learning selectively occurred in the n-back training group, who displayed marked gains on the trained task and not in the visuospatial attention training group. This n-back training induced significant increases in functional connectivity within and between the two networks. Fifth, Sánchez-Pérez et al. (2019) showed that a computer-based program aiming to train schoolchildren in cognitive tasks that mainly tap working memory leads to improvements in cognitive and academic skills compared with an active control group. They also found stronger relationships between inhibitory control scores and functional connectivity within the executive control network in trained children than in children from the control group.

In light of all the results presented in the two preceding sections, we can conclude that the hypothesis of a decrease in effort costs with training is plausible and supported by behavioral as well as activation and resting-state functional brain imaging data.

Durable Increases in Effort Valuing With Training

According to the second mechanism, prolonged experience in exerting effortful control would increase the value of a goal that required effort to be reached (e.g., Inzlicht et al., 2018): the higher the level of practice in effortful tasks is, the higher the expected benefit from any activity that requires effortful control. This hypothesis was initially formulated by Eisenberger in the framework of learned industriousness theory (Eisenberger et al., 1976; Eisenberger, 1992). This theory is based on the operant conditioning process (Skinner, 1938), a type of associative learning process through which the strength of a behavior is modified by a reinforcer. In operant conditioning, reinforcement occurs only after the organism intentionally executes a specified behavioral act. For instance, a child may learn to perform a chore without complaints to receive praise. From this perspective, animals and humans learn to engage in effortful tasks to maximize rewards. The learned industriousness theory views effort as a secondary reinforcer. If an organism learns that effortful tasks are consistently associated with greater rewards, the feeling of effort experienced during a task increases the expectation of a large reward once the task is performed.

Robert Eisenberger and his team conducted a series of intervention studies in animals and humans from the seventies to the nineties to demonstrate the soundness of this theory. The first experiment included a training program staggered over several days and was conducted with children (Eisenberger et al., 1985). In this experiment, 46 children were separated into three groups. Participants in the first group were paid for high effort in tasks

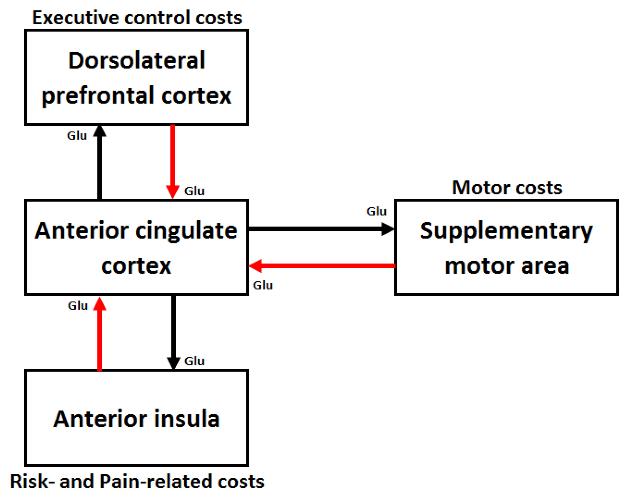

FIGURE 1 | Schematic illustration of cortical areas involved in effort costs computation. The supplementary motor area is involved in computation of motor costs, the dorsal lateral prefrontal cortex in executive control costs, and the anterior insula in risk- and pain-related costs. These three regions are interconnected to the anterior cingulate cortex (ACC) through glutamatergic (Glu) pathways. The red arrows represent the cost signals sent by these cortical areas to the ACC that integrates costs and benefits signals and makes decisions on how much effort deploying to achieve the task goal. The black arrows represent the control signal sent by the ACC to the brain areas computing the cost signals to enhance their capacity of processing.

involving object counting, picture memory, and shape matching, whereas participants in the second group were paid the same amount of money for a low-effort version of the same tasks. Participants in the third group did not undergo effort training. The training program for the first two groups included three training sessions given on consecutive days. Before and after the training program, all the participants made repeated choices between the tedious tasks of copying non-sense words for a large monetary reward versus waiting the equivalent duration for a small monetary reward. Before the intervention, the three groups did not differ in the number of times they chose to work for the larger reward. By contrast, after the intervention, the high-effort group chose the high-effort/high-reward alternative more frequently than did either the low-effort group or the control group, whereas the latter two groups did not significantly differ. Eisenberger and Adornetto (1986) replicated these results in a very similar experiment that manipulated the delay to the reward in addition to the effort required to obtain the reward. The results of these two studies clearly showed that repeatedly rewarding high levels of effort increases a person's generalized choice of high-effort large rewards over low-effort small rewards and may contribute to individual differences in the willingness to postpone gratification in pursuit of long-term goals.

In a third experiment, Eisenberger et al. (1989) replicated these results in animals and trained two groups of rats to run down a runway for food pellets in a low-effort or high-effort condition for 18 days. In the low-effort condition, the rats received one pellet for one trip during the entire training period, whereas in the high-effort condition, they received one pellet for one trip at the beginning of training and one pellet for five trips at the end of training. Two groups of rats were added as control groups and received the same number and temporal distribution of pellet presentations as in the two experimental groups but without the instrumental requirement (i.e., completion of a given number of round trips). At the end of the training program, the four groups of rats performed 12 choice test sessions the same day. They were tested by giving repeated choices of exerting low force on one lever for a small reward versus exerting high force on the alternative lever for a large reward. The results clearly showed that rats in the high-effort training group chose the high-effort, large-reward goal box more frequently than the three other groups. These results demonstrated that training animals in a rewarding high-effort task during several sessions increased the likelihood that these animals chose to exert a higher level of effortful control associated with a higher reward in a subsequent transfer task. A more recent study (Laurence et al., 2015) replicated these results in rats with a similar protocol but with a longer training program. For a period of 7 weeks, exercise rats were individually placed in a rodent running ball for five sessions per week (20 min/session). To our knowledge, this series of experiments initiated by Eisenberger constitutes the first elements of proof that repeatedly associating high effort with high reward during a training phase can transfer to other tasks and drive the trained individuals to choose more effortful tasks to increase the likelihood of gaining more benefits.

Where do the long-term synaptic changes underpinning the association between high effort and high reward take place in the brain? A series of experiments mainly conducted in rodents identified a set of four interconnected key structures allowing animals to overcome effort costs to obtain greater benefits. **Figure 2** illustrates the connections between these four structures: the anterior cingulate cortex (ACC), nucleus accumbens (NAC), basolateral amygdala (BLA), and ventral tegmental area (VTA).

John Salamone from the University of Connecticut and his collaborators took a first step in the comprehension of effort-based decision-making. In rodents, effort-based decision-making is typically assessed using tasks that offer animals a choice between a relatively preferred reinforcer (i.e., reward) that can only be obtained by a high exertion of effort versus a lower effort/lower value option (for reviews, see Assadi et al., 2009; Salamone et al., 2018). In the first experiment, working for a preferred food (i.e., high carbohydrate pellets) by lever pressing was the high-effort/high reward option, whereas simply approaching and consuming a less rewarding food (i.e., ordinary lab chow) was the low-effort/low-reward option (Cousins and Salamone, 1994). Rats typically pressed at high rates to obtain the preferred food and ate little of the lab chow; i.e., they preferentially chose the high-effort/high-reward option. However, dopamine depletions produced by injections of the neurotoxic agent 6-hydroxydopamine (6-OHDA) in the NAC produced a dramatic decrease in lever pressing and an increase in chow consumption (Cousins and Salamone, 1994).

These results have been replicated in a different experimental setup (Salamone et al., 1994; Cousins et al., 1996; Denk et al., 2005). Rats were trained on a T-maze task with one arm containing a large reinforcer (four pellets) associated with a

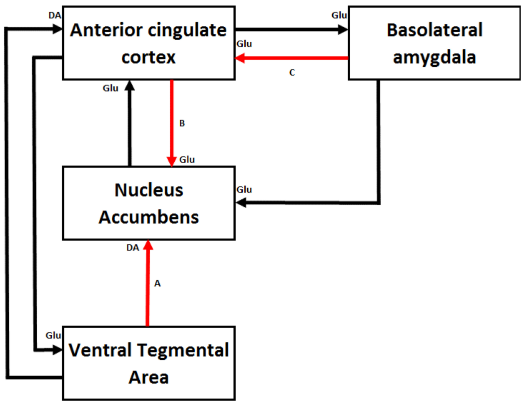

FIGURE 2 | Schematic illustration of the key structures and neurotransmitter pathways involved in effort-based decision-making in rodents and more particularly those that allow animals to overcome effort costs to obtain higher rewards. Pathway A connects the ventral tegmental area to the nucleus accumbens (NAC). Pathway B connects the anterior cingulate cortex (ACC) to the NAC. Pathway C connects the basolateral amygdala (BLA) to the ACC. Destruction of dopamine terminals in the NAC (Cousins and Salamone, 1994), lesions of the ACC (Walton et al., 2002) and bilateral inactivation of the BLA (Floresco and Ghods-Sharifi, 2007) impair effort-based decision-making and reduce the preference of animals to exert more effort to obtain a larger reward. These three structures clearly participate to a bias of behavior toward response options leading to larger rewards that come at larger costs but their respective contribution differ. In situations where an animal must choose between response options associated with differential magnitudes of reward, BLA neurons would encode the expected magnitude of reward that each choice may provide. This reward-related information would be relayed to the ACC *via* glutamatergic (Glu) projections. The ACC would bias behavior in a particular direction by integrating these reward-related signals with other information about response costs associated with each action. Then, the ACC would send the result of the decision-making to the NAC for an implementation of the appropriate behavioral output. Dopaminergic (DA) input from the ventral tegmental area to the NAC would be essential to energize appropriately the chosen instrumental activity in order to obtain the expected reward.

large vertical barrier (44 cm) and the other arm containing a small reinforcer (two pellets) associated with unobstructed access. Similar to previous experiments, in standard conditions, animals prioritized the high-effort/high-reward option, and this effect was reversed when 6-OHDA was injected into the NAC or when rats received injections of 0.1 mg/kg haloperidol, a dopamine antagonist. In other words, disruption of the dopaminergic pathway by drug treatment led rats to prefer the low effort/low reward option. These results showed that across a wide variety of tasks, administration of low doses of DA antagonists and NAC DA depletions have a detrimental effect on effort-based decision-making, producing a low-effort bias that shifts animals away from the high-effort option and toward the low-effort choice. Other authors obtained similar results with similar experimental setups and different dopamine receptor antagonists, such as flupenthixol (Floresco et al., 2008). A similar paradigm in which subjects choose between two options with different benefits and costs and a manipulation of dopamine availability has not yet been tested in primates or humans (Assadi et al., 2009).

Mark Walton from the University of Oxford and his collaborators used the same paradigm but targeted the ACC (Walton et al., 2002, 2003, 2009; Rudebeck et al., 2006, experiment 2). As Salamone and his team showed, all animals preferred to select the high-cost/high-reward option in the standard T-maze task. In these experiments, rats had to choose between a high effortful action (i.e., climbing a 30-cm barrier) to obtain a large quantity of reward (high-cost/high-reward) or a lower effortful action (i.e., climbing a 10-cm barrier) to obtain a smaller reward (low-cost/low-reward). However, after excitotoxic lesions of the ACC, rats selected the low-cost/low-reward response on nearly every trial. In contrast, both control animals and rats with prelimbic and infralimbic lesions continued to choose to climb the larger barrier for the larger reward. These results indicated that the ACC is an important region within the medial frontal cortex when evaluating how much effort to expend for a specific reward.

Stan Floresco from the University of British Columbia took a third step in the comprehension of brain mechanisms supporting effort-based decision-making (Floresco and Ghods-Sharifi, 2007). In their first experiment, they used exactly the same T-maze task as Walton and coworkers but focused on the role of the BLA in the effort-based decision-making process. They replicated the results in standard conditions and observed that bilateral inactivation of the BLA *via* infusion of the local anesthetic bupivacaine hydrochloride impaired decision-making by reducing the preference for the high-effort/high-reward arm.

From the above, we hypothesize that in animals and humans, the generalized bias toward high effort/large rewards resulting from effortful control training is inscribed within the circuitry described in **Figure 2**, and more specifically, in glutamatergic synapses connecting the BLA, ACC, NAC and VTA. To our knowledge, only one recent study conducted in humans with fMRI (Bernacer et al., 2019) showed that functional connectivity between the amygdala and ACC was strengthened after a 3-month fitness program (20–30 min sessions of walking and running on a treadmill, 2–3 days a week for 3 months).

The two preceding sections show that our field needs more theory-driven studies using animals as well as activation and resting-state fMRI in humans to determine precisely where when and how these durable changes in neural activity and connectivity occur. Some methodological suggestions in this direction will be made in the following section.

CHALLENGING THE TRAINABILITY OF EFFORTFUL CONTROL CAPACITY

The preceding sections provide arguments for a possible strengthening of effortful control capacity through the practice of effortful tasks. Then, two plausible mechanisms have been proposed to explain these gains in effortful control capacity. The aim of this last section is to address several theoretical and methodological issues to improve the effectiveness of training programs and comprehension of the mechanisms that underpin these gains in effortful control capacity.

The first issue concerns the choice of an appropriate protocol to prove and generalize a causal relationship between the regular practice of effortful tasks and durable improvements in effortful control capacity. The best way to eliminate bias that comes from confounders and demonstrate causality is to conduct randomized controlled trials (RCTs). In RCTs, study participants are randomly assigned to either receive the treatment or be in a control group (placebo). In the present case, the treatment group receives the training program aiming to improve effortful control capacity.

Proposing a control intervention that is as similar as possible to the treatment intervention with the exception that the level of effortful control differs across group activities is certainly the most difficult methodological issue to address in the context of an RCT protocol using human activities. An appropriate strategy could be to include two control groups: an active control group practicing activities requiring little effort (e.g., relaxation exercises, passive stretching exercises, massage and hydromassage sessions, watching emotionally neutral but interesting documentaries) and a passive control group that does not change its life habits during the period of the intervention. Fifteen out the 63 meta-analyses included in the present systematic review considered the type of control group as a moderator of the effect size of the intervention. Five out of these 15 meta-analyses showed that the effect size was significantly larger for studies that used a passive control group rather than an active one (Karr et al., 2014; Beames et al., 2018; Northey et al., 2018; Nguyen et al., 2019; Ren et al., 2021).

Regarding the treatment intervention, we recommend the use of effortful exercises (e.g., a combination of aerobic and resistance exercises) that stimulate brain plasticity (Fernandes et al., 2017; Walsh and Tschakovsky, 2018), in combination with cognitive tasks tapping EFs or mindfulness exercises. Physical exercises and cognitive tasks can be performed sequentially or simultaneously (team games or situational problem-solving tasks). The same is true for physical exercises and mindfulness exercises (e.g., yoga).

The second issue concerns the content of the treatment intervention program to generate transferable gains in effortful control capacity. In this perspective, the training program must be tailored, progressive and varied to optimize the likelihood of success in obtaining the desired effect. Tailoring the program means individualizing task difficulty and exercise intensity (e.g., difficulty expressed in percentage of individual's maximal capacity). The respect for this first principle ensures that there will be no large imbalances in perception of task-related constraints across participants, thereby resulting in quite similar levels of engagement. The second principle concerns the progressive increase in task difficulty and exercise intensity throughout the training program. This second principle allows the maintenance of a high level of participant engagement throughout the program. At last, it is important to vary training exercises to improve the generalizability/transferability of gains in the capacity to exert effortful control (Eisenberger et al., 1982) and reduce boredom.

The third issue concerns the choice of the outcomes that will assess the gain in effortful control capacity. These outcomes can be assessed at three levels of observation (i.e., subjective, behavioral, and physiological) and at different times of the intervention study (e.g., before and after the program). Behavioral indexes, such as the level of performance in a specific task, are valuable data that provide information about the level of engagement of the participant in the task and his/her skill level in this task. Experimenters need to choose tasks sensitive to practice effects with no risk of ceiling effects. The subjective measurements, such as effort required to perform the task and perceived fatigue at the end of the task, contribute to and facilitate the interpretation of results. Physiological indexes of effort engagement (i.e., effortful control), such as pupil size, pre-ejection period (PEP) and prefrontal theta power density, may contribute to the picture by adding objective measurements of effort costs and top-down control to cope with the task goals. All these indexes (subjective, behavioral, and physiological) are complementary and make their own contribution to understanding variations in outcomes as a function of the intervention. A large majority of RCTs selected in the reviewed meta-analyses did not use physiological indexes of effortful control.

