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Testing anxiety's effect on movement planning and correction: Online upper-limb corrections are not completely automatic

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ABSTRACT

Via three experiments, we investigated heightened anxiety's effect on the offline planning and online correction of upper-limb target-directed aiming movements. In Experiment 1, the majority of task trials allowed for the voluntary distribution of offline planning and online correction to achieve task success, while a subset of cursor jump trials necessitated the use of online correction to achieve task success. Experiments 2 and 3 replicated and elaborated Experiment 1 by assessing movement-specific reinvestment propensity and manipulating the self-control resources of participants. This allowed more detailed inference of cognitive resource utilisation to tease apart the effects of conscious processing and distraction-based anxiety mechanisms. For the first time, we demonstrate that: anxiety-induced online-to-offline motor control shifts can be overridden when the need for online correction is necessitated (i.e., in jump trials); anxiety-induced online-to-offline shifts seem to be positively predicted by conscious processing propensity; and optimal spatial efficacy of limb information-based online correction seems to require cognitive resources. We conclude that long-standing definitions of limb information-based online correction require revision, and that both conscious processing and distraction theories appear to play a role in determining the control strategies of anxiety induced upper limb target directed aiming movements.

A myriad of studies show that heightened state anxiety can have a negative effect on performance via conscious processing or distraction mechanisms (Beilock & Carr, 2001; Gucciardi & Dimmock, 2008; Hardy, Mullen, & Jones, 1996; Murray & Janelle, 2003; Nieuwenhuys, Pijpers, Oudejans, & Bakker, 2008). The distraction-based attentional control theory (Eysenck, Derakshan, Santos, & Calvo, 2007) and conscious processing-based reinvestment theory (Masters, 1992) are now well-established in the literature as two of the most prominent mechanistic explanations for the anxiety-performance relationship (Payne, Wilson, & Vine, 2019; Zhang, Woodman, & Roberts, 2018). Attentional control theory proposes that heightened state anxiety impairs the central executive's attentional shifting (i.e., changing attention allocation between task-relevant stimuli) and inhibition (i.e., inhibiting attention allocation to task-irrelevant stimuli) (Eysenck et al., 2007). In turn, worrisome thoughts have greater leeway to *distract* and occupy attentional capacity and impair performance once attentional capacity is exceeded. The consequent manifestation of distraction's detrimental effect on outcome motor performance is proposedly sporadic and most consistently evident when viewed/averaged across multiple attempts (Eysenck & Wilson, 2016). However, these detrimental effects can be overcome provided sufficient cognitive

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resources are available and mobilised (e.g., mental effort). It is suggested that this act of mobilising cognitive resources to overcome the deleterious effects of heightened anxiety requires self-control (Englert & Bertrams, 2015a). Reinvestment theory suggests that the automatic motor performance of skilled performers can be disrupted if attempts are made to *consciously process* movements using declarative memory (Masters & Maxwell, 2008). Although this may be a well-intended trait coping strategy (i.e., trait conscious processing propensity), the net result is often impaired performance. Both distraction and conscious processing approaches have garnered sizable empirical evidence in relation to outcome performance (e.g., goals scored and distance from target) as the primary dependent variable (Beilock & Carr, 2001; Gucciardi & Dimmock, 2008; Hardy et al., 1996; Murray & Janelle, 2003; Nieuwenhuys et al., 2008; Wilson, Wood, & Vine, 2009). However, much less is known about the effect of these processes on the offline planning and online correction of movements.