In addition to the outcomes described previously, we recommend assessing at least three categories of transfer outcomes: (1) near-transfer outcomes such as performance in tasks tapping EFs and self-control (e.g., use of the sequential task protocol before and after the intervention); (2) far-transfer outcomes related to performance in everyday functioning tasks, such as academic performance; and (3) far-transfer outcomes concerning general self-regulation abilities, such as snacking, speeding, and periods of inattention. It could also be appropriate to have several follow-up assessments (or retention tests), e.g., 1 month, 3 months, and 6 months after the end of the training program, to show stability of the gains in effortful control capacity. Few interventional studies include follow-up measurements.

The fourth and last issue concerns the choice of an appropriate method that allows a better understanding of the durable changes in connectivity occurring within and between several large-scale neuronal networks involved in effortful tasks, such as the salience network, the executive control network, the default network and the mesolimbic network. In the future, resting-state and activation functional MRI techniques in conjunction with graph theory could be used before and after the training program to disentangle the role of these brain networks in the improvement to the capacity to exert effortful control. Only few interventional studies used functional MRI to assess network connectivity.

We are aware that the type of RCT described above is time and money consuming, but it is the best guarantee to demonstrate that this type of intervention is a plausible and possible way to train the effortful control capacity and explain which mechanisms underpin these durable gains. In addition, the gains provided by the identification of the determinants of the effectiveness of willpower training programs overcome the costs of the research leading to such scientific advances. As mentioned in the introduction, these gains in willpower can increase the likelihood of success, well-being and productivity of each individual in society.

CONCLUSION

The first question we addressed in this paper concerns the existence of empirical evidence that supports possible gains in effortful control capacity through training. In the second section "Improvements in Effortful Control with Practice: An Umbrella Review of Meta-Analytic Reviews," we provided clear evidence that executive control and effortful control can be improved through interventions using physical, cognitive or mindfulness exercises. However, we showed that the generalizability of these gains depends directly on the type of training interventions. In other words, people can definitely be trained to improve their executive functioning and self-control, but results have been inconsistent and variable as to how widely the improvements generalize to tasks different from those used in the training. Self-control training programs seem more effective than process-based training programs in inducing generalizability. Moreover, physical and mindfulness exercises seem to be two promising training methods that deserve to be included in self-control training programs. The higher effectiveness of self-control training programs in leading to generalizable gains most likely rests on the fact that these training programs include a greater variety of effortful tasks than process-based training programs.

The second question concerns the durable changes in brain structure and brain functioning that explain these increments in the capacity to exert effortful control. We pointed out two plausible brain mechanisms that can explain these gains in top-down control: (1) a decrease in effort costs combined with a greater efficiency of brain regions involved in the task and (2) a change in the value of effort through operant conditioning in the context of high effort and high reward. Our article shows that these two mechanisms have received clear empirical support from functional brain imaging studies in humans and neurophysiological studies in animals. The first mechanism is rather in favor of the hypothesis of the strengthening of the capacity to exert effortful control (i.e., more effortful control with less energy). By contrast, the second mechanism rather supports the motivational hypothesis: a durable predisposition to engage in effortful activities (i.e., an amplification of the benefit signal). Both mechanisms are certainly synergistic in contributing to how training improves effortful control. In addition, Bavelier and Green (2019) presented very interesting arguments suggesting that these two systems (i.e., the attentional/effortful control system and the reward system) foster learning and brain plasticity.

Based on the present literature review, what are the most pressing questions that would need further data collection on this topic in the near future? First, we need more resting-state electroencephalographic (EEG) and brain imaging studies examining the durable changes in connectivity, within and between large-scale neuronal networks, induced by training programs aiming to improve the capacity to exert effortful control. Three between-network connectivity hypotheses could be tested: (1) an increase of connectivity with training between the salience network and the executive control network supporting the strengthening hypothesis, (2) a decrease of connectivity with training between the executive control network and the default-mode network also supporting the strengthening hypothesis, and (3) an increase in connectivity between the mesolimbic reward network and the salience network supporting the motivational hypothesis. Second, we need to define more precisely the characteristics of the theory-based training programs that are more effective to strengthen the general capacity to exert effortful control, more particularly the most effective training exercises and the minimum volume of training needed to obtain significant gains according to the target population. Third, we need to know which theory-based behavioral change techniques are most effective at maintaining an effortful training program in the long-term.

Finally, if training programs are effective in strengthening effortful control capacity, citizens should be encouraged to practice and maintain engagement with such programs over the long term to continue developing these gains throughout their lives. In this way, health policies could promote the maintenance of a virtuous circle between healthy behaviors, including "willpower training" and the capacity to exert effortful control (for a description of this virtuous circle, see Audiffren and André, 2019). Based on this virtuous circle, training improves the capacity to exert effortful control and then a higher capacity to exert effortful control facilitates the maintenance of training and healthy behaviors.

AUTHOR CONTRIBUTIONS

MA, NA, and RB participated to the conceptualization of the theoretical ideas, writing of the article, and manuscript revision. MA and NA conducted the literature reviews. MA elaborated the tables and figures. All authors approved the submitted version.

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SUPPLEMENTARY MATERIAL

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Perception of effort and the allocation of physical resources: A generalization to upper-limb motor tasks

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Purpose: The perception of effort (PE) is widely used to prescribe and monitor exercise during locomotor and resistance tasks. The present study examines the validity of PE to prescribe and monitor exercise during upper-limb motor tasks under various loads and speed requirements.

Methods: Forty participants volunteered in two experiments. In experiment 1, we used four PE intensities to prescribe exercise on a modified version of the box and block test (BBT) and a pointing task. We investigated the possibility of monitoring the exercise intensity by tracking changes in PE rating in response to three different tempos or additional weights. Experiment 2 replicated the possibility of prescribing the exercise with the PE intensity during the BBT and explored the impact of additional weights on performance and PE during the standardized version of the BBT. Muscle activation, heart rate, and respiratory frequencies were recorded.

Results: In experiment 1, increasing the PE intensity to prescribe exercise induced an increased performance between each intensity. Increasing task difficulty with faster movement tempo and adding weight on the forearm increased the rating of PE. Experiment 2 replicated the possibility to use PE intensity for exercise prescription during the BBT. When completing the BBT with an additional weight on the forearm, participants maintained performance at the cost of a higher PE. In both experiments, changes in PE were associated with changes in muscle activation.

Conclusion: Our results suggest that PE is a valid tool to prescribe and monitor exercise during upper-limb motor tasks.

KEYWORDS

perceived exertion, upper-limb task, CR100 scale, motor control, psychophysiology, box and block test, pointing tasks

1. Introduction

The perception of effort, also known as perceived exertion or sense of effort (Marcora, 2010; Pageaux, 2016), can be described as "*the particular feeling of that energy being exerted,*" and "*is accompanied by a sensation of strain and labor, a feeling that intensifies the harder a person tries*" (Preston and Wegner, 2009). Effort is experienced during physical (e.g., running to catch the bus) or cognitive tasks (e.g., completing Sudoku) and in the context of self-restraint behavior (e.g., smoking cessation; Preston and Wegner, 2009). It is thought to influence how we move, i.e., how the nervous system selects a given movement among a myriad of possibilities (Izawa et al., 2008; Gaveau et al., 2021). Due to its omnipresence in our daily life, the interest in understanding the perception of effort is growing among researchers. This perception is linked to the task intensity and the amount of resources invested (Inzlicht et al., 2018); strongly influences the self-regulation of human behavior (Marcora, 2015; Inzlicht et al., 2018); is one of the main features of fatigue in various contexts (Enoka and Stuart, 1992; Pageaux and Lepers, 2016); and is exacerbated in various pathologies such as chronic fatigue syndrome (Cook et al., 2007; Barhorst et al., 2020), stroke (Kuppuswamy et al., 2015), chronic kidney disease (Macdonald et al., 2012), or cancer (Fernandez et al., 2020). Perception of effort is a fundamental experience that directly influences our everyday decisions to engage or disengage in various actions, by monitoring the cognitive and motor resources necessary to perform any task (Preston and Wegner, 2009; Pageaux, 2016). The perception of the amount of effort invested in a task is also closely linked to the regulation of motor performance (Pageaux, 2014, 2016; Marcora, 2019). According to the motivation intensity theory (Brehm and Self, 1989; Richter et al., 2016), one maintains performance by increasing effort when task difficulty increases and one lets performance decrease when no longer able or willing to invest additional effort.

Perception of effort is widely investigated during global locomotor tasks, such as walking or cycling, in both healthy and symptomatic populations (Horstman et al., 1979; Au et al., 2017; Zinoubi et al., 2018; Décombe et al., 2020; Flairty and Scheadler, 2020) to prescribe and monitor exercise (Impellizzeri et al., 2004; Azevedo et al., 2016; Eston and Parfitt, 2018). Perception of effort is also investigated during isolated motor tasks involving the upper or lower limb, in strength training programs (Miller et al., 2009; Zourdos et al., 2016), in studies aiming at better understanding the regulation of endurance performance (Maikala and Bhambhani, 2006; Pageaux et al., 2013) or the mechanisms associated with the development of muscle fatigue during repetitive tasks (de Morree et al., 2012; Otto et al., 2019; Yang et al., 2019; Jacquet et al., 2021). To the best of our knowledge, most of the studies investigating the perception of effort are performed during locomotor exercises or isolated exercises performed with the lower limbs (de Morree et al., 2014; Meir et al., 2015; Luu et al., 2016; Faelli et al., 2019). Although the perception of effort is of interest to understand how the nervous system controls our everyday movements, motor control studies mostly indirectly investigated it by measuring the force output, the decision made by the participants or motor strategies (Izawa and Shadmehr, 2008; Shadmehr et al., 2016; Cos, 2017; Morel et al., 2017; Gaveau et al., 2021; Wang et al., 2021). While these methods present several advantages in the context of decision-making tasks, not considering the rating of perception of effort as a dependent variable limits the exploration of the subjective experience of the participant during task completion (Pageaux, 2016; Wang et al., 2021). As the perception of effort has been recently proposed to finely regulate motor control (Cos, 2017) and, thus, to affect decision-making and performance in a task involving movement regulation (Shadmehr et al., 2016; Wang et al., 2021), there is an urgent need for studies exploring the perception of effort during upper limb tasks. Such studies could provide opportunities to better understand the interaction between the perception of effort and motor control.

In this context, the present study aimed to validate the use of the perception of effort to prescribe and monitor exercise in healthy young adults performing upper limb motor tasks. To do so, two experiments manipulated the physical demand to alter the task difficulty. In the first experiment, by using a modified version of the classical box and block test (Mathiowetz et al., 1985) and a pointing task, we tested the possibility (i) to prescribe exercise at different intensities with the perception of effort and (ii) to monitor changes in perception of effort when task difficulty was altered with manipulation

TABLE 1 Description of participants.

	Experiment 1		Experiment 2	
	Women (*n* = 18)	Men (*n* = 2)	Women (*n* = 7)	Men (*n* = 13)
Age (yrs)	24 ± 2	24 ± 2	26 ± 2	25 ± 2
Weight (kg)	62 ± 11	72 ± 14	59 ± 7	76 ± 10
Height (cm)	164 ± 10	187 ± 5	163 ± 6	178 ± 5.4
Physical activity (/30)	19.06 ± 5.4	23 ± 0	21.5 ± 6.3	23.6 ± 3.5
Right-handed	17	2	7	11
Left-handed	1	–	–	2

Yrs, years; kg, kilogram; cm, centimeter. The physical activity score was measured with the Dijon physical activity questionnaire (Robert et al., 2004). Data are presented as mean ± SD.

of the physical demand. As effort and its perception vary in relation to performance (Brehm and Self, 1989; Richter et al., 2016), we monitored the perception of effort while controlling for performance. We hypothesized that (i) it is possible to prescribe different exercise intensities with the perception of effort, as attested by an increased task performance when the prescribed intensity of perceived effort increases and (ii) increasing task difficulty, with faster tempos or additional weights, will be reflected in higher perceptions of effort. In the second experiment, by using the classical box and block test with its validated instructions, we tested the effect of increasing physical demand on subsequent performance and rating of perception of effort. We hypothesized that performance could be maintained at the cost of a higher resource mobilization as reflected by the increases in the perception of effort.

2. Materials and methods

2.1. Participants

Twenty participants volunteered to participate in experiment 1 and twenty participants volunteered to participate in experiment 2. The description of the participants is available in **Table 1**. None of the participants reported any pain-related, neurological, psychological disorders, or somatic illnesses. Written informed consent was obtained from each participant. Experiment 1 took place at the Centre de recherche de l'Institut universitaire de gériatrie de Montréal. Experiment 2 took place at the Espace d'Etude du Mouvement—Etienne Jules MAREY de l'Université de Bourgogne. We performed two experiments with different participants to challenge the replication of our results. All participants gave written informed consent, and procedures were approved by the local ethics committee (CER VN 18-19-35). As caffeine and sleep deprivation are known to alter the perception of effort (Temesi et al., 2013; de Morree et al., 2014), participants in both experiments were asked to refrain from ingesting caffeine at least 3 h before their visits and to get at least 7 h of sleep the night before.

2.2. Upper limb motor tasks

In this study, the upper limb motor tasks were the Box and Block Test (BBT) and a Pointing Task (PT). A full description of these tests is available below. We chose these two tests for their relevance in the context of clinical settings as well as research.

2.2.1. Box and block test

The BBT (Mathiowetz et al., 1985), illustrated in **Figure 1A**, is used to assess manual dexterity, defined as "the ability to make coordinated hand and finger movements to grasp and manipulate objects" (Makofske, 2011). This test has been validated in several populations such as older adults (Desrosiers et al., 1994), fibromyalgia patients (Canny et al., 2009), and stroke rehabilitation (Lin et al., 2010). The test consists of a wooden box (53.7 cm × 25.4 cm × 8.5 cm) separated into two containers of 25.4 cm each. It includes 150 wooden cubes (2.5 cm). Participants have to grasp one block at a time with the dominant hand, transport the block over the partition, and release it into the opposite compartment. Standardized instructions require participants to move as many blocks as possible in 60 s, and performance is monitored as the number of blocks moved. In experiment 1, we used a 30-s modified version of the BBT where participants had to move the blocks at a prescribed effort intensity or by following a pre-determined tempo signaled by an auditory cue to control for the number of blocks moved (performance). In experiment 2, we used the standardized instructions in the absence and presence of additional weight on the dominant forearm. In both experiments, the compartment containing the block was placed in front of the participants' dominant hand. Errors were visually counted by an experimenter when the fingertips did not go beyond the partition, and the associated block was not counted in the final score. Participants were informed that blocks will not be counted in the final score when the fingertips do not go beyond the partition.

2.2.2. Pointing task

Pointing tasks (PT) are widely used in research to study motor control (e.g., Domkin et al., 2002; Missenard et al., 2009). A PT (illustrated in **Figure 1B**) was performed in experiment 1. Participants had to go back and forth between targets (squares of 1 cm^2) as quickly as possible in a given time. Participants started from target 1 (reference target) and had to follow a pre-determined order, depending on their dominant hand. Right-handed participants had to reach target 2 and come back to target 1, then reach target 3 and come back to target 1, then reach target 4 and come back to target 1, and then reach target 5 and come back to target 1. This sequence was repeated for 30 s, either

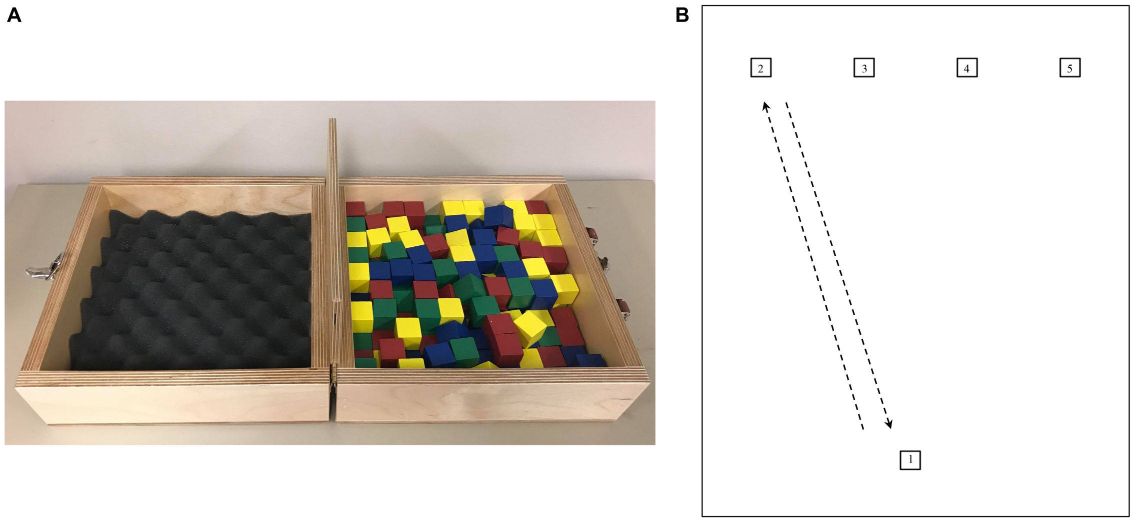

FIGURE 1
(A) Illustration of the box and block test (Mathiowetz et al., 1985) used in experiments 1 and 2. Briefly, participants had to grasp one block at a time with the dominant hand, transport the block over the partition, and release it into the opposite compartment. **(B)** Illustration of the pointing task used in experiment 1. Starting from target 1, participants had to go back and forth between each target. Right-handed participants started by reaching target 2 for their first-round trip, while left-handed participants started by reaching target 5 for their first-round trip. Measures are being taken from the center of all squares (1 × 1 cm). The distance between each upper square is 5.1 cm. The distance between targets 1–2 and 1–5 is 22.3 cm, respectively. The distance between targets 1–3 and 1–4 is 21 cm, respectively.

with the instructions of reaching the targets at a prescribed effort intensity or by following a pre-determined tempo to control for the number of targets reached (i.e., performance). For left-handed participants, the order of the sequence was reversed. They had to first reach target 5. Participants performed the test with a pencil in their hand and had to point where they reached, thus allowing an experimenter to visually control for the exact number of targets correctly reached. Participants were informed that a target will be counted in the final score only when the mark is placed inside a target.

2.3. Overview of the two experiments

2.3.1. Experiment 1

This experiment aimed to test, with a modified version of the BBT and a PT, the possibility (i) to use the perception of effort to prescribe exercise (Exp. 1A), and (ii) to monitor changes in the rating of perception of effort when performance is controlled, and task difficulty manipulated (Exp. 1B). (i) To test the possibility of prescribing exercise with a target level of perceived effort, we monitored performance associated with four intensities of perception of effort (presented in **Figure 2A**). (ii) To test the possibility of monitoring changes in the perception of effort, we manipulated task difficulty by increasing physical demand. Task difficulty was increased by increasing the speed of movement (*tempo session*) or by adding a weight on the forearm (*weight session*). The weight session was performed at a controlled pace such that the effect of task demand on perception of effort was assessed at a controlled performance level (i.e., constant speed). The *tempo session* and *weight session* were performed in two different laboratory visits, in a randomized order. An overview of the sessions is presented in **Figure 2B**. All tests were performed in a seated position. At the onset of the first laboratory visit, participants completed several questionnaires allowing the characterization of the population studied (anthropometry, physical activity score; Robert et al., 2004), Edinburgh Handedness Inventory (Oldfield, 1971). Then, each session was performed as described below, with all BBT trials performed in one block and all PT trials related performed in another block. The order of each block (BBT performed first vs. PT performed first) was randomized between participants and kept constant for each participant between the two laboratory visits (*tempo session* vs. *weight session*).

2.3.1.1. Tempo session

Participants were equipped with the apparatus allowing measurement of EMG, heart rate, and/or respiratory frequency. We subsequently provided standardized instructions on how to use the psychophysical rating scale to monitor the perception of effort and how to perform the BBT and the PT. Participants had 1 min to familiarize themselves with each test and could ask any questions. Following this familiarization, participants were asked to perform a block of trials for the BBT or PT. The first block consisted of trials using a target level of perceived effort intensity to prescribe the exercise, and the second block consisted of trials where performance was controlled by

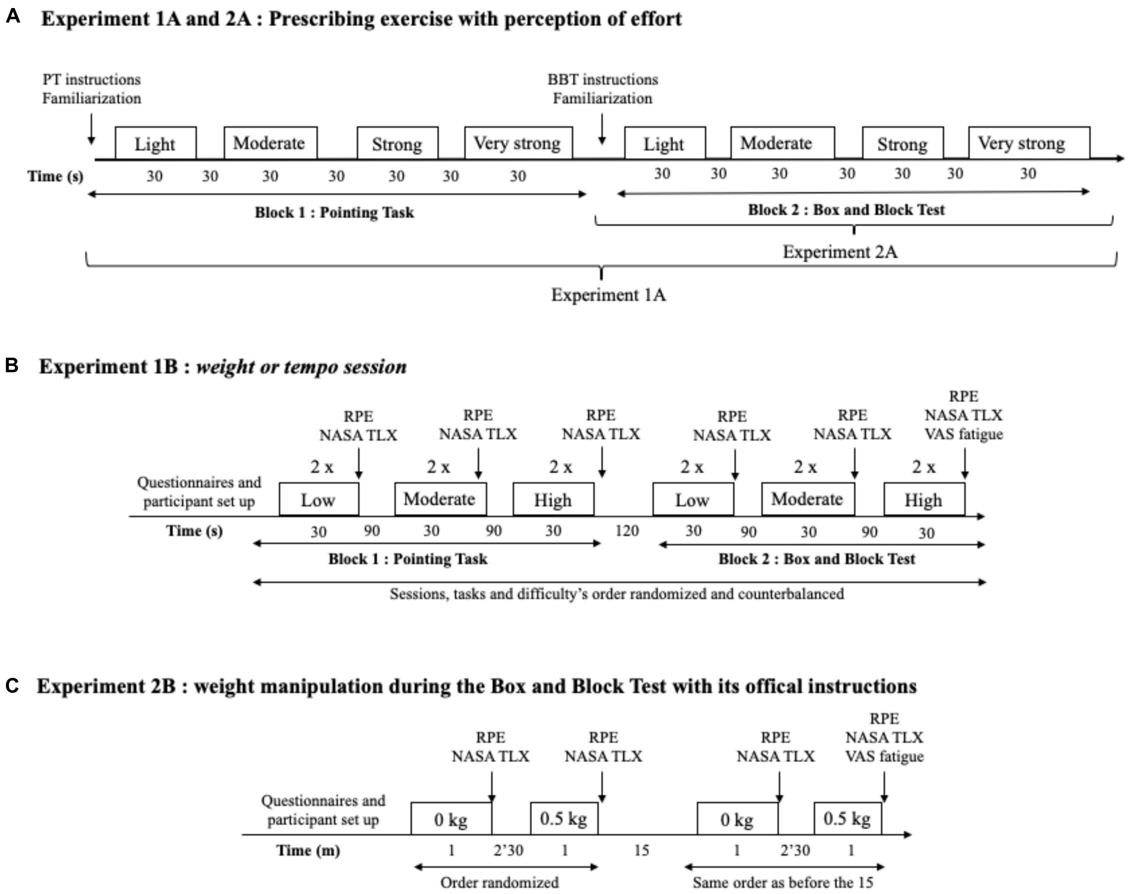

FIGURE 2
(A) Experiments 1A and 2A overview: The procedures used to test the possibility to prescribe exercise using the perception of effort. The exercise was prescribed at four intensities of perceived effort *via* the CR100 scale: light (13/100), moderate (23/100), strong (50/100), and very strong (70/100). While both the pointing task (PT) and the box and block test (BBT) were performed in experiment 1A, only the BBT was performed in experiment 2A. **(B)** Experiment 1B overview. Set up consisted of the placement of the respiratory frequency belt, heart rate monitor, and EMG surface electrodes. Then, participants completed the indicated questionnaire or visual analog scale (VAS). Participants performed two repetitions per level of difficulty with 30 s of recovery in between. Rating of perceived effort (RPE) and subjective workload using NASA TLX scale were assessed in-between each level of difficulty. **(C)** Experiment 2B overview. Participants performed the box and block test for 60 with the absence (0 kg) or presence (0.5 kg) of additional weights. Set up consisted of the placement of the heart rate monitor and the EMG surface electrodes. Then, participants completed the indicated questionnaire or scale.

different tempos and where the perception of effort was reported by the participant. Trials related to the use of perception of effort to prescribe the exercise intensity consisted of performing one test of 30 s per target perceived effort intensity level (light effort, moderate effort, strong effort, and very strong effort), with each test interspaced by 30 s of recovery. The experimenter recorded performance for each prescribed intensity. Then, participants performed two tests of 30 s per difficulty level (low, moderate, and high), with each test interspaced by 90 s of recovery. Once a block (BBT vs. PT trials) was completed, a 120 s rest was given, and participants completed the other block following the same structure. Following pilot experiments, three tempos specific to each task were chosen to produce three levels of difficulty. For the PT, the following tempos were used: 1 Hz (slow tempo), 1.5 Hz (moderate tempo), and 2 Hz (fast tempo). For the BBT, the following tempos were used: 0.5 Hz (slow tempo), 0.75 Hz (moderate tempo), and 1 Hz (fast tempo). The order of the level of difficulties was randomized. The rating of perceived effort was measured immediately at the end of each repetition. Following the two repetitions of each of the difficulty level, participants reported their perceived workload using the NASA TLX scale as described below.

2.3.1.2. Weight session

The procedures in the *weight sessions* are identical to the procedures in the *tempo session*, except that task difficulty was manipulated by adding weights (4-lb pair, Enhance Fitness) on the dominant forearm of the participant while performing the BBT and PT at a fixed tempo (BBT: 0.75 Hz; PT: 1.5 Hz). The low difficulty level was performed with no additional weight (0 kg, light weight) on the forearm. The moderate and high difficulty levels were performed with additional weights, 0.5 kg (moderate weight) and 1 kg (heavy weight), respectively, on the forearm.

2.3.2. Experiment 2

The second experiment aimed (Exp. 2A) to replicate the results of the perception of effort prescription condition of experiment 1A and to test the effect of increasing physical demand to manipulate BBT difficulty on subsequent performance and ratings of perception of effort (Exp. 2B). Participants visited the laboratory one time. At their arrival, participants were equipped with the apparatus allowing measurement of EMG and heart rate. We subsequently provided standardized instructions on how to use the psychophysical scale to monitor the perception of effort and how to perform the BBT. Participants had 1 min to familiarize themselves with each test and could ask any questions. Following this familiarization, participants were asked to perform two blocks of trials. The first block consisted of trials related to using the perception of effort intensity to prescribe the exercise, as performed in experiment 1. In the second block of trials, participants completed the BBT according to the standardized duration of 60 s, in the absence (0 kg, low difficulty level) and the presence (0.5 kg, high difficulty level) of additional weight on the dominant forearm, interspaced by a 2.5 min recovery between difficulties. The order of difficulty levels (0 kg, low difficulty level vs. 0.5 kg, high difficulty level) was randomized between participants and repeated after a 15 min break. In total, each participant repeated each level of difficulty twice.

Pilot experiments revealed that the duration of 60 s with an additional weight of 1 kg induced an important level of fatigue in the participants. Consequently, to limit the induction of fatigue, the high level of difficulty was performed with a weight of 0.5 kg and a between level of difficulty recovery period of 2.5 min. The rating of perceived effort and performance (i.e., number of blocks moved) was monitored immediately at the end of each repetition (three repetitions per level of difficulty, with the order of difficulty randomized). Following each level of difficulty, participants reported their perceived workload using the NASA TLX scale as described below. An overview of the session is presented in **Figure 2C**.

2.4. Psychological measurements

2.4.1. Perception of effort

Perception of effort, defined as the conscious sensation of "how hard, heavy and strenuous a physical task is" (Marcora, 2010; Pageaux, 2016), was measured and used to prescribe the exercise with the CR100 scale (Borg and Kaijser, 2006). This scale ranges from 0 ("nothing at all") to 100 ("maximal") and includes verbal anchors, such as light (weak), moderate, and strong (heavy) for intermediate values (Borg and Kaijser, 2006). Standardized instructions on how to use the CR100 scale were provided. Then, participants received standardized instructions on how to evaluate the perception of effort and exclude the perception of pain from their rating (Pageaux, 2016; Pageaux et al., 2020). Participants had the opportunity to ask questions on the scale and effort rating instructions before starting the experiments. To prescribe exercise, participants were asked to perform the tasks at four different effort intensities associated with the following verbal anchors and numbers on the CR100 scale: light (13), moderate (23), strong (50), and very strong (70). To report their perception of effort, participants were asked to first refer to the verbal anchors and then to report a number that best represents the intensity of their perception. The CR100 scale was printed in a legal format (8.5 × 14 in) and fixed on a wall ~1 m in front of the participants.

2.4.2. Perceived workload

Perceived workload was measured with the Nasa Task Load Index (NASA TLX; Hart and Staveland, 1988). In line with the aims of our study, only the four following subscales were considered: Physical Demand, Mental Demand, Temporal Demand, and Effort. Participants had to score each of the items on a scale divided into 20 equal intervals anchored by a bipolar descriptor (e.g., High/Low). This score was multiplied by 5, resulting in a final score between 0 and 100 for each of the six subscales.

2.4.3. Fatigue

The presence of fatigue is known to increase the perception of effort (Enoka and Stuart, 1992; Pageaux and Lepers, 2016). We consequently monitored feelings of fatigue at the beginning and the end of each visit with a visual analog scale (Le Mansec et al., 2017). Participants had to place a mark on a 100 mm line with bipolar end anchors (0 = not fatigued at all; 100 = extremely fatigued). The fatigue score was determined by measuring the distance (in mm) from the left-hand end of the line to the mark made by the participant.

2.5. Physiological measurements

Electromyography (EMG) of the biceps brachii and triceps lateral head was measured in both experiments with adhesive, pre-gelled surface electrodes (Covidien, CA). The decision to measure muscle activation of these two muscles was taken following a preliminary experiment where participants (N = 20) performed the block and block tests with and without the addition of a 0.5 kg weight on the forearm. During task completion, measurements of the EMG signal of eight muscles were performed. The results are available in **Supplementary material** and revealed that the biceps brachial was the muscle presenting the greater increase in root mean square EMG in the presence of the additional weight over the forearm. Consequently, we decided to measure as a second muscle an antagonist, the triceps lateral head. Before placing the electrodes, the skin was shaved, cleaned with alcohol, and dried. Electrodes were placed using SENIAM recommendations (Hermens et al., 2000). The electrode reference was attached to the extremity of the elbow of the dominant arm. In experiment 1, EMG was recorded using a PowerLab system (26T, ADInstruments) with an acquisition rate of 1 KHz and filtered with bandpass ranging from 20 to 400 Hz (auto adjust) and a notch filter with a center frequency of 60 Hz (auto adjust). Data were analyzed using the LabChart software (AD Instruments). In experiment 2, EMG was recorded using a Biopac system (MP150, Biopac Systems, Inc.) with an acquisition rate of 1 KHz and filtered with bandpass ranging from 20 to 400 Hz (auto adjust) and a notch filter with a center frequency of 60 Hz (auto adjust). Data were analyzed using Acknowledge software (Biopac Systems, Inc.). The root mean square (RMS) was automatically calculated with each software. Data were averaged for the last 5 s of each 30 s (experiment 1) or 60 s (experiment 2) trials.

Heart rate frequency was measured in both experiments. In experiment 1, we used a finger pulse transducer (TN1012/ST, AD Instruments) placed on the non-dominant index finger. To limit movement artifacts, the non-dominant hand was placed on a homemade support to rest on the table and stay as steady as possible. The signal was recorded with an acquisition rate of 1 KHz and filtered with a digital filter of 7 Hz (low pass). Data analysis was automatically performed by the LabChart software. Heart rate frequency was averaged for the last 5 s of each 30 s trials. Due to numerous movement artifacts in experiment 1, monitoring heart rate was measured using a chest strap *via* the paired Polar watch (Polar RS400; Polar Electro Oy, Kempele, Finland) and measured as the average of the 60 s trial. The experimenter pressed the start/stop button of the watch at the beginning and end of each trial and then recorded the average heart rate frequency calculated by the watch.

Respiratory frequency was measured in experiment 1 only *via* a respiratory belt transducer (TN11132/ST, AD Instruments). The respiratory belt was fixed on the participant's chest, the signal was recorded with an acquisition rate of 1 KHz and filtered with a digital filter of 7 Hz (low pass). Data analysis was automatically performed by the LabChart software. Respiratory frequency was averaged from the last 5 s of each 30 s trials.

2.6. Statistical analysis

All data are presented as mean ± standard deviation in the text. Assumptions of statistical tests such as normal distribution and sphericity of data were checked as appropriate. Greenhouse-Geisser correction to the degrees of freedom was applied when violation to sphericity was present.

2.6.1. Experiment 1A

All analyses subsequently described were performed for the modified BBT and PT. A 2 × 4 repeated-measures ANOVA was used to assess the effects of visits (1 and 2) and effort intensity (light, moderate, strong, and very strong) on performance, heart rate frequency, and respiratory frequency. A 2 × 4 × 2 repeated-measures ANOVA was used to assess the effects of visit (1 and 2), effort intensity (light, moderate, strong, and very strong), and muscle (biceps brachial and triceps brachial) on RMS EMG. As these analyses were performed to test the possibility to use the perception of effort to prescribe the exercise, a significant main effect of effort intensity only was followed with the following pairwise comparisons adjusted with the Bonferroni correction: light effort vs. moderate effort, moderate effort vs. strong effort, and strong effort vs. very strong effort.