In an attempt to address this research lacuna, recent research has investigated the variability profiles of upper limb trajectories during target-directed aiming tasks (Allsop, Lawrence, Gray, & Khan, 2017; Cassell, Beattie, & Lawrence, 2018; Lawrence, Khan, & Hardy, 2013; Roberts, Wilson, Skultety, & Lyons, 2018). The rationale for such aiming movements stems from Woodworth (1899) two-component model. Here, the first component (an offline planned initial impulse) is assumed to be a centrally programmed 'ballistic' movement intended to end at the location of the target. It is characterised by a fairly rapid, continuous change in the position of the limb. If the programming of the initial impulse is such that a discrepancy between its endpoint and the target location occurs, the limb movement enters a homing in (online current control) phase. In this second phase, adjustments are made to the movement trajectory, based on visual or proprioceptive information about limb position relative to the target, to reduce this discrepancy so that the original goal of bringing the limb to rest on the target can be achieved (for a review see Elliott, Helsen, & Chua, 2001). More recently, Elliott and colleagues (Elliott et al., 2010, 2017) build on the variants proposed by Woodworth (1899) and present the multiple-processes model of limb control. Similarly to Woodworth (1899), the model presents a description of limb control that considers the time and accuracy constraints associated with the particular motor task but also includes the strategic approach of the performer within the model. This includes prior knowledge about the availability of sensory information, one's experience in the task, and the 'costs' associated with engaging in concurrent control (see Bennett, Elliott, & Rodacki, 2012; Elliott et al., 2014; Khan & Franks, 2003). Specifically, whilst the planning and control of target directed upper limb movements are proposed to contain offline planning and online correction processes, the evidence indicates that there are three phases of online correction. The processes within the first two phases are termed impulse control and those in the third limb-target control. In the first phase, the previously proposed ballistic initial impulse may be corrected online if there is a mismatch between the efference copy and the efferent outflow (e.g., because of noise in the motor system). In the second phase, the initial impulse may be controlled further if there is a mismatch between the expected sensory consequences and the early dynamic properties of the limb (inferred via peripheral vision and proprioception). The third phase is in line with the concurrent control phase of Woodworth's model, proposing that as the limb nears the target and central vision, the individual can initiate limb-target online control if feedforward models suggest the current dynamic properties of the limb are unlikely to hit the target.

Given the proposals of the multiple-process model of limb control (Elliott et al., 2010, 2017), upper limb aiming movements are reported to consist of both effortful pre-planned offline components (feedforward) and automatic online components (feedback). Offline planning is the effortful (i.e., attention demanding) use of information from prior movements to plan upcoming movements via feedforward mechanisms (Glover & Dixon, 2002; Glover, Rosenbaum, Graham, & Dixon, 2004; Schmidt, Zelaznik, Hawkins, Frank, & Quinn Jr, 1979; Wolpert & Kawato, 1998). Online movement correction is the automatic (i.e., attention-free) use of impulse and limb target control mechanisms to detect and correct movement errors (Elliott et al., 2010, 2017; Reichenbach, Franklin, Zatka-Haas, & Diedrichsen, 2014; Reichenbach, Thielscher, Peer, Bülthoff, & Bresciani, 2009; Veyrat-Masson, Brière, & Proteau, 2010; Wolpert & Kawato, 1998).[1] Offline planning and online correction components of upper limb target directed aiming can be investigated by measuring movement variability (i.e., the within subject SD at different landmarks of the unfolding limb trajectory) (for a review see Khan et al., 2006). Research has revealed that offline planning processes are represented in changes to the early portions of trajectory variability profiles (Lawrence, Khan, Buckolz, & Oldham, 2006; Lawrence, Khan, Mourton, & Bernier, 2011) and online correction processes are represented in changes to the latter aspects of these variability profiles (Khan, Lawrence, Franks, & Buckolz, 2004; Khan, Lawrence, Franks, & Elliott, 2003).

By employing the variability method, Lawrence et al. (2013) were the first to propose two contrasting variability-based outcome hypotheses to quantify anxiety-induced distraction and conscious processing's potentially differential effects on offline and online motor control. Firstly, the resource-diminishing mechanisms of distraction were hypothesized to have a detrimental effect on relatively resource-intensive offline planning (i.e., via overloaded cognitive resource demands). Conversely, the automaticity-diminishing mechanisms of conscious processing were hypothesized to have a negative effect on automatic online correction of cursor trajectories (i.e., via anxiety-compensatory attempts to consciously control automatic process, ultimately resulting in detrimental interference). In line with this rationale, Lawrence et al. (2013) observed increased offline planning and decreased online corrections under heightened state anxiety, attributed to automaticity-impairing conscious processing mechanisms. This, along with concurrent increases in self-reported mental effort under conditions of heightened state anxiety, led Allsop et al. (2017) to suggest that participants may strategically dedicate increased cognitive resources to improve offline planning and compensate for conscious processing-impaired online correction (also see Cassell et al., 2018).

Roberts et al. (2018) tested this notion by assessing spatial variability alongside measures of how frequently participants attempted

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[1] Online corrections based on reafferent information of the self, such as vision of one's hand, is proposed to be automatic in rapidly executed tasks or at least initially automatic in slowly executed tasks (Proteau, Roujoula, & Messier 2009; Veyrat-Masson et al., 2010; Cressman et al., 2010).

to engage in planning and correction processes (i.e., time to peak velocity and the number of trials with two-component sub-movements). Variability results again suggested increased offline planning and reduced online correction under heightened anxiety, but measures of online correction prominence remained unchanged. This led Roberts and colleagues to suggest that whilst there may be a resource shift between planning and online control in the presence of anxiety, it was unlikely to be 'strategic' as suggested by Allsop et al. (2017) since participants seemingly still attempted to undertake a similar degree of reliance on online control.