2.6.2. Experiment 1B

To test the possibility to monitor changes in perception of effort when task difficulty is altered with manipulation of the physical demand in both *tempo* and *weight sessions,* a repeated-measures ANOVA was used to assess the effects of difficulty (easy, medium, and hard) on heart rate and respiratory frequencies. A 3 × 2 repeated-measures ANOVA was used to assess the effects of difficulty (easy, medium, and hard) and muscle (biceps brachial and triceps brachial) on RMS EMG. The significant effect of difficulty was followed-up with pairwise comparisons adjusted with the Bonferroni correction. A Friedman ANOVA was used to assess the effects of difficulty on performance, rating of perceived effort, as well as the physical demand, mental demand, temporal demand, and effort subscales of the NASA TLX scale. The significant effect of difficulty was followed up with the Wilcoxon signed-ranked tests adjusted with the Bonferroni correction.

2.6.3. Experiment 2A

A repeated-measures ANOVA was used to assess the effects of effort intensity (light, moderate, strong, and very strong) on performance, heart rate frequency, and RMS EMG. A 4 × 2 repeated-measures ANOVA was used to assess the effects of effort intensity (light, moderate, strong, and very strong) and muscle (biceps brachial and triceps brachial) on RMS EMG. As these analyses were performed to test the possibility to use the perception of effort to prescribe the exercise, a significant effect of effort intensity only was followed with the following pairwise comparisons adjusted with the Bonferroni correction: light effort vs. moderate effort, moderate effort vs. strong effort, and strong effort vs. very strong effort.

2.6.4. Experiment 2B

A 2 × 2 repeated-measures ANOVA was used to assess the effects of repetition (1 and 2) and difficulty (easy and hard) on performance, rating of perceived effort, heart rate frequency, as well as the physical demand, mental demand, and effort subscales of the NASA TLX scale. A 2 × 2 × 2 repeated-measures ANOVA was used to assess the effects of repetition (1 and 2), difficulty (easy and hard), and muscle (biceps brachial and triceps brachial) on RMS EMG. As experiment 2B did not constrain the temporal demand of the task by imposing a tempo, we did not analyze the temporal demand subscale of the NASA TLX scale. If a repetition × difficulty interaction reached significance, the following follow-up tests were performed and adjusted with the Bonferroni correction: repetition 1/0 kg vs. repetition 2/0 kg, repetition 1/0.5 kg vs. repetition 2/0.5 kg, repetition 1/0 kg vs. repetition 1/0.5 kg, and repetition 2/0 kg vs. repetition 2/0.5 kg.

For both experiments, all statistical analyses were performed using the Statistical Package for the Social Sciences software, version 27 for Mac OS X (SPSS, Chicago, IL) and jamovi software, version 2.0.0.0. Effect sizes for the repeated measures ANOVA are reported as the partial eta squared (η_p^2) provided by SPSS. Effects sizes for the pairwise comparisons are reported with r and calculated with Microsoft Excel according to the equations described below for parametric (i) and non-parametric and (ii) tests (Field, 2005). Parameters t, df, and Z were provided by SPSS, and N corresponds to the total number of observations (Field, 2005).

$$\text{(i)}\ r = \sqrt{\frac{t^2}{t^2 + df}} \qquad \text{(ii)}\ r = \frac{Z}{\sqrt{N}}$$

Significance was set at 0.05 (2-tailed). Thresholds for small, moderate, and large effects were set at 0.1, 0.3, and 0.5 for r (Cohen, 1988).

3. Results

3.1. Experiment 1

In this experiment, we used a modified version of the BBT and PT. We prescribed 30 s of exercise performed at four intensities of effort (light, moderate, strong, and very strong) in two different visits. Performance, RMS EMG, heart rate, and respiratory frequencies were monitored for each prescribed effort intensity. We also manipulated task difficulty levels (low, moderate, and high) by manipulating physical demand and imposing three tempos or adding three different weights on the participant's dominant forearm while performing the task at a fixed tempo. Performance, heart rate frequency, respiratory frequency, RMS EMG, and the subjective workload were measured for each difficulty.

3.1.1. Experiment 1A: Using the perception of effort to prescribe the exercise

The results of the main effects of effort intensity for the BBT and PT are presented in Figures 3 and 4, respectively.

3.1.1.1. Performance

For the BBT (Figure 3A), the main effect of visit did not reach significance [$F(1, 19) = 2.105$, $p = 0.163$, $\eta_p^2 = 0.099$]. Increasing the prescribed effort intensity resulted in an increased performance during the BBT [$F(1.6, 31.2) = 172.335$, $p < 0.001$, $\eta_p^2 = 0.901$]. The follow-up test revealed an increase in performance between the light and moderate intensities [$t(19) = 10.509$, $p < 0.001$, $r = 0.924$], between the moderate and strong intensities [$t(19) = 10.474$, $p < 0.001$, $r = 0.923$], as well as between the strong and very strong intensities [$t(19) = 7.191$, $p < 0.001$, $r = 0.855$]. The visit × effort intensity interaction did not reach significance [$F(3, 57) = 0.401$, $p = 0.752$, $\eta_p^2 = 0.021$]. For the PT (Figure 4A), the main effect of visit did not reach significance [$F(1, 19) = 0.749$, $p = 0.397$, $\eta_p^2 = 0.038$]. The main effect of effort intensity reached significance [$F(1.6, 31.1) = 112.050$, $p < 0.001$, $\eta_p^2 = 0.855$]. The follow-up test revealed an increase in performance between the light and moderate intensities [$t(19) = 8.162$, $p < 0.001$, $r = 0.882$], between the moderate and strong intensities [$t(19) = 10.681$, $p < 0.001$, $r = 0.926$], as well as between the strong and very strong intensities [$t(19) = 6.291$, $p < 0.001$, $r = 0.822$]. The visit × effort intensity interaction did not reach significance [$F(1.4, 26.8) = 1.342$, $p = 0.270$, $\eta_p^2 = 0.065$].

3.1.1.2. RMS EMG

For the BBT (Figure 3B), the mean RMS EMG of the biceps brachii was higher than the mean RMS EMG of the triceps [$F(1, 18) = 11.174$, $p = 0.003$, $\eta_p^2 = 0.081$]. The main effect of visit did not reach significance [$F(1, 18) = 2.018$, $p = 0.172$, $\eta_p^2 = 0.003$]. There was a main effect of effort intensity [$F(1.3, 24.7) = 37.667$, $p < 0.001$, $\eta_p^2 = 0.161$] showing an increase between the light and moderate intensities [$t(18) = 5.904$, $p < 0.001$, $r = 0.812$], between the moderate and strong intensities [$t(18) = 5.229$, $p < 0.001$, $r = 0.777$], and between the strong and very strong intensities [$t(18) = 4.109$, $p = 0.002$, $r = 0.696$. The muscle × effort intensity interaction did not reach significance [$F(1.6, 29.2) = 0.752$, $p = 0.454$, $\eta_p^2 = 0.001$]. For the PT (Figure 4B), the mean RMS EMG of the biceps brachii was higher than the mean RMS EMG of the triceps [$F(1, 19) = 14.477$, $p = 0.001$, $\eta_p^2 = 0.187$]. The main effect of visit did not reach significance [$F(1, 19) = 0.029$, $p = 0.866$, $\eta_p^2 < 0.001$]. There was a main effect of effort intensity [$F(1.2, 24.1) = 43.575$, $p < 0.001$, $\eta_p^2 = 0.085$] showing an increase between the light and moderate intensities [$t(19) = 6.410$, $p < 0.001$, $r = 0.827$], between the moderate and strong intensities [$t(19) = 5.541$, $p < 0.001$, $r = 0.786$], and between the strong and very strong intensities [$t(19) = 4.812$, $p < 0.001$, $r = 0.741$]. The

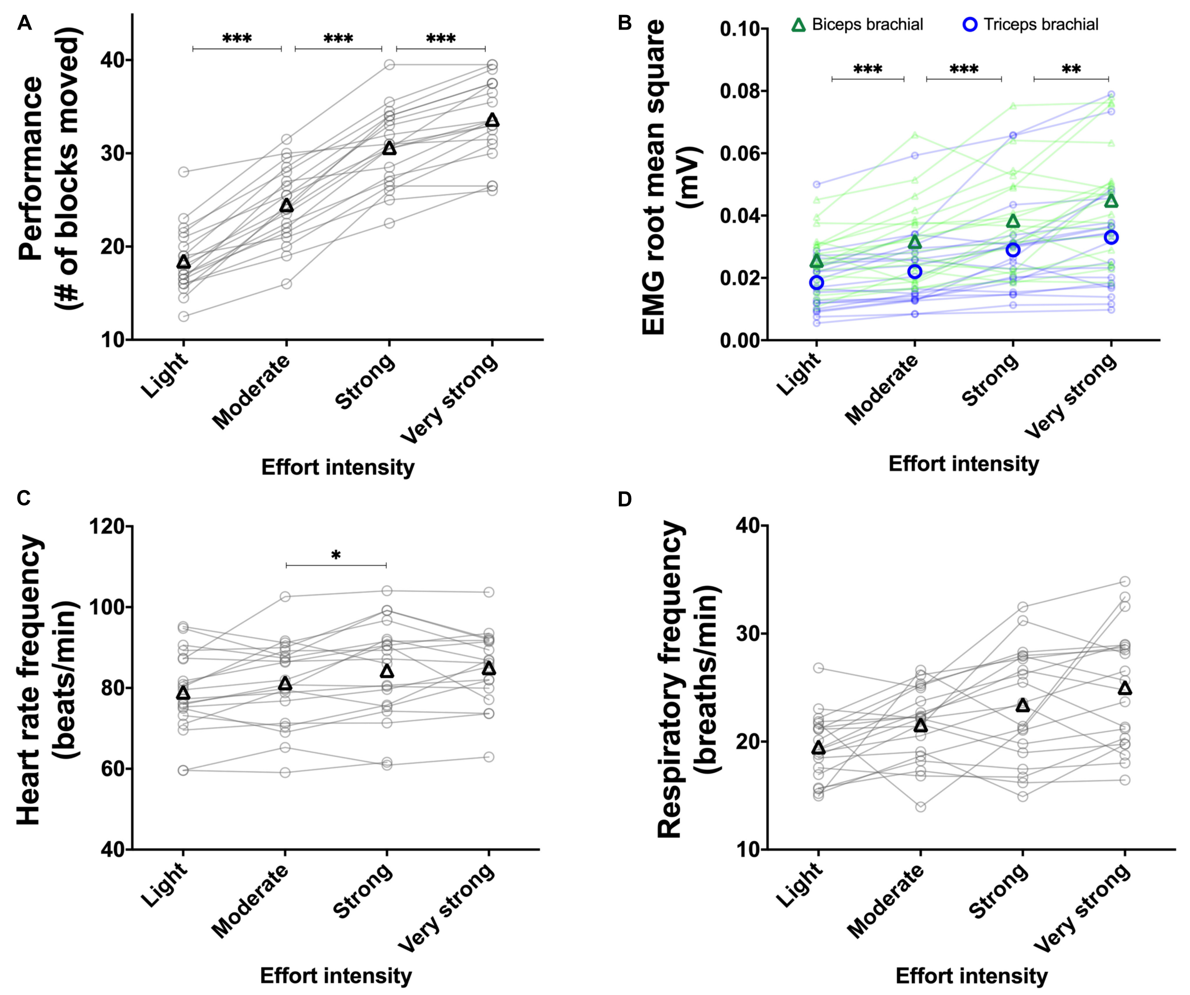

FIGURE 3
Experiment 1A: Using the perception of effort to prescribe the exercise during the box and block test. Effect of increasing the prescribed effort intensity on performance (**A**, n = 20), EMG root mean square of the biceps (green line) and triceps (blue line) brachial muscles (**B**, n = 19), heart rate frequency (**C**, n = 18), and respiratory frequency (**D**, n = 20) during the box and block test. The exercise was prescribed at four intensities of perceived effort via the CR100 scale: light (13/100), moderate (23/100), strong (50/100), and very strong (70/100). Data are presented as the main effect of effort intensity **(A, C, D)** and effort intensity × muscle interaction **(B)**. The n indicates the number of participants with all the data in each four effort intensities. Changes in the n reflect data loss due to the issue with equipment or movement artifact. Individual data are presented in light markers and means in dark markers. *Main effect of intensity, the difference between two effort intensities. *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$.

muscle × effort intensity interaction did not reach significance [$F(1.3, 24.7) = 3.281$, $p = 0.072$, $\eta_p^2 = 0.002$].

3.1.1.3. Heart rate frequency

For the BBT (Figure 3C), the main effect of visit did not reach significance [$F(1, 8) = 0.851$, $p = 0.383$, $\eta_p^2 = 0.096$]. The main effect of effort intensity reached significance [$F(3, 24) = 8.166$, $p = 0.001$, $\eta_p^2 = 0.505$]. The follow-up tests revealed an increase in heart rate frequency between the moderate and strong intensities [$t(17) = 3.176$, $p = 0.017$, $r = 0.610$]. Neither the increase in heart rate frequency between the light and moderate intensities [$t(17) = 1.490$, $p = 0.464$, $r = 0.340$] nor the one between the strong and very strong intensities did reach significance [$t(17) = 0.334$, $p = 1.000$, $r = 0.081$]. The visit × effort intensity interaction did not reach significance [$F(3, 24) = 0.896$, $p = 0.458$, $\eta_p^2 = 0.101$]. For the PT (Figure 4C), the main effect of visit did not reach significance [$F(1, 14) = 0.218$, $p = 0.647$, $\eta_p^2 = 0.015$]. The main effect of effort reached significance [$F(3, 42) = 14.804$, $p < 0.001$, $\eta_p^2 = 0.513$]. The follow-up test revealed an increase in heart rate frequency between the moderate and strong intensities [$t(19) = 3.285$, $p = 0.012$, $r = 0.602$], but not between the light and moderate intensities [$t(19) = 2.182$, $p = 0.126$, $r = 0.448$] not between the strong and very strong intensities [$t(19) = 1.941$, $p = 0.202$, $r = 0.407$]. The visit × effort intensity interaction did not reach significance [$F(3, 42) = 0.406$, $p = 0.748$, $\eta_p^2 = 0.028$].

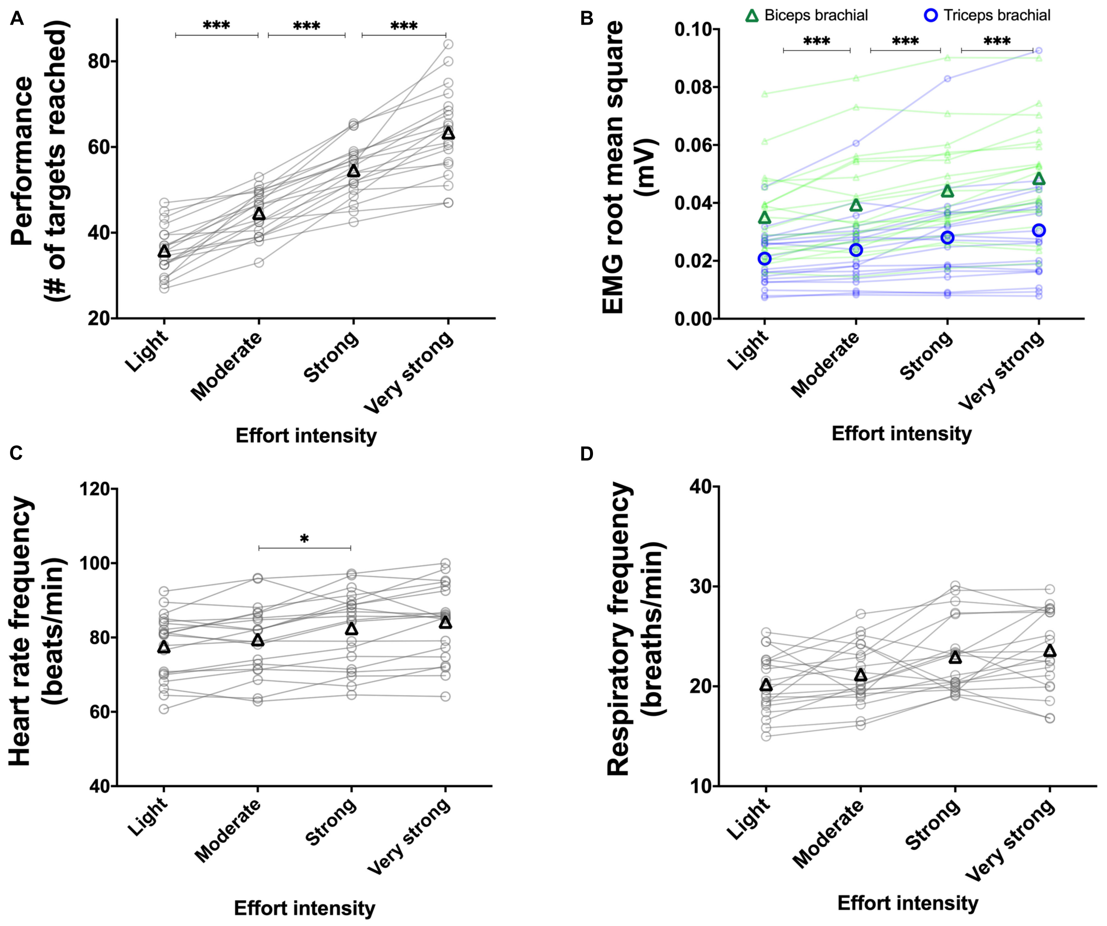

FIGURE 4
Experiment 1A: Using the perception of effort to prescribe the exercise during the pointing task. Effect of increasing the prescribed effort intensity on performance (**A**, $n = 20$), EMG root mean square of the biceps (green line) and triceps (blue line) brachial muscles (**B**, $n = 20$), heart rate frequency (**C**, $n = 20$), and respiratory frequency (**D**, $n = 20$) during the pointing task. The exercise was prescribed at four intensities of perceived effort *via* the CR100 scale: light (13/100), moderate (23/100), strong (50/100), and very strong (70/100). Data are presented as the main effect of effort intensity **(A, C, D)** and effort intensity × muscle interaction **(B)**. Individual data are presented in light markers and means in dark markers. *Main effect of intensity, the difference between two effort intensities. *$p < 0.05$, ***$p < 0.001$.

3.1.1.4. Respiratory frequency

For the BBT (**Figure 3D**), the main effect of visit did not reach significance [$F(1, 13) = 0.008$, $p = 0.930$, $\eta_p^2 = 0.001$]. The main effect of effort intensity reached significance [$F(3, 39) = 6.463$, $p = 0.001$, $\eta_p^2 = 0.332$]. However, neither the increase in respiratory frequency between the light and moderate intensities [$t(19) = 2.450$, $p = 0.072$, $r = 0.490$], between the moderate and strong intensities [$t(19) = 2.131$, $p = 0.139$, $r = 0.439$], or between the strong and very strong intensities did reach significance [$t(19) = 1.663$, $p = 0.338$, $r = 0.357$]. The visit × effort intensity interaction did not reach significance [$F(3, 39) = 0.084$, $p = 0.970$, $\eta_p^2 = 0.006$]. For the PT (**Figure 4D**), the main effect of visit did not reach significance [$F(1, 15) = 0.142$, $p = 0.711$, $\eta_p^2 = 0.009$]. The main effect of effort intensity reached significance [$F(3, 45) = 10.893$, $p < 0.001$, $\eta_p^2 = 0.421$]. However, again, neither the increase in respiratory frequency between the light and moderate intensities [$t(19) = 1.648$, $p = 0.347$, $r = 0.354$], between the moderate and strong intensities [$t(19) = 2.451$, $p = 0.072$, $r = 0.490$], or between the strong and very strong intensities did reach significance [$t(19) = 1.052$, $p = 0.917$, $r = 0.235$]. The visit × effort intensity interaction did not reach significance [$F(3, 45) = 0.195$, $p = 0.899$, $\eta_p^2 = 0.012$].

3.1.2. Experiment 1B: Manipulating the tempo to alter task difficulty

The results for the BBT and PT during the tempo sessions are presented in **Figures 5** and **6**, respectively.

3.1.2.1. Performance

For the BBT (**Figure 5A**), manipulation of the tempo increased performance [$\chi^2(2) = 40$, $p < 0.001$]. Performance increased between the low and moderate difficulties ($Z = 3.990$,

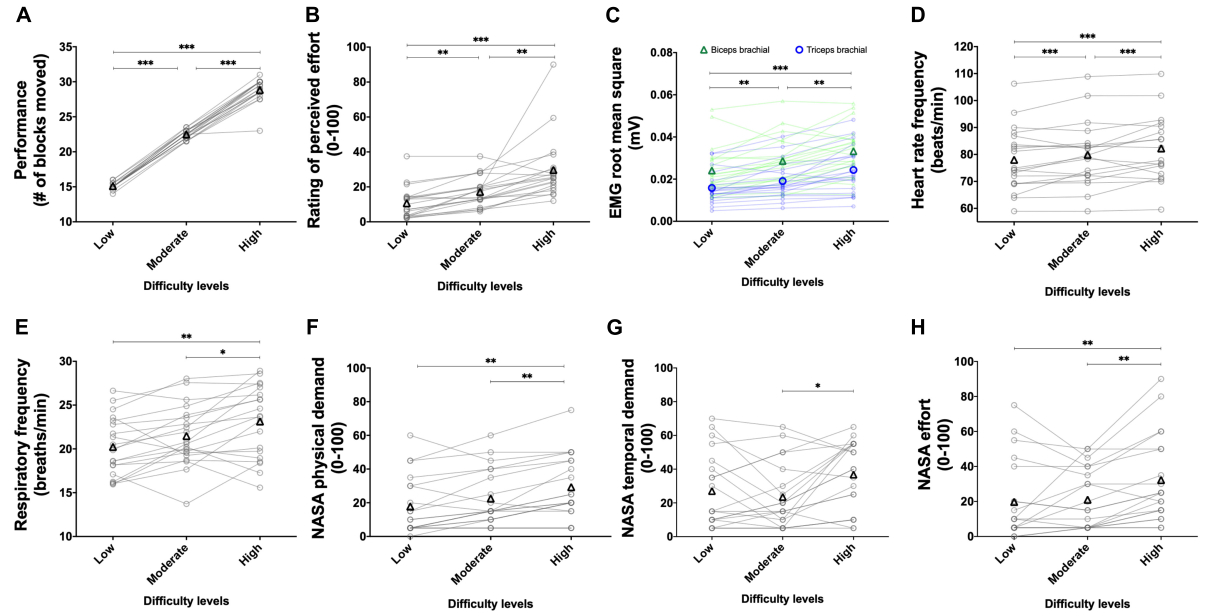

FIGURE 5
Experiment 1B: Manipulating the tempo to alter task difficulty during the box and block test. Effect of manipulating the tempo during the box and block test on performance (**A**, $n = 20$), rating of perceived effort (**B**, $n = 20$), EMG root mean square of the biceps (green line) and triceps (blue line) brachial muscles (**C**, $n = 20$), heart rate frequency (**D**, $n = 18$), respiratory frequency (**E**, $n = 20$) and NASA TLX scores for physical demand (**F**, $n = 20$), temporal demand (**G**, $n = 20$), and subjective effort (**H**, $n = 20$). For the low difficulty, a 0.5 Hz tempo was imposed. For moderate difficulty, a 0.75 Hz tempo was imposed. For the high difficulty, a 1 Hz tempo was imposed. Data are presented as the main effect of difficulty, except for panel **(C)** presenting the difficulty × muscle interaction. The n indicates the number of participants with all the data in each of the three levels of difficulties. Changes in the n reflect data loss due to issues with equipment or movement artifact. Individual data are presented in light markers and means in dark markers. *Main effect of difficulty, the difference between two difficulty levels. *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$.

$p < 0.001$, $r = 0.631$), between the low and high difficulties ($Z = 3.935$, $p < 0.001$, $r = 0.622$), as well as between the moderate and high difficulties ($Z = 3.941$, $p < 0.001$, $r = 0.623$). One participant did not show an increase in performance between the moderate and high difficulties, as shown in the figure. For the PT (**Figure 6A**), manipulation of the tempo increased performance too [$\chi^2(2) = 40$, $p < 0.001$]. Performance increased between the low and moderate difficulties ($Z = 3.965$, $p < 0.001$, $r = 0.627$), between the low and high difficulties ($Z = 3.941$, $p < 0.001$, $r = 0.623$), as well as between the moderate and high difficulties ($Z = 3.932$, $p < 0.001$, $r = 0.622$).

3.1.2.2. Perception of effort

For the BBT (**Figure 5B**), manipulation of the tempo increased the rating of perceived effort [$\chi^2(2) = 30.152$, $p < 0.001$]. Rating of perceived effort increased between the low and moderate difficulties ($Z = 3.747$, $p = 0.001$, $r = 0.592$), between the low and high difficulties ($Z = 3.790$, $p < 0.001$, $r = 0.599$), and between the moderate and high difficulties ($Z = 3.460$, $p = 0.002$, $r = 0.547$). For the PT (**Figure 6B**), manipulation of the tempo increased the rating of perceived effort too [$\chi^2(2) = 36.1$, $p < 0.001$]. Rating of perceived effort increased between the low and moderate difficulties ($Z = 3.865$, $p < 0.001$, $r = 0.611$), between the low and high difficulties ($Z = 3.921$, $p < 0.001$, $r = 0.620$), as well as between the moderate and high difficulties ($Z = 3.883$, $p < 0.001$, $r = 0.614$).

3.1.2.3. RMS EMG

For the BBT (**Figure 5C**), the mean RMS EMG of the biceps brachii was higher than the mean RMS EMG of the triceps [$F(1, 19) = 10.441$, $p = 0.004$, $\eta_p^2 = 0.355$]. There was a main effect of difficulty [$F(1.46, 27.73) = 22.851$, $p < 0.001$, $\eta_p^2 = 0.546$], showing an increase between the low and moderate difficulties [$t(19) = 4.29$, $p = 0.001$, $r = 0.701$], the low and high difficulties [$t(19) = 5.44$, $p < 0.001$, $r = 0.780$], and the moderate and high difficulties [$t(19) = 3.81$, $p = 0.004$, $r = 0.658$]. The difficulty × muscle interaction did not reach significance [$F(2, 38) = 0.376$, $p = 0.689$, $\eta_p^2 = 0.019$]. For the PT (**Figure 6C**), the mean RMS EMG of the biceps brachii was higher than the mean RMS EMG of the triceps [$F(1, 19) = 15.95$, $p < 0.001$, $\eta_p^2 = 0.456$]. There was a main effect of difficulty [$F(1.21, 22.95) = 132.51$, $p < 0.001$, $\eta_p^2 = 0.875$], showing an increase between the low and moderate difficulties [$t(19) = 9.43$, $p < 0.001$, $r = 0.908$], the low and high difficulties [$t(19) = 12.07$, $p < 0.001$, $r = 0.941$], and the moderate and high difficulties [$t(19) = 11.33$, $p < 0.001$, $r = 0.933$]. The difficulty × muscle interaction reached significance [$F(1.28,

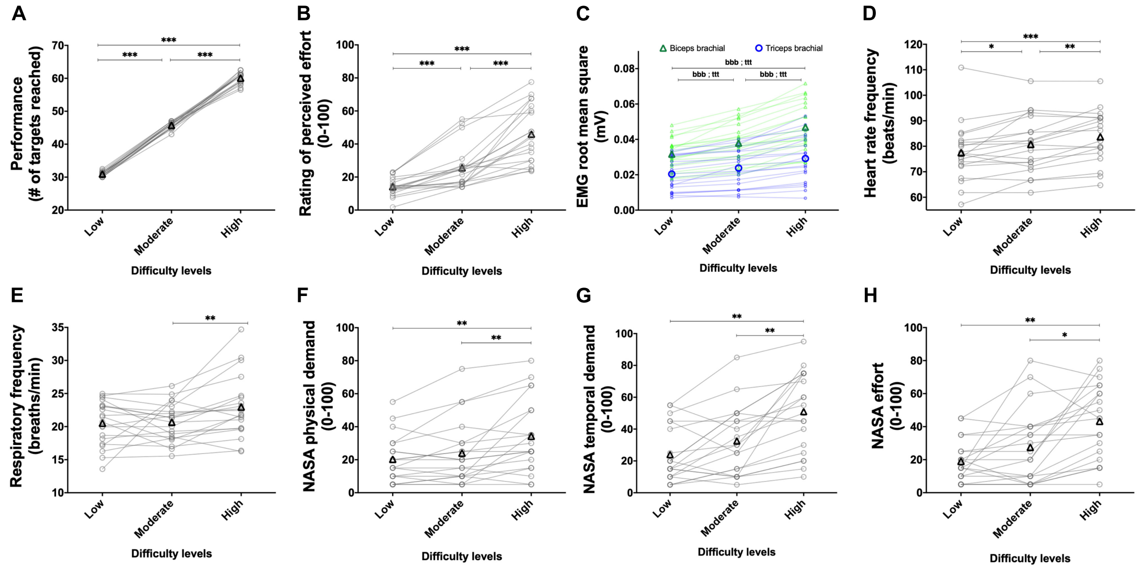

FIGURE 6
Experiment 1B: Manipulating the tempo to alter task difficulty during the pointing task. Effect of manipulating the tempo during the pointing task on performance (**A**, n = 20), rating of perceived effort (**B**, n = 20), EMG root mean square of the biceps (green line) and triceps (blue line) brachial muscles (**C**, n = 20), heart rate frequency (**D**, n = 18), respiratory frequency (**E**, n = 20) and NASA TLX scores for physical demand (**F**, n = 20), temporal demand (**G**, n = 20) and subjective effort (**H**, n = 20). For the low difficulty, a 1 Hz tempo was imposed. For the moderate difficulty, a 1.5 Hz tempo was imposed. For the high difficulty, a 2 Hz tempo was imposed. Data are presented as the main effect of difficulty, except for panel C presenting the difficulty × muscle interaction. The n indicates the number of participants with all the data in each of the three levels of difficulties. Changes in the n reflect data loss due to issues with equipment or movement artifact. Individual data are presented in light markers and means in dark markers. *Main effect of difficulty, the difference between two difficulty levels. *b* and *t* difference between two difficulty levels for the biceps and triceps brachial muscles, respectively. One symbol: p < 0.05, two symbols: p < 0.01, and three symbols: p < 0.001.

24.31) = 7.26, p = 0.008, η_p^2 = 0.276]. Follow-up tests are presented in Figure 6C.

3.1.2.4. Heart rate frequency

Despite controlling for movement artifacts, data were lost in two participants during the BBT and two participants during the PT, both during the completion of the high difficulty. For the BBT (Figure 5D), manipulation of the tempo increased the heart rate frequency [$F(2, 34)$ = 9.826, p < 0.001, η_p^2 = 0.366]. Heart rate frequency increased between the low and moderate difficulties [$t(19)$ = 2.517, p < 0.001, r = 0.500], between the low and high difficulties [$t(17)$ = 3.861, p < 0.001, r = 0.684], as well as between the moderate and high difficulties [$t(17)$ = 2.297, p < 0.001, r = 0.487]. For the PT (Figure 6D), manipulation of the tempo increased the heart rate frequency too [$F(2, 34)$ = 15.707, p < 0.001, η_p^2 = 0.480]. Heart rate frequency increased between the low and moderate difficulties [$t(19)$ = 2.707, p = 0.042, r = 0.528], between the low and high difficulties [$t(17)$ = 4.911, p < 0.001, r = 0.766], and between the moderate and high difficulties [$t(17)$ = 3.604, p = 0.007, r = 0.658].

3.1.2.5. Respiratory frequency

For the BBT (Figure 5E), manipulation of the tempo increased the respiratory frequency [$F(2, 38)$ = 10.5, p < 0.001, η_p^2 = 0.355]. The increase in respiratory frequency between the low and moderate difficulties did not reach significance [$t(19)$ = 2.373, p = 0.085, r = 0.478]. Respiratory frequency increased between the low and high difficulties [$t(19)$ = 3.797, p = 0.004, r = 0.657] as well as between the moderate and high difficulties [$t(19)$ = 2.8, p = 0.036, r = 0.537]. For the PT (Figure 6E), manipulation of the tempo increased the respiratory frequency too [$F(2, 38)$ = 5.3, p = 0.009, η_p^2 = 0.219]. Respiratory frequency increased between the moderate and high difficulties [$t(19)$ = 3.380, p = 0.009, r = 0.613]. The increase in respiratory frequency neither reached significance between the low and high difficulties [$t(19)$ = 2.391, p = 0.082, r = 0.481] nor between the low and moderate difficulties [$t(19)$ = 0.184, p = 1.000, r = 0.042].