It is important to note that the studies of Lawrence et al. (2013), Allsop et al. (2017), and Roberts et al. (2018) do not necessitate online corrections to maintain outcome performance. Therefore, observations of reduced online corrections under heightened anxiety may have been because increased offline planning simply lessened the perceived need for corrections. In essence, the conflicting hypotheses of the attentional control theory and reinvestment theory could not be robustly tested because of a directionality issue; it is not clear whether planning was improved when anxious to compensate for corrections impaired by anxiety, or whether corrections were reduced because greater effort when anxious improved planning and reduced the 'need' for online correction. One way to test participants' ability to undertake online corrections independently of offline planning is via early-movement cursor jumps. When these cursor jumps occur randomly and infrequently in peripheral vision, they are unexpected and force participants to utilise online processes to correct their movement during execution to hit their target. These jump corrections have been suggested to be a valid test of "automatic" and "attention free" online motor control of cursor trajectories, since they occur without the knowledge of participants, uninfluenced by repeat occurrences, independent of cued attention, and cannot be inhibited (Brière & Proteau, 2011; Proteau, Roujoula, & Messier, 2009; Reichenbach et al., 2014; Veyrat-Masson et al., 2010). Hence, while both jump and no-jump trials should *allow* for continuous and automatic utilisation of reafferent visual feedback for corrections, jump trials should externally induce or *necessitate* this to maintain outcome performance. In this regard, cursor jumps provide a more robust and direct test of whether online correction processes are impaired by anxiety; in previous studies, online correction may have been reduced because the need was less, as part of limb control strategies (see Allsop et al., 2017; also see Elliott et al., 2010), rather than being impaired by anxiety.

In the present study, three experiments systematically investigated heightened anxiety's effect on the offline planning and online correction. Experiment 1 featured a subset of cursor jump trials which 'necessitated' the use of online correction to achieve task success. Experiments 2 and 3 replicated and elaborated Experiment 1 by examining the influence of movement-specific reinvestment propensity and manipulating the self-control resources of participants.

1. Experiment 1

The primary purpose of Experiment 1 was to directly manipulate the need for online corrections to maintain outcome performance in 20% of all trials, using cursor jumps. This aimed to test whether the increased prominence of offline planning under heightened anxiety observed by previous literature (e.g., Allsop et al., 2017; Lawrence et al., 2013; Roberts et al., 2018), simply *reduces* the need for automatic online correction, or is a strategic mechanism employed to compensate for anxiety *directly impairing* online correction processes. Thus, for no-jump trials where online correction is not externally induced, we hypothesized that heightened compared to low state anxiety conditions would exhibit similar or reduced variability early in movement (i.e., increased offline planning) and a reduction in the levelling off of variability profiles typically seen when vision is used online in directional aiming tasks (see Khan et al., 2004) (i.e., reduced online correction), overall resulting in unchanged outcome performance. For jump trials where online corrections are externally induced and necessitated to maintain outcome performance, we hypothesized heightened anxiety would produce impaired correction magnitude and variability. Overall, this would support the proposals within the conscious processing theory that