3.1.2.6. NASA TLX scale, physical demand

For the BBT (Figure 5F), manipulation of the tempo increased the physical demand score [$\chi^2(2)$ = 17.815, p < 0.001]. The increase in physical demand score between the easy and medium difficulties did not reach significance (Z = 2.213, p = 0.081, r = 0.350). The physical demand score increased between the low and high difficulties (Z = 3.307, p = 0.003, r = 0.523) as well as between the moderate and high difficulties (Z = 3.051, p = 0.007, r = 0.482). For the PT (Figure 6F), manipulation of the tempo increased the physical demand score too [$\chi^2(2) = 14.464$, $p = 0.001$]. The physical demand score did not increase between the low and moderate difficulties ($Z = 1.690$, $p = 0.273$, $r = 0.267$). The physical demand score increased between the low and high difficulties ($Z = 3.354$, $p = 0.002$, $r = 0.530$) as well as between the moderate and high difficulties ($Z = 3.066$, $p = 0.007$, $r = 0.485$).

3.1.2.7. NASA TLX scale, mental demand

For the BBT, manipulation of the tempo increased the mental demand score [$\chi^2(2) = 15.672$, $p < 0.001$]. The increase in mental demand score between the low (19.5 ± 17.2 a.u.) and moderate (24.3 ± 16.2 a.u.) difficulties did not reach significance ($Z = 1.825$, $p = 0.204$, $r = 0.289$). Mental demand score increased between the low and high (35.3 ± 23.3 a.u.) difficulties ($Z = 3.196$, $p = 0.004$, $r = 0.505$) and between the moderate and high difficulties ($Z = 3.219$, $p = 0.004$, $r = 0.509$). For the PT, manipulation of the tempo increased the mental demand score [$\chi^2(2) = 12.649$, $p = 0.002$]. The increase in mental demand score between the low (22.3 ± 12.4 a.u.) and moderate (30.5 ± 21.0 a.u.) difficulties did not reach significance ($Z = 1.556$, $p = 0.359$, $r = 0.246$). The mental demand score increased between the low and high (40.8 ± 22.6 a.u.) difficulties ($Z = 3.012$, $p = 0.008$, $r = 0.476$) and between the moderate and high difficulties ($Z = 2.710$, $p = 0.020$, $r = 0.428$).

3.1.2.8. NASA TLX scale, temporal demand

For the BBT (**Figure 5G**), manipulation of the tempo increased the temporal demand score [$\chi^2(2) = 7.28$, $p = 0.026$]. The temporal demand score neither increased between the low and moderate difficulties ($Z = 0.572$, $p = 1.000$, $r = 0.090$) nor between the low and high difficulties ($Z = 2.194$, $p = 0.085$, $r = 0.347$). The temporal demand score significantly increased between the moderate and high difficulties ($Z = 2.686$, $p = 0.022$, $r = 0.425$). For the PT (**Figure 6G**), manipulation of the tempo increased the temporal demand score too [$\chi^2(2) = 23.792$, $p < 0.001$]. The increase in temporal demand score between the low and moderate difficulties did not reach significance ($Z = 2.144$, $p = 0.096$, $r = 0.339$). The temporal demand score significantly increased between the low and high difficulties ($Z = 3.712$, $p = 0.001$, $r = 0.587$) as well as between the moderate and high difficulties ($Z = 3.736$, $p = 0.001$, $r = 0.591$).

3.1.2.9. NASA TLX scale, effort

For the BBT (**Figure 5H**), manipulation of the tempo increased the effort score [$\chi^2(2) = 18.123$, $p < 0.001$]. Effort score did not increase between the low and moderate difficulties ($Z = 0.177$, $p = 1.000$, $r = 0.028$) but did so between the low and high difficulties ($Z = 3.184$, $p = 0.004$, $r = 0.503$), as well as between the moderate and high difficulties ($Z = 3.202$, $p = 0.004$, $r = 0.506$). For the PT (**Figure 6H**), manipulation of the tempo increased the effort demand score too [$\chi^2(2) = 22.776$, $p < 0.001$]. Effort score did not increase between the low and moderate difficulties ($Z = 1.759$, $p = 0.236$, $r = 0.278$) but did so between the low and high difficulties ($Z = 3.637$, $p = 0.001$, $r = 0.575$), as well as between the moderate and high difficulties ($Z = 2.882$, $p = 0.012$, $r = 0.456$).

3.1.2.10. VAS fatigue

Feelings of fatigue did not increase during the tempo session (from 2.9 ± 2.2 to 3.2 ± 1.9; $Z = 0.952$, $p = 0.340$).

3.1.3. Experiment 1B: Adding weight on the forearm to alter task difficulty

The results for the BBT and PT during the weight sessions are presented in **Figures 7** and **8**, respectively.

3.1.3.1. Performance

For the BBT (**Figure 7A**) and PT (**Figure 8A**), manipulation of the weight did not alter performance [BBT, $\chi^2(2) = 4.899$, $p = 0.086$; PT, $\chi^2(2) = 2.032$, $p = 0.362$].

3.1.3.2. Perception of effort

For the BBT (**Figure 7B**), manipulation of the weight increased the rating of perceived effort [$\chi^2(2) = 36.026$, $p < 0.001$]. Rating of perceived effort increased between the low and moderate difficulties ($Z = 3.341$, $p = 0.003$, $r = 0.528$), between the low and high difficulties ($Z = 3.921$, $p < 0.001$, $r = 0.620$), and between the moderate and high difficulties ($Z = 3.624$ $p = 0.001$, $r = 0.573$). For the PT (**Figure 8B**), manipulation of the weight increased the rating of perceived effort too [$\chi^2(2)$. $= 32.076$, $p < 0.001$]. Rating of perceived effort increased between the low and moderate difficulties ($Z = 3.324$, $p = 0.003$, $r = 0.526$), between the low and high difficulties ($Z = 3.920$, $p < 0.001$, $r = 0.620$), and between the moderate and high difficulties ($Z = 3.502$, $p = 0.001$, $r = 0.554$).

3.1.3.3. RMS EMG

For the BBT (**Figure 7C**), the mean RMS EMG of the biceps brachii was higher than the mean RMS EMG of the triceps [$F(1, 19) = 11.339$, $p = 0.003$, $\eta_p^2 = 0.374$]. There was a main effect of difficulty [$F(1.27, 24.08) = 25.276$, $p < 0.001$, $\eta_p^2 = 0.571$] showing an increase between the low and moderate difficulties [$t(19) = 2.954$, $p = 0.024$, $r = 0.561$], between the moderate and high difficulties [$t(19) = 7.065$, $p < 0.001$, $r = 0.851$] as well as between the low and high difficulties [$t(19) = 5.499$, $p < 0.001$, $r = 0.784$]. The difficulty × muscle interaction reached significance [$F(2, 38) = 14.857$, $p < 0.001$, $\eta_p^2 = 0.438$]. Follow-up tests are presented in **Figure 7C** for the PT (**Figure 8C**), the mean RMS EMG of the biceps brachii was higher than the mean RMS EMG of the triceps [$F(1, 19) = 11.001$, $p = 0.003$, $\eta_p^2 = 0.367$]. There was a main effect of difficulty [$F(1.33, 25.20) = 13.148$, $p < 0.001$, $\eta_p^2 = 0.409$] showing an increase and between the moderate and high difficulties [$t(19) = 3.974$, $p < 0.01$, $r = 0.674$] and between the low and high difficulties [$t(19) = 3.686$, $p < 0.01$, $r = 0.646$], but not between the low and moderate difficulties [$t(19) = 0.048$, $p > 0.05$, $r = 0.011$]. The difficulty × muscle interaction reached

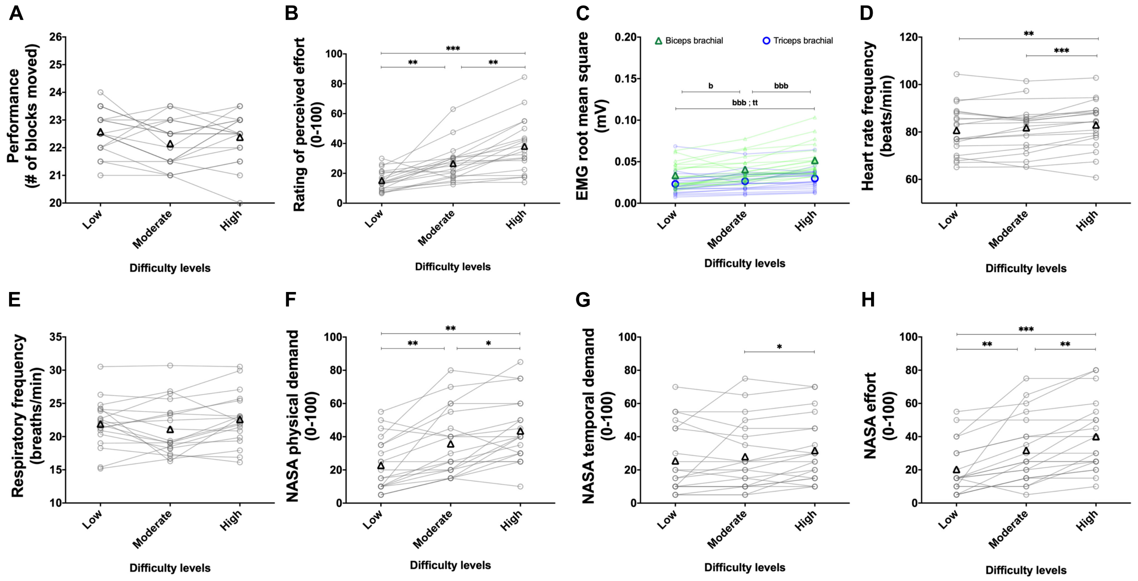

FIGURE 7
Experiment 1B: Adding weight on the forearm to alter task difficulty during the box and block test. The effect of manipulating the weight during the box and block test on performance (**A**, n = 20), rating of perceived effort (**B**, n = 20), EMG root mean square of the biceps (green line) and triceps (blue line) brachial muscles (**C**, n = 20), heart rate frequency (**D**, n = 16), respiratory frequency (**E**, n = 20) and NASA TLX scores for the physical demand (**F**, n = 20), the temporal demand (**G**, n = 20), and the subjective effort (**H**, n = 20). Movements were performed at a fixed tempo of 0.75 Hz. For the low difficulty, no additional weight on the forearm was added. For the moderate difficulty, a weight of 0.5 kg was added. For the high difficulty, a weight of 1 kg was added. Data are presented as the main effect of difficulty, except for panel **(C)** presenting the difficulty × muscle interaction. The n indicates the number of participants with all the data in each of the three levels of difficulties. Changes in the n reflect data loss due to issues with equipment or movement artifact. Individual data are presented in light markers and means in dark markers. *Main effect of difficulty, the difference between two difficulty levels. *b* and *t* difference between two difficulty levels for the biceps and triceps brachial muscles, respectively. One symbol: $p < 0.05$, two symbols: $p < 0.01$, and three symbols: $p < 0.001$.

significance [$F(1.30, 24.74) = 48.057$, $p < 0.001$, $\eta_p^2 = 0.717$]. Follow-up tests are presented in **Figure 8C**.

3.1.3.4. Heart rate frequency

Despite controlling for movement artifacts, data were lost during the BBT in four participants during the completion of the moderate difficulty and in one participant during the completion of the high difficulty. During the PT, data were lost in two participants during the completion of the low difficulty, in one participant during the completion of the moderate difficulty and in one participant during the completion of the high difficulty. For the BBT (**Figure 7D**), manipulation of the weight increased the heart rate frequency [$F(2, 30) = 13.758$, $p < 0.001$, $\eta_p^2 = 0.478$]. Heart RM rate frequency did not increase between the low and moderate difficulties [$t(15) = 0.748$, $p = 1.000$, $r = 0.190$] but did so between the low and high difficulties [$t(15) = 4.213$, $p = 0.002$, $r = 0.736$], as well as between the moderate and high difficulties [$t(15) = 5.115$, $p < 0.001$, $r = 0.797$]. For the PT (**Figure 8D**), manipulation of the weight significantly increased the heart rate frequency too [$F(2, 32) = 11.257$, $p < 0.001$, $\eta_p^2 = 0.413$]. The increase in the heart rate frequency between the low and moderate difficulties [$t(16) = 2.636$, $p = 0.054$, $r = 0.550$] as well as between the moderate and high difficulties [$t(16) = 2.541$, $p = 0.065$, $r = 0.536$] did not reach significance. Heart rate frequency significantly increased between the low and high difficulties [$t(16) = 4.190$, $p = 0.002$, $r = 0.723$].

3.1.3.5. Respiratory frequency

During the BBT, data were lost in one participant during the completion of both the low and high difficulties. During the PT, data were lost in one participant for the three difficulties and in one participant during the high difficulty. For the BBT (**Figure 7E**) and PT (**Figure 8E**), manipulation of the weight did not alter respiratory frequency [BBT, $F(2, 36) = 1.931$, $p = 0.159$, $\eta_p^2 = 0.097$; PT, $F(2, 34) = 1.477$, $p = 0.243$, $\eta_p^2 = 0.080$].

3.1.3.6. NASA TLX scale, and physical demand

For the BBT (**Figure 7F**), manipulation of the weight increased the physical demand score [$\chi^2(2) = 18.2$, $p < 0.001$]. Physical demand score increased between the low and moderate difficulties ($Z = 3.373$, $p = 0.002$, $r = 0.533$), between the moderate and high difficulties ($Z = 2.630$, $p = 0.026$, $r = 0.416$), and between the low and high difficulties ($Z = 3.497$, $p = 0.001$, $r = 0.553$). For the PT (**Figure 8F**), manipulation of the weight significantly increased the physical demand score too [$\chi^2(2) = 35.351$, $p < 0.001$]. Physical demand score increased between the low and moderate difficulties

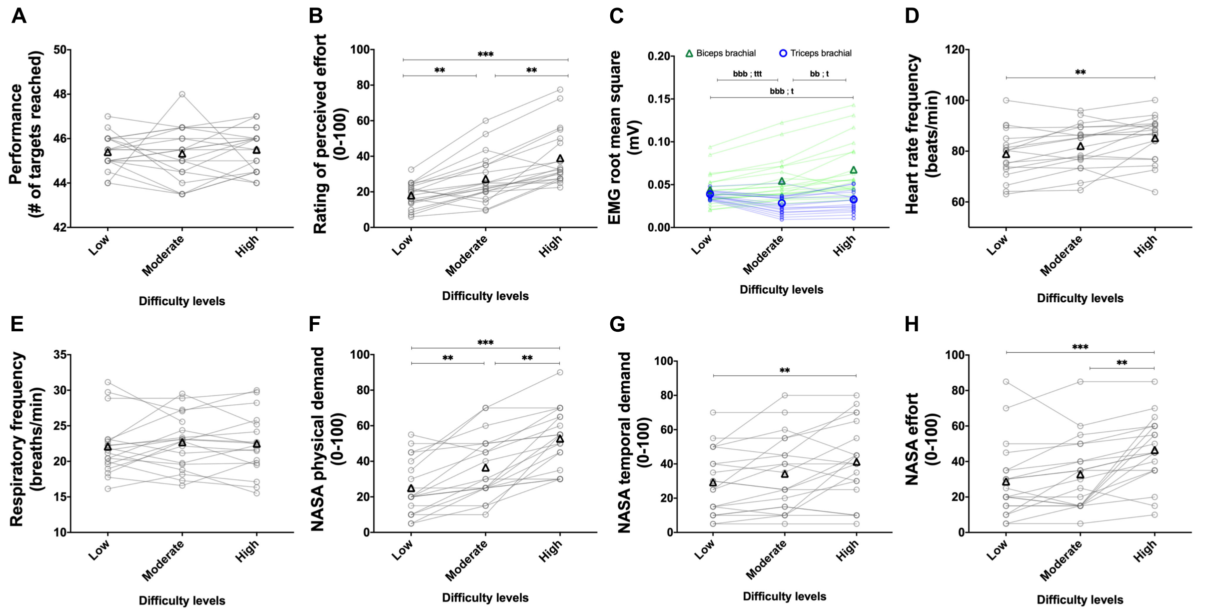

FIGURE 8
Experiment 1B: Adding weight on the forearm to alter task difficulty during the pointing task. Effect of manipulating the weight during the pointing Task on performance (**A**, $n = 20$), rating of perceived effort (**B**, $n = 20$), EMG root mean square of the biceps (green line) and triceps (blue line) brachial muscles (**C**, $n = 20$), heart rate frequency (**D**, $n = 17$), respiratory frequency (**E**, $n = 20$) and NASA TLX scores for the physical demand (**F**, $n = 20$), the temporal demand (**G**, $n = 20$) and the subjective effort (**H**, $n = 20$). Movements were performed at a fixed tempo of 1.5 Hz. For the low difficulty, no additional weight on the forearm was added. For the moderate difficulty, a weight of 0.5 kg was added. For the high difficulty, a weight of 1 kg was added. Data are presented as the main effect of difficulty, except for panel **(C)** presenting the difficulty × muscle interaction. The n indicates the number of participants with all the data in each of the three levels of difficulties. Changes in the n reflect data loss due to the equipment. Individual data are presented in gray circles and means in black triangles. *Main effect of difficulty, the difference between two difficulty levels. *b* and *t* difference between two difficulty levels for the biceps and triceps brachial muscles, respectively. One symbol: $p < 0.05$, two symbols: $p < 0.01$, and three symbols: $p < 0.001$.