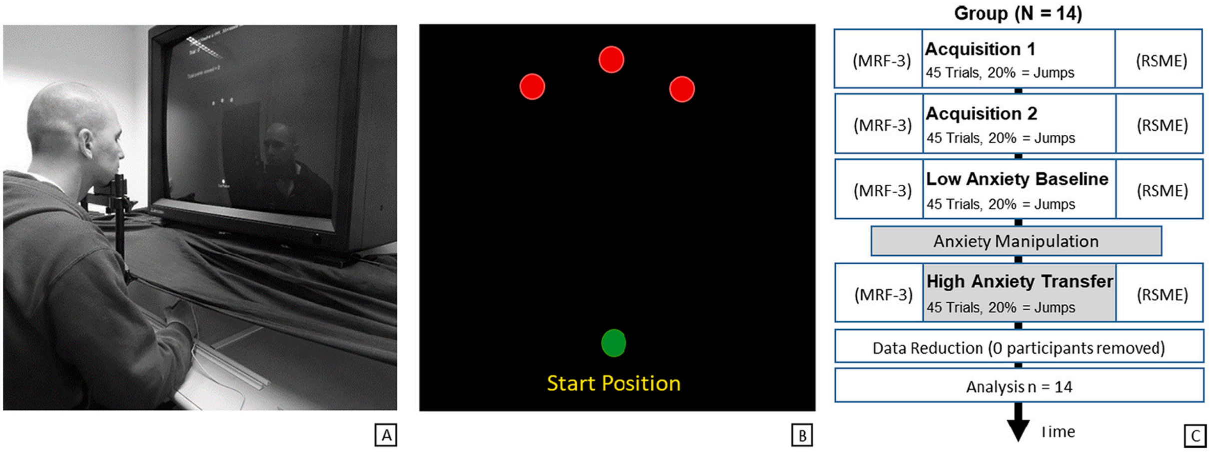

Fig. 1. Experiment 1 Setup and schematic.
Note. Panel A displays participant, apparatus, and on-screen setup. Panel B displays a not-to-scale representation of the targets and start position participants saw on the screen during trial blocks. Panel C displays Experiment 1's schematic. The mental readiness form-3 (MRF-3) was completed by participants before every block of trials and the rating scale of mental effort (RSME) was completed after every block of trials. In the final block of trials comprised a high anxiety transfer.

heightened state anxiety directly *impairs* online correction processes.

1.1. Methods

Participants. The experiment sample comprised of 14 university students (all male, $\text{mean}_{\text{age}} = 19.46$, $SD_{\text{age}} = 1.20$). G*Power (G*Power 3.1.9.3; Faul, Erdfelder, Lang, & Buchner, 2007) sample size estimation for Experiment 1 confirmed 14 participants necessary to provide power = 0.8 for the interaction between trial block (baseline and transfer) and distance (at 25, 50, 75, and 100%) of the movement trajectory when alpha = 0.05, correlation among repeated measures = 0.5, $\varepsilon = 1$, and $\eta_p^2 = 0.10$; a conservative effect size estimation based on Lawrence et al. (2013) and Allsop et al. (2017). All participants were: naïve to the hypotheses being tested; self-reported right hand dominant; with normal or corrected to normal vision; offered course credit together with the chance to win £100 in exchange for participating; and gave informed consent prior to participating. The experiment was reviewed by, granted ethical approval from, and conducted in accordance with the ethical guidelines laid out by the institutional ethics committee in accordance with the ethical standards laid down in the 1964 Declaration of Helsinki and its later amendments.

Anxiety and effort scales. Cognitive state anxiety was assessed via the Mental Readiness Form-3 (MRF-3) (Krane, 1994) which comprises three polar-opposite Likert scales: 1 (not worried) to 11 (worried), 1 (confident) to 11 (not confident), and 1 (not tense) to 11 (tense). For the purpose of present studies, only the cognitive anxiety (worry) measure was used during analysis. Task mental effort was assessed via the Rating Scale of Mental Effort (RSME) (Zijlstra, 1993) which comprises a 1–150 vertical scale with 9 anchors starting from 3 (no mental effort at all) to 114 (extreme mental effort).

Apparatus. A Calcomp 3 digitizing tablet was placed horizontally in front of participants (size = 122 × 91.5 cm, sample rate = 200 hz). Aiming movements were performed on this tablet using a stylus. The X/Y position of the stylus on the tablet corresponded, with one-to-one mapping, to the X/Y position of a round cursor (white, 1 cm diameter) on a black background displayed using a 37 in. Mitsubishi Diamond Pro monitor (refresh rate = 85 hz) located 33 cm in front the participants' eyes and 20 cm above the tablet (see Fig. 1). An opaque shield obscured participants' vision of their hand. The tablet was interfaced with a PC via a serial link and all visual stimuli on the monitor were produced using Visual Basic and Direct X Software. A circular 'start position' (green, 1 cm diameter) was displayed at the bottom centre of the monitor. Three circular 'targets' (red, 1 cm diameter) were each displayed 24 cm centre-to-centre from the start position; one target was directly above the start position and the other two targets were located 10° either side (angle subtended from the home position). The distance between the start position and targets formed a visual angle of 40°. Participants' chair and chinrest were adjustable to ensure their eyes were midway between the start and targets. Cursor, targets, and the start position remained visible throughout testing.

Task and procedure. Participants began each trial by centring their cursor over the start position. Once the cursor was steadily aligned, the experimenter initiated the trial, and the target participants had to aim for turned green. Following a random 500–1500 ms delay, a go-tone signaled participants to initiate their movement through, and past, the target centre. Target order was randomized within each block of trials and between participants; with the condition that all targets (left, middle, or right) occurred before any given target was repeated. At the beginning of testing, the experimenter demonstrated four correct trials. It was emphasised to participants that: (1) they had to keep their eyes on the target throughout movement execution; (2) reaction time to the go-tone was not important; (3) they had to make one smooth and straight movement as closely as possible through and past the target centre; and (4) they did not have to stop on the target (thus giving the task a directional but not amplitude requirement). Once participants had gone through and past the target arc, they had to come to a complete stop. The target would become red again and participants had to return to the start position ready for the next trial. At this point knowledge of results in the form of movement time (MT) (ms) and a point score (see Allsop et al., 2017) was presented on the monitor. MT had to be 450 ms (±10%) and movements outside this criterion were immediately repeated until correct.[2]