($Z = 3.218$, $p = 0.004$, $r = 0.509$), between the moderate and high difficulties ($Z = 3.734$ $p = 0.001$, $r = 0.590$), and between the low and high difficulties ($Z = 3.930$, $p < 0.001$, $r = 0.621$).

3.1.3.7. NASA TLX scale, mental demand

For the BBT, manipulation of the tempo increased the mental demand score [$\chi^2(2) = 8.400$, $p = 0.015$]. The mental demand score increased between the low (22.5 ± 15.6 a.u.) and moderate (29.3 ± 17.6 a.u.) difficulties ($Z = 2.695$, $p = 0.021$, $r = 0.426$) as well as between the low and high (29.8 ± 19.3 a.u.) difficulties ($Z = 2.435$, $p = 0.045$, $r = 0.385$). The mental demand score did not increase between the moderate and high difficulties ($Z = 0.109$, $p = 1.000$, $r = 0.017$). For the PT, manipulation of the tempo increased the mental demand score [$\chi^2(2) = 7.750$, $p = 0.021$]. The increase in the mental demand score between the low (27.3 ± 14.0 a.u.) and moderate (36.5 ± 21.5 a.u.) difficulties did not reach significance ($Z = 2.226$, $p = 0.078$, $r = 0.352$). The mental demand score increased between the low and high (42.5 ± 17.9 a.u.) difficulties ($Z = 3.274$, $p = 0.003$, $r = 0.518$). The mental demand score did not increase between the moderate and high difficulties ($Z = 1.706$, $p = 0.264$, $r = 0.270$).

3.1.3.8. NASA TLX scale, temporal demand

For the BBT (**Figure 7G**), manipulation of the weight increased the temporal demand score [$\chi^2(2). = 7$, $p = 0.031$]. The temporal demand score did not increase between the low and moderate difficulties ($Z = 0.361$, $p = 1.000$, $r = 0.057$), as well as between the low and high difficulty ($Z = 1.934$, $p = 0.159$, $r = 0.306$), but increased between the moderate and high difficulty ($Z = 2.423$, $p = 0.046$, $r = 0.383$). For the PT (**Figure 8G**), manipulation of the weight increased the temporal demand score too [$\chi^2(2) = 8.222$, $p = 0.016$]. The temporal demand score did not increase between the low and moderate difficulties ($Z = 2.042$, $p = 0.123$, $r = 0.323$), as well as between the moderate and high difficulties ($Z = 2.110$, $p = 0.105$, $r = 0.334$), but increased between the low and high difficulties ($Z = 3.086$, $p = 0.006$, $r = 0.488$).

3.1.3.9. NASA TLX scale, effort

For the BBT (**Figure 7H**), manipulation of the weight increased the effort score [$\chi^2(2) = 28.353$, $p < 0.001$]. The effort score increased between the low and moderate difficulties ($Z = 3.309$, $p = 0.003$, $r = 0.523$), between the moderate and high difficulties ($Z = 3.225$, $p = 0.004$, $r = 0.510$), as well as between the low and high difficulties ($Z = 3.798$, $p < 0.001$, $r = 0.601$). For the PT (Figure 8H), manipulation of the weight increased the effort score [$\chi^2(2) = 25.507$, $p < 0.001$]. The effort score did not increase between the low and moderate difficulties ($Z = 1.720$, $p = 0.256$, $r = 0.272$), but did so between the moderate and high difficulties ($Z = 3.362$, $p = 0.002$, $r = 0.532$), as well as between the low and high difficulties ($Z = 3.604$, $p = 0.001$, $r = 0.570$).

3.1.3.10. VAS fatigue

Feelings of fatigue increased during the tempo session (from 3.1 ± 2.3 to 3.9 ± 1.9; $Z = 2.315$, $p = 0.021$).

3.2. Experiment 2

In this experiment, participants visited the laboratory once. In Experiment 2A, we prescribed 30 s of exercise with the BBT performed at four intensities of effort (light, moderate, strong, and very strong). Performance, RMS EMG, and heart rate frequency were monitored for each prescribed effort intensity. Then, in Experiment 2B, we manipulated task difficulty (low, high) by adding two different weights on the participant's dominant forearm while performing the standardized 60 s BBT. Each level of difficulty was repeated twice. Performance, rating of perceived effort, RMS EMG heart rate frequency, and the subjective workload were measured for each repetition of each level of difficulty.

3.2.1. Experiment 2A: Using the perception of effort to prescribe the exercise

Results are presented in Figure 9.

3.2.1.1. Performance

Increasing the prescribed effort intensity resulted in an increased performance [$F(1.7, 31.6) = 168.560$, $p < 0.001$, $\eta_p^2 = 0.899$; Figure 9A]. Performance increased between the light and moderate effort intensities [$t(19) = 11.393$, $p < 0.001$, $r = 0.934$], between moderate and strong effort intensities [$t(19) = 12.564$, $p < 0.001$, $r = 0.945$], and between strong and very strong effort intensities [$t(19) = 4.258$, $p = 0.001$, $r = 0.699$].

3.2.1.2. RMS EMG

Mean RMS EMG of the biceps brachii was lower than the mean RMS EMG of the triceps [$F(1, 19) = 11.285$, $p = 0.003$, $\eta_p^2 = 0.373$]. There was a main effect of effort intensity [$F(1.41, 26.76) = 36.852$, $p < 0.001$, $\eta_p^2 = 0.659$], showing an increase between the light and moderate intensities [$t(19) = 4.471$, $p < 0.001$, $r = 0.716$], between the moderate and strong intensities [$t(19) = 5.235$, $p < 0.001$, $r = 0.769$], and between the strong and very strong [$t(19) = 4.310$, $p = 0.001$, $r = 0.703$].]. The muscle x effort intensity interaction reached significance [$F(1.45, 27.56) = 38.540$, $p < 0.001$, $\eta_p^2 = 0.670$]. Follow-up tests are presented in Figure 9B.

3.2.1.3. Heart rate frequency

Increasing the prescribed effort intensity resulted in an increased heart rate [$F(3, 57) = 29.074$, $p < 0.001$, $\eta_p^2 = 0.605$; Figure 9C]. The increase in heart rate frequency between the light and moderate effort intensities did not reach significance [$t(19) = 2.316$, $p = 0.096$, $r = 0.469$]. Heart rate frequency significantly increased between the moderate and strong difficulty [$t(19) = 4.027$, $p = 0.002$, $r = 0.679$], and between strong and very strong effort intensities [$t(19) = 2.925$, $p = 0.026$, $r = 0.557$].

3.2.2. Experiment 2B: Effects of adding weight on the forearm when completing the box and block test with the standardized instructions

The results of the main effects of difficulty are presented in Figure 10.

3.2.2.1. Performance

The main effect of repetition revealed a greater performance in the second repetition compared to the first repetition [$F(1, 19) = 34.836$, $p < 0.001$, $\eta_p^2 = 0.647$]. The main effect of difficulty did not reach significance [$F(1, 19) = 1.867$, $p = 0.188$, $\eta_p^2 = 0.090$; Figure 10A]. The repetition × difficulty interaction reached significance [$F(1, 19) = 5.166$, $p = 0.035$, $\eta_p^2 = 0.214$]. Follow-up tests revealed an increased performance between the first and second repetitions for both the low {from 84.3 ± 6.6 to 89.7 ± 8.0; [$t(19) = 5.219$, $p < 0.001$, $r = 0.768$]} and high {from 84.0 ± 7.0 to 86.8 ± 6.9; [$t(19) = 3.667$, $p = 0.005$, $r = 0.644$]} difficulties. Performance did not differ for the first repetition between the low and high difficulties {84.3 ± 6.6 and 84.0 ± 7.0; [$t(19) = 0.188$, $p = 1.000$, $r = 0.043$]}. During the second repetition, performance did not significantly decrease between the low and high difficulties {89.7 ± 8.0 and 86.8 ± 6.9; [$t(19) = 2.316$, $p = 0.096$, $r = 0.469$]}.

3.2.2.2. Perception of effort

The main effect of repetition revealed a higher rating of perceived effort in the second repetition compared to the first repetition [$F(1, 19) = 14.350$, $p = 0.001$, $\eta_p^2 = 0.430$]. The main effect of difficulty revealed an increase in the rating of perceived effort with the increase in difficulty [$F(1, 19) = 6.779$, $p = 0.017$, $\eta_p^2 = 0.263$; Figure 10B]. The repetition × difficulty interaction did not reach significance [$F(1, 19) = 0.005$, $p = 0.946$, $\eta_p^2 < 0.001$].

3.2.2.3. RMS EMG

The main effect of muscle did not reach significance [$F(1, 19) = 3.024$, $p = 0.098$, $\eta_p^2 = 0.137$]. The main effect of repetition revealed a higher RMS EMG in the second repetition compared to the first repetition [$F(1, 19) = 11.677$, $p = 0.003$, $\eta_p^2 = 0.381$]. The main effect of difficulty revealed an increase in RMS EMG with the increase in difficulty [$F(1, 19) = 14.289$, $p = 0.001$, $\eta_p^2 = 0.429$]. The

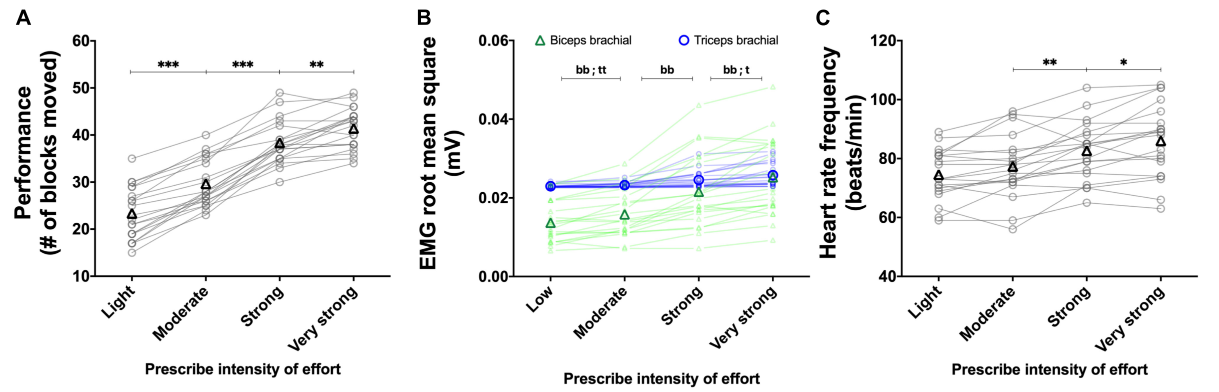

FIGURE 9
Experiment 2A: Using the perception of effort to prescribe the exercise during the box and block test. Effect of increasing the prescribed intensity of effort on performance (**A**, $n = 20$), EMG root mean square of the biceps (green line) and triceps (blue line) brachial muscles (**B**, $n = 20$), heart rate frequency (**C**, $n = 20$) during the box and block test. The exercise was prescribed at four intensities of perceived effort *via* the CR100 scale: light (13/100), moderate (23/100), strong (50/100), and very strong (70/100). Data are presented as the main effect of effort intensity, except for panel **(B)** presenting the effort intensity × muscle interaction. Individual data are presented in light markers and means in dark markers. *Main effect of difficulty, the difference between two difficulty levels. *b* and *t* are the difference between two difficulty levels for the biceps and triceps brachial muscles, respectively. One symbol: $p < 0.05$, two symbols: $p < 0.01$, and three symbols: $p < 0.001$.

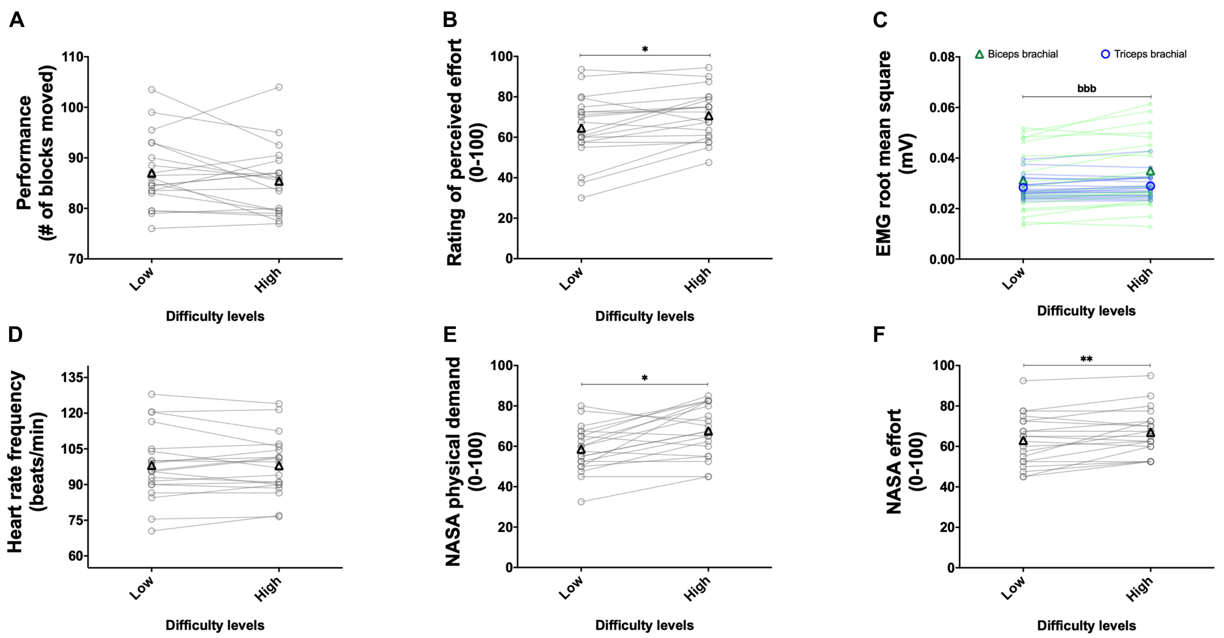

FIGURE 10
Experiment 2B: Adding weight on the forearm to alter task difficulty during the box and block test with its validated instructions. Effect of weight manipulation on performance (**A**, $n = 20$), rating of perceived effort (**B**, $n = 20$), EMG root mean square of the biceps (green line) and triceps (blue line), brachial muscles (**C**, $n = 20$), heart rate frequency (**D**, $n = 20$), and NASA TLX scores for physical demand (**E**, $n = 20$) and effort (**F**, $n = 20$) during the box and block test with its official instructions. Data are presented as the main effect of difficulty, except for panel **(C)** presenting the effort difficulty × muscle interaction. Individual data are presented in light markers and means in dark markers. *Main effect of difficulty, the difference between two difficulty levels. *b* is the difference between two difficulty levels for the biceps and triceps brachial muscles, respectively. One symbol: $p < 0.05$ and two symbols: $p < 0.01$.

muscle × difficulty interaction reached significance [$F(1, 19) = 20.525$, $p < 0.001$, $\eta_p^2 = 0.519$], follow-up tests are presented in Figure 10C. The muscle × repetition interaction [$F(1, 19) = 0.378$, $p = 0.546$, $\eta_p^2 = 0.019$], difficulty × repetition interaction [$F(1, 19) < 0.001$, $p = 0.978$, $\eta_p^2 < 0.001$], and muscle × difficulty × repetition interaction [$F(1, 19) = 0.032$, $p = 0.860$, $\eta_p^2 = 0.002$] did not reach significance.

3.2.2.4. Heart rate frequency

Main effect of repetition [$F(1, 19) = 1.094$, $p = 0.309$, $\eta_p^2 = 0.054$], difficulty [$F(1, 19) = 0.664$, $p = 0.425$, $\eta_p^2 = 0.034$; **Figure 10D**], and repetition × difficulty interaction [$F(1, 19) = 0.492$, $p = 0.492$, $\eta_p^2 = 0.025$] did not reach significance.

3.2.2.5. NASA TLX scale, physical demand

The main effect of repetition revealed a higher physical demand score in the second repetition compared to the first repetition [$F(1, 19) = 20.328$, $p < 0.001$, $\eta_p^2 = 0.517$]. The main effect of difficulty revealed an increase in physical demand score with the increase in difficulty [$F(1, 19) = 13.426$, $p = 0.002$, $\eta_p^2 = 0.414$; **Figure 10E**]. The repetition × difficulty interaction did not reach significance [$F(1, 19) = 1.342$, $p = 0.261$, $\eta_p^2 = 0.066$].

3.2.2.6. NASA TLX scale, mental demand

The main effect of repetition revealed a higher mental demand score in the second (49.4 ± 28.4 a.u.) repetition compared to the first (44.8 ± 25.8 a.u.) repetition [$F(1, 19) = 4.916$, $p = 0.039$, $\eta_p^2 = 0.206$]. Neither the main effect of difficulty [$F(1, 19) = 0.514$, $p = 0.482$, $\eta_p^2 = 0.026$] nor the difficulty × repetition interaction [$F(1, 19) = 0.112$, $p = 0.742$, $\eta_p^2 = 0.006$] reached significance.

3.2.2.7. NASA TLX scale, effort

The main effect of repetition did not reach significance [$F(1, 19) = 2.664$, $p = 0.119$, $\eta_p^2 = 0.123$]. The main effect of difficulty revealed an increase in effort score with the increase in difficulty [$F(1, 19) = 8.780$, $p = 0.008$, $\eta_p^2 = 0.316$; **Figure 10F**]. The repetition × difficulty interaction did not reach significance [$F(1, 19) = 0.039$, $p = 0.846$, $\eta_p^2 = 0.002$].

3.2.2.8. VAS fatigue

Feelings of fatigue did not increase during the session (from 5.75 ± 0.6 to 5 ± 1.7; $Z = 1.916$, $p = 0.055$).

4. Discussion

In this study, we investigated the possibility to prescribe and monitor exercise with the perception of effort during two upper-limb motor tasks: the box and block test and a pointing task. Our results suggest that performance in both tasks increased when the perception of effort intensity used to prescribe the exercise increased. When the task difficulty was altered by manipulating the physical demand *via* different tempos or weights added on the forearm, our results suggest that perception of effort increased when task difficulty increased and that performance could be maintained at a cost of a higher perception of effort. This increased perception of effort was observed during both the modified version of the box and block test as well as the pointing task performed in experiment 1. Finally, when completing the standardized version of the box and block test in the absence and presence of additional weight on the forearm, in experiment 2, we observed a maintained performance at a cost of a higher perception of effort. Overall, the results from both experiments suggest that perception of effort can be efficiently used in healthy young adults to prescribe and monitor physical resources allocation during upper-limb motor tasks.

4.1. Perception of effort can be used to prescribe the exercise intensity of upper-limb motor tasks

Perception of effort is widely used in the field of exercise sciences to prescribe exercise (Borg, 1998; Eston and Parfitt, 2018). As an example, the intensity of perception of effort has been used to prescribe locomotor exercise such as running or cycling (e.g., Christian et al., 2014; Hobbins et al., 2019), and resistance exercise involving the upper and lower limb (e.g., Gearhart et al., 2009; Zourdos et al., 2016; Helms et al., 2017). However, to the best of our knowledge, the possibility to use the intensity of perception of effort for exercise prescription in the context of upper-limb motor tasks remains untested. As the intensity of effort engaged in a task is proposed to determine the performance in this task (Brehm and Self, 1989; Richter et al., 2016), performance should increase when the intensity of perceived effort increases. We tested this possibility in both experiments. In experiment 1, we observed, during the box and block test and a pointing task, a gradual increase in performance between each intensity of perceived effort used to prescribe the exercise. This observation was subsequently reproduced in experiment 2 with another sample of participants performing the regular box and block test. Therefore, as previously observed during locomotor exercise or resistance exercise, our results suggest that the intensity of perceived effort could be an efficient tool to prescribe the exercise during upper-limb motor tasks. Interestingly, we did not observe any main effect of visit on performance for prescribing exercise during upper-limb motor tasks. This result suggests that our familiarization with the CR100 scale and associated instructions, combined with a 1-min practice of the tasks, was sufficient to control for a familiarization effect. In other words, when using the CR100 scale and associated instructions, our results imply that it is not necessary to perform an extensive practice of the motor tasks (e.g., exploring all range of intensity) to use the CR100 in the context of exercise prescription. This result is of great interest for researchers and clinicians willing to explore the use of this scale as it suggests that its use could be time-efficient when an extensive familiarization with the motor task is not possible due to time constraints.

To further confirm the possibility to use the perception of effort to prescribe exercise, we also monitored several physiological responses to the task performed: muscle activation, heart rate, and respiratory frequencies. These physiological responses are known to rise when the intensity of a task is increased during locomotor exercise as well as resistance exercise (de Morree and Marcora, 2010, 2012; Eston and Parfitt, 2018); we, therefore, hypothesized that the physiological responses would rise with the increased perceived effort intensity. As expected, all physiological parameters rose with the increased exercise intensity, confirming an increase in physical resources involved in the upper-limb motor tasks performed when the prescribed perceived effort intensity increased. However, it is crucial to note that solely the muscle activation gradually increased between each prescribed perceived effort intensity. In experiment 1, our planned follow-up tests on the main effect of effort intensity failed to reveal a significant increase in respiratory frequency between each intensity. These tests also revealed that heart rate frequency solely increased between the intensities moderate to strong, and not between the light to moderate and strong to very strong intensities. As upper-limb motor tasks involve a lower muscle mass than locomotor exercise or resistance exercise and increasing the muscle mass involved in a task is known to increase cardiorespiratory responses to the exercise (Sidhu et al., 2013; MacInnis et al., 2017), the lack of observed increase between intensities in heart rate frequency and respiratory frequency in our study may be due to the low muscle mass involved in the tasks performed. In experiment 2, we used a chest belt to better control movement artifact and increase the quality of our heart rate frequency measurement. Using the chest belt, compared to the finger pulse transducer, allowed us to avoid data loss and capture an increased heart rate frequency between the moderate to strong and strong to very strong intensities, but not between the light to moderate intensities. Consequently, by integrating the two experiments, our results suggest that when prescribing the exercise during upper-limb motor tasks with the intensity of perceived effort, researchers and clinicians should prioritize the use of EMG over heart rate and respiratory frequencies to monitor physiological changes in the physical resources engaged in the task.

4.2. Perception of effort changes with the manipulation of physical demand

Perception of effort is not only used to prescribe the exercise but also to monitor the exercise (Borg, 1998; Eston and Parfitt, 2018). Indeed, the intensity of perception of effort during a motor task has been extensively shown to be responsive to changes in task difficulty imposed by various experimental manipulations. As an example, the perception of effort is altered by the intensity of muscle contraction (e.g., de Morree and Marcora, 2010, 2012), the presence of muscle or mental fatigue (e.g., Pageaux and Lepers, 2016, 2018; Jacquet et al., 2021), or changes in environmental conditions (e.g., Girard and Racinais, 2014; Borg et al., 2018; Jeffries et al., 2019). In our study, to test the possibility to monitor the exercise intensity during upper-limb motor tasks, we altered task difficulty by manipulating the physical demand of the tasks performed *via* imposing various movement tempos or adding weights on the forearm. We expected the perception of effort to raise with task difficulty, regardless of the type of physical demand manipulation used.

In experiment 1, during the tempo session, we manipulated the physical demand of the task by imposing three different movement speeds to complete the box and block test and pointing task. The increased number of blocks moved during the box and block test and targets reached during the pointing task confirmed that we were successful in our experimental manipulation. We observed an increased perception of effort between each task difficulty, suggesting the possibility to track changes in task difficulty imposed by changes in movement speed during upper-limb motor tasks. This increased perception of effort was associated with consistently increased muscle activation and heart rate frequency during both tasks. During the weight session, we manipulated the physical demand of the task by adding weights on the forearm and imposing a single movement tempo to constrain performance across task difficulties. The lack of changes in performance in both tasks across difficulties confirms that we were successful in our experimental manipulation. In line with the motivational intensity theory (Brehm and Self, 1989; Richter et al., 2016), when task difficulty increases, performance could be maintained by increasing the effort invested in the task. This proposed mechanism to maintain performance is verified in our experiment *via* the increased perception of effort intensity between each task difficulty, suggesting the possibility to track changes in task difficulty imposed by manipulating the weight of the exercising forearm moved during upper-limb motor tasks. The increased muscle activation and heart rate frequency over task difficulties further support the mechanism proposed by the motivational intensity theory. However, it is noticeable that muscle activation consistently increased between difficulties solely in the biceps brachial muscle and not the triceps brachial muscle. This result suggests that researchers and clinicians interested in monitoring EMG as a physiological marker of perception of effort may prioritize the monitoring of the biceps brachial EMG signal.

In experiment 2, we performed the standardized version of the box and block test by adding a weight on the forearm to increase task difficulty. Neither performance nor movement speed was controlled, the participants had to move as many blocks as possible in 60 s. In this specific experimental paradigm, the motivational intensity theory would predict two possible outcomes (Brehm and Self, 1989; Richter et al., 2016): (i) performance will drop if the increase in task difficulty is beyond the participant's capacity, or (ii) performance will be maintained if the increase in task difficulty is within the participant's capacity, and this maintained performance will be possible at a cost of a higher effort invested in the task. As our participants were young and healthy, and the weight added to the forearm was chosen following pilot experiments aiming to limit the development of fatigue, we expected that our participants would be able to maintain performance by increasing the effort invested in the task. In line with our hypothesis performance did not differ between the easy and hard difficulty, and the maintained performance was associated with an increased rating of perceived effort reported by the participants. This increase in perception of effort was associated with increased muscle activation, as observed in experiment 1 to compensate for the heavier forearm to move during the box and block test.

Not all the physiological variables monitored were responsive to changes in task difficulty in both experiments. In experiment 1, the respiratory frequency did not increase between the difficulties easy and medium in both tasks when the physical demand was manipulated with the tempo, and no main effect of task difficulty was observed on respiratory frequency when the physical demand was manipulated with the addition of weight on the forearm. Regarding heart rate frequency, changes in this variable between each difficulty were consistently observed only when the task difficulty was manipulated with the tempo. Furthermore, the increased perception of effort observed in experiment 2 to maintain performance during the box and block test performed with the standardized instructions did not occur in the presence of increased heart rate frequency. These results extend the previous observation of the lack of changes in heart rate frequency and respiratory frequency when the intensity of perceived effort is used to prescribe the exercise and confirm that neither heart rate nor respiratory frequency can be used as an efficient physiological correlate of perception of effort in the context of upper-limb motor tasks. The only parameter responsive to our experimental manipulations was muscle activation, especially biceps brachial muscle activation. Our results suggest that muscle activation of the biceps brachial could be an appropriate physiological marker of the perception of effort during upper-limbs motor tasks. As muscle groups other than the biceps and triceps brachial are involved in the tasks performed, future studies should challenge and extend this observation by measuring activation of other muscle groups during similar tasks (e.g., deltoid muscles). Most likely, the muscles that best quantify effort and correlate with its perception will change with the investigated tasks.

Additionally, it is important to note that we systematically monitored the perceived workload of each task at each difficulty by using the NASA-TLX scale, a validated tool used to monitor perceived workload in various contexts (Hart and Staveland, 1988; Hart, 2006). While this scale captured most manipulations of the physical demand performed in both experiments, a lack of changes in the physical demand score, temporal demand score, or effort score was observed in some experimental conditions. Therefore, our results suggest that the monitoring of the perception of effort with category ratio scales as we did in this study could be a complementary approach for researchers in human factors interested in capturing fine changes in perceived workload when task difficulty is manipulated.

4.3. Integration with the neurophysiology of perception of effort

While our experiment did not aim to investigate the neurophysiology of perception of effort, the changes (or lack of changes) in the physiological variables monitored during both experiments allow us to reconcile our results with existing theories on the neurophysiology of perception of effort in the context of motor tasks (de Morree and Marcora, 2015; Pageaux, 2016). In brief, while there is an ongoing debate on the sensory signal(s) generating the perception of effort (Marcora, 2009; Amann and Light, 2014; Smirmaul, 2014; Pageaux, 2016; Broxterman et al., 2018; Steele and Fisher, 2018), accumulating evidence suggests that when effort perception is investigated as a sensation dissociated from other exercise-related sensations (e.g., pain or discomfort), perception of effort is generated by the neuronal process of the corollary discharge associated with the central motor command and not by afferent feedback from the working muscles and organs (Marcora, 2009; de Morree et al., 2012, de Morree and Marcora, 2015; Pageaux and Gaveau, 2016). Our results are consistent with this corollary discharge model of perception of effort. Indeed, muscle activation measured with EMG is traditionally used as a marker of the central motor command (Thoroughman and Shadmehr, 1999; Carrier et al., 2011; Gaveau et al., 2021; Kozlowski et al., 2021), and among the three physiological variables measured, only muscle activation was able to track the changes in perception of effort across manipulations of task difficulties and prescription of exercise *via* the intensity of this perception. Furthermore, in line with the corollary discharge model of perception of effort and the traditional use of this perception as a marker of the central motor command (McCloskey et al., 1974; Mitchell et al., 1989; Kjær et al., 1999; Seed et al., 2019; Jacquet et al., 2021; Kozlowski et al., 2021), our results should motivate the monitoring of this perception in various population with impaired motor control such as older adults (Carment et al., 2018), patients with stroke (Neva et al., 2019), patients with Parkinson's disease (Sacheli et al., 2019), or other populations with neurological disorders. Future studies should replicate our results with such populations and explore how this perception in the context of specific upper-limb motor tasks is impaired in comparison to healthy individuals. Such studies could provide interesting insights into this perception by further validating its use as a marker of the central motor command in various populations, and potentially open new possibilities in the rehabilitation and testing of capacities.

4.4. Limits, perspectives, and conclusion

While our results provide strong support in favor of the use of the perception of effort to prescribe and monitor exercise in the context of upper-limb motor tasks, we have to acknowledge some limitations to be considered for future studies. While our sample size is appropriate for detecting changes associated with moderate to large effect sizes, future studies should increase the sample size and test finer manipulations of the physical demand. Such an increase in sample size and additional manipulations of the physical demand are important next steps to identify the responsiveness of the CR100 scale to measure the perception of effort in the context of upper-limb motor tasks. However, it is important to note that from a clinical perspective, our results replicating moderate to large effects across different experiments are of great importance and should not be neglected. Increasing the sample size could also provide perspectives for investigating sex, gender, and ethnicity differences in the use of the perception of effort to monitor and prescribe exercise. Despite our attempt to control for the induction of fatigue, subjective feelings of fatigue slightly increased in the weight session of experiment 1 ($+ 0.9 \pm 1.5$ on a visual analog scale). However, as the completion of the box and block test and the pointing task, as well as the difficulties, were randomized, we are confident that this slight increase in fatigue did not impact the validity of our results. Nonetheless, future studies using physical demand manipulations and controlling for the presence of fatigue should consider increasing the recovery period between each task completion. In this study, we focused on the box and block test as well as a pointing task, and our results should be extended to other upper-limb tasks routinely used in research and clinical settings with a stronger focus on manual dexterity such as the Purdue pegboard test (Backman et al., 1992; Shahar et al., 1998) or the Minnesota manual dexterity test (Lourenção et al., 2005; Cederlund, 2009). To conclude, this study provides strong evidence in favor of the use of the perception of effort to prescribe and monitor the exercise in the context of upper-limb motor tasks. By integrating the results of the two experiments, measurement of muscle activation, and especially muscle activation of the biceps brachial, seems to be the best physiological correlate of perception of effort during upper-limb motor tasks when the physical demand of the task is manipulated. However, as the muscles that best quantify effort and correlate with its perception will likely change with the investigated tasks, and physiological responses other than muscle activation are likely task-specific, future studies should further explore the identification of psychophysiological correlates of perception of effort in different upper-limb motor tasks. Additionally, the results demonstrating an increased mental demand when physical demand was manipulated with the tempo and weight add to the literature proposing shared mechanisms between physical and mental effort (e.g., Preston and Wegner, 2009). These results reinforce the need for future research challenging the idea that effort perception may encompass both physical and mental aspects of engagement in a task. As effort is perceived not only in the physical domain but also in the mental domain (Preston and Wegner, 2009; Pageaux, 2016; Inzlicht et al., 2018), future studies should test the possibility to extend our results in the context of the manipulation of the mental demand.

Data availability statement

The original contributions presented in this study are included in the article/**Supplementary material**, further inquiries can be directed to the corresponding author.

Ethics statement

The studies involving human participants were reviewed and approved by Local Ethics Committee. The patients/participants provided their written informed consent to participate in this study.

Author contributions

AC, BP, JG, MG, and PR designed the study. AC, CF-B, and MG conducted the experiments. AC, BP, JG, and MG contributed to the data analysis. MG and BP created the figures. MG created the first draft of the manuscript. All authors edited and/or approved the final version.

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Conflict of interest

The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

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Supplementary material

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fpsyg.2022.974172/full#supplementary-material

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