All participants had to complete three blocks of 45 trials under low state anxiety for acquisition followed by one transfer block of 45 trials under heightened state anxiety. Therefore, Experiment 1 adopted a within-participant pre-post-test design wherein the final acquisition block acted as a low anxiety baseline. A combination of outcome and monitoring pressure was utilised to heighten cognitive state anxiety; this method has repeatedly been shown successful in laboratory settings (Allsop et al., 2017; DeCaro, Thomas, Albert, & Beilock, 2011; Lawrence et al., 2013; Wilson, Smith, & Holmes, 2007). Participants were informed that: (1) their cursor movements on the monitor were being filmed for future analysis; (2) they were being paired with another participant who had improved their performance by 20%, and that they too needed to improve by 20% for either their partner or themselves to be entered into a £100 prize draw; and (3) their partners would be emailed who they were paired with and whether they had achieved the 20% performance increase. Following testing, participants were debriefed and informed that everyone would be entered into the prize-draw regardless of performance and that they were not filmed, nor paired with a partner. Cognitive state anxiety was assessed via the MRF-3 immediately before each block. Mental effort was assessed via the RSME immediately after each block of trials (see Fig. 1 panel B).

In each block of trials, a randomized 20% of trials featured a cursor perturbation. In these 'jump trials', perturbations occurred 100 ms after movement initiation shifted the on-screen cursor 15 mm to the right of its current position, perpendicularly to the straight line connecting the start position and trial target (Veyrat-Masson et al., 2010). The rationale for randomly distributed and infrequent cursor jumps, that occurred outside of central vision (> 10° visual angle), was to make offline planning irrelevant and force participants to initiate a sizable online correction (Proteau et al., 2009; Veyrat-Masson et al., 2010).

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[2] Although trials were immediately repeated in Experiment 1 if MT was outside 450 ms ±10%, jump trial error was calculated using the first jump trial, not a repeated trial. Therefore, error calculation was in line with Experiment 2 and 3 where jump trials were not repeated based on MT.

1.1.1. Data reduction and dependent measures

Stylus displacement data were filtered using a second-order dual-pass Butterworth filter with a 10 Hz low-pass cut-off frequency. The initiation of movement was defined as the point in time that the cursor moved 1 mm from the home position. The movement endpoint was the point at which the trajectory crossed the arc subtended by the three targets (see Khan et al., 2003). Trials were immediately repeated if participants' movement trajectories did not reach this point. The above calculations were performed on raw data from the task software (Visual Basic) in real time via a custom LabView analysis programme. MT and points scored for trial knowledge of results, were transferred back from LabView to Visual basic within 400 ms of data collection cessation.

Dependent variables consisted of: MRF-3 cognitive state anxiety, RSME mental effort, MT, constant error, and variability. MT was defined as the interval in time (ms) between movement initiation and movement endpoint. Directional error (mm) was calculated at 25, 50, 75, and 100% of the start-to-target distance as the stylus' orthogonal deviation from the longitudinal axis connecting the start position and target on each trial. Directional error to the right of this line was numerically positive whereas directional error to the left of the line was numerically negative. Trials where ≥25% of directional error across the movement trajectory deviated by ≥2 standard deviations from the respective within-participant block, target and jump/no-jump mean, were eliminated prior to analysis.[3] Constant error was calculated as the within-participant mean directional error. Variability was calculated as the within-participant standard deviation of directional error.

In no-jump trials, endpoint (100% start-to-target distance), constant error and variability were used to evaluate outcome performance. Directional variability at 25, 50, 75, and 100% start-to-target distance was used to evaluate offline planning and non-necessitated online corrections. Reduced outcome performance would reflect increases in constant error and variability at endpoint. Reduced offline planning would reflect increased variability at 25% start-to-target distance. Reduced non-necessitated online corrections would reflect increases in variability between 75 and 100% start-to-target distance (see Khan et al., 2006).

In jump trials, endpoint constant error was used to evaluate correction magnitude, and variability was used to evaluate correction consistency; together these provided an overall measure of outcome performance of necessitated online corrections. Note, in line with the work of Proteau and colleagues (Brière & Proteau, 2011; Proteau et al., 2009; Veyrat-Masson et al., 2010), constant error reflected that of the stylus, not the cursor. Therefore, constant error of −15 mm at 100% start-to-target distance would reflect perfect correction magnitude against the +15 mm rightward jumps. Reduced necessitated online corrections would reflect less negative / more positive constant error (i.e., decreased correction magnitude) and greater positive variability (i.e., decreased correction consistency).

Analyses. For brevity and clarity, our analysis focused on the low anxiety baseline and high anxiety transfer trial blocks. Likewise, all analysis below used data collapsed across targets because no significant target (left, middle & right) x block (low anxiety baseline & high anxiety transfer) interaction was observed at endpoint. Paired samples *t*-tests compared low anxiety baseline and high anxiety transfer block MT, MRF-3 state anxiety, and RSME mental effort scores. Fully repeated measures 2 block (low anxiety baseline & high anxiety transfer) x 4 distance (25, 50, 75, & 100% of the start-to-target distance) ANOVAs separately analysed constant error and variability. Note, jump and no-jump trial data were analysed separately using the above analyses. Jump trials' endpoint constant error and variability in acquisition 1, 2, and, 3 were also compared via separate 3 (block) one way repeated measures ANOVAs to confirm correction efficacy was automatic and uninfluenced by practice. Post-hoc analysis comprised planned repeated contrasts, Bonferroni corrected ANOVAs, and two-tailed t-tests where appropriate. Alpha was set at 0.05. Data met the assumptions of normality. Greenhouse-Geisser corrected tests are reported when sphericity was violated.

Importantly, recent work which tested hierarchical statistical approaches (i.e., post-hoc breakdown exclusively following omnibus significance) demonstrated that such an approach inflates type 1 error rates and recommended post-hoc analysis be performed regardless of omnibus significance (Chen, Xu, Tu, Wang, & Niu, 2018; relatedly, see also Greenland et al., 2016). Therefore, greater importance was assigned to the post-hoc comparisons involving hypotheses, than arbitrary omnibus analyses.

1.2. Results

1.2.1. Monitored confounds

State anxiety. All participants reported increased state anxiety at high anxiety transfer ($M = 5.93$, $SD = 2.02$) compared to low anxiety baseline ($M = 2.50$, $SD = 1.29$), resulting in statistical significance ($t(13) = -7.02$, $p < .001$, 95% CI [−4.48, −2.37]).

Mental effort. Correspondingly with state anxiety, mental effort also increased significantly ($t(13) = -5.37$, $p < .001$, 95% CI [−29.35, −12.51]) from low anxiety baseline ($M = 76.79$, $SD = 22.42$) to high anxiety transfer ($M = 97.71$, $SD = 12.83$).

Movement time. For no-jump trials, no significant difference was observed ($t(13) = -0.34$, $p = .740$, 95% CI [−4.68, 3.41]) between low anxiety baseline ($M = 447.53$ ms, $SD = 5.88$) and high anxiety transfer ($M = 448.17$ ms, $SD = 6.72$). Similarly, for jump trials, no significant difference was observed ($t(13) = -0.13$, $p = .902$, 95% CI [−12.94, 11.51]) between low anxiety baseline ($M = 462.99$ ms, $SD = 15.47$) and high anxiety transfer ($M = 463.70$ ms, $SD = 14.16$). Thus, differences in error between low anxiety baseline and high anxiety transfer should not be attributable to a speed-accuracy trade-off.

Practice effect for jump trials. No participant reported explicitly noticing the cursor jumps, even when debriefed. Neither endpoint

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[3] In Experiment 1, directional error was calculated at every 25% of start-to-target distance. In Experiment 2 and 3, directional error was calculated at every 2.5%. Directional error of a given trial had to deviate >2 standard deviations at ≥25% of these trajectory measurements to be excluded (i.e., at two measurement points in experiment 1 or 10 measurement points in experiment 2). In Experiment 1, of all trials at baseline and transfer respectively, 4.17% and 3.97% were removed. In Experiment 2, of all trials at baseline and the final trial block respectively, 3.63% and 4.30% were removed. In Experiment 3, of all trials at baseline and transfer respectively, 3.47% and 3.58% were removed.

correction constant error nor endpoint variability significantly changed across acquisition trial blocks ($F(2, 26) = 1.13$, $p = .339$, $\eta_p^2 = 0.08$; $F(2, 26) = 0.94$, $p = .400$, $\eta_p^2 = 0.07$, respectively). Therefore, differences between jump trials between low anxiety baseline and high anxiety transfer should not be due to practice effects.

1.2.2. No-jump trials

No-jump constant error. The 2 block × 4 distance ANOVA revealed a significant main effect for distance ($F(1.67, 21.76) = 45.24$, $p < .001$, $\eta_p^2 = 0.78$), but no significant main effect for block ($F(1,13) = 1.33$, $p = .269$, $\eta_p^2 = 0.09$), nor block x distance interaction ($F(1.88, 24.39) = 1.36$, $p = .275$, $\eta_p^2 = 0.10$). Endpoint constant error for high anxiety transfer ($M = -0.68$ mm, $SD = 1.46$) was not significantly different to low anxiety baseline ($M = -0.42$ mm, $SD = 1.08$).

No-jump variability. Analysis revealed a significant main effect for distance ($F(1.82, 23.67) = 116.77$, $p < .001$, $\eta_p^2 = 0.90$), a non-significant main effect for block ($F(1,13) = 1.96$, $p = .185$, $\eta_p^2 = 0.13$), and a block x distance interaction ($F(1.42, 18.44) = 3.60$, $p = .061$, $\eta_p^2 = 0.22$) that approached significance.[4] As shown in Fig. 2, there was no difference at 25% of distance, but variability tended to be lower at high anxiety transfer compared to low anxiety baseline at 75% and 100% of the distance.

1.2.3. Jump trials

1.2.3.1. Jump constant error.

As shown in Fig. 3, the 2 block × 4 distance ANOVA revealed a significant main effect for distance ($F(1.77, 23.02) = 88.45$, $p < .001$, $\eta_p^2 = 0.87$), a significant main effect for block ($F(1, 13) = 6.57$, $p = .024$, $\eta_p^2 = 0.34$), and significant block x distance interaction ($F(1.44, 18.75) = 5.94$, $p = .016$, $\eta_p^2 = 0.31$). Contrasts revealed a significant difference in trial blocks between 25 and 50%, and 50% and 75% of distance, resulting in significantly greater high anxiety transfer correction magnitude come endpoint ($M = -10.99$ mm, $SD = 2.64$) compared to low anxiety baseline ($M = -9.23$ mm, $SD = 2.85$).

1.2.3.2. Jump variability.

The 2 block × 4 distance ANOVA showed a significant main effect for distance ($F(3, 39) = 113.69$, $p < .001$, $\eta_p^2 = 0.90$), but no significant effect for block ($F(1, 13) = 0.59$, $p = .455$, $\eta_p^2 = 0.04$), nor block x distance interaction ($F(1.77, 23.01) = 0.17$, $p = .817$, $\eta_p^2 = 0.01$). Correction variability at endpoint in high anxiety transfer ($M = 6.65$ mm, $SD = 1.70$) was not significantly different to low anxiety baseline ($M = 7.36$ mm, $SD = 2.51$).[5]

1.3. Discussion

Offline planning is the effortful preparation of movements before their initiation and online correction is the automatic detection and correction of errors during movement execution (Elliott et al., 2017). In no-jump trials, participants were free to use a combination of offline planning and online correction to attain their level of outcome performance. In jump trials however, online correction was necessitated to hit the target. Participants reported a significant increase in state anxiety and invested mental effort from the initial low anxiety baseline block to the subsequent high anxiety transfer block.

No-jump trials' variability at 25% movement distance did not differ between low and heightened anxiety, but changes in variability between 75% and 100% of movement distance did. Under low anxiety, there was a tendency for variability profiles to level off late in their trajectory (i.e., values were similar between 75% and 100% of movement distance), whereas in heightened anxiety transfer variability profiles had a tendency to continue to increase towards the end of movement trajectory. One might expect variability to decrease between 75% and 100% of movement distance as the limb enters central vision and comes under control process associated with the limb-control phase of online control (Elliott et al., 2017; Khan, Lawrence, Fourkas, et al., 2003). However, the current experiment adopted an aiming task consisting of directional constraints only. In such tasks, a 'levelling off' of variability profiles is indicative of online correction processes (Khan et al., 2004). Therefore, in line with past research, the pattern of the variability profiles in the no-jump trials displayed a tendency for maintained offline planning and reduced online correction under heightened anxiety (Allsop et al., 2017; Cassell et al., 2018; Lawrence et al., 2013; Roberts et al., 2018). However, these trajectory patterns were not large enough to affect outcome performance. Endpoint (i.e., 100% movement distance) variability and constant error did not significantly differ between low and heightened anxiety conditions. As in past studies and as hypothesized, this suggests participants were able to maintain outcome performance despite heightened anxiety, likely though an increase in mental effort and a shift in strategy between offline and online control processes (Allsop et al., 2017; Elliott et al., 2017).

However, jump trial results showed the converse to what was hypothesized. Endpoint correction magnitude significantly increased

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[4] Comparison between low anxiety baseline and the 2nd acquisition trial block revealed there was a significant 2 block × 4 distance interaction ($F(1.410, 18.33) = 5.41$, $p = .022$, $\eta_p^2 = 0.29$) wherein variability between acquisition 2 and low anxiety baseline remained similar up to and including 75% distance, after which low anxiety baseline featured significantly reduced variability compared to acquisition 2. Therefore, although the low anxiety baseline to high anxiety transfer interaction was non-significant, anxiety reversed the block-on-block change in online correction from better to worse.

[5] Despite the jump trials' reduced frequency (9 trials per block) and the consequently reduced data resolution, their variability was comparable to that of the normal trials (36 trials per block). For example, the mean(SD) of Experiment 1 baseline jump variability was 3.45(1.28), 5.30(1.69), 6.48 (2.04), and 7.36(2.51)mm at 25, 50, 75, and 100% of the start-to-target distance while the baseline normal trial variability was 3.01(0.66), 4.37 (1.05), 5.43(1.27), and 5.87(1.39)mm at baseline at 25, 50, 75, and 100% of the start-to-target distance. Experiment 2 and 3 jump/normal trial differences were similarly negligible.

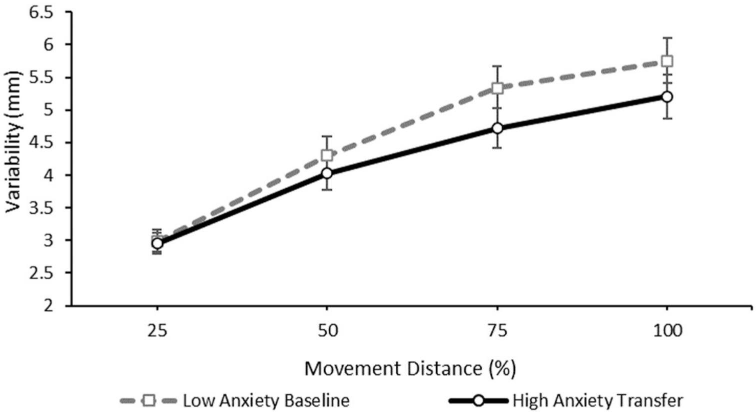

Fig. 2. Experiment 1 No-jump trial variability.
Note. Experiment 1 no-jump trial variability (mm) is displayed for low anxiety baseline and high anxiety transfer at 25, 50, 75, and 100% movement distance. Note, variability at 25% movement distance is indicative of decreased offline planning and variability between 75 and 100% movement distance is indicative of decreased movement correction. The interaction between the baseline and high anxiety transfer block variability was near-significant ($p = .061$). Error bars = ±1 standard error of the mean.

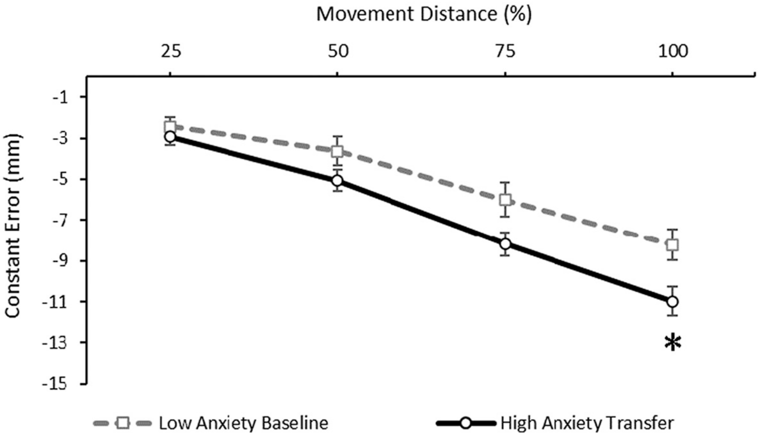

Fig. 3. Experiment 1 Jump trial constant error.
Note. Experiment 1 jump trial constant error is displayed for low anxiety baseline and high anxiety transfer trial blocks at 25, 50, 75, and 100% movement distance. Note, increased negative constant error, closer to -15 mm, indicates greater correction magnitude and therefore increased online corrections when necessitated. The asterisk represents the significantly greater high anxiety transfer endpoint correction at 100% movement distance compared to baseline. Error bars = ±1standard error of the mean.

and endpoint variability was maintained under heightened anxiety compared to low anxiety. Overall, this suggested increased online correction and improved jump-trial outcome performance. Therefore, if the need for correction is great enough (e.g., via artificial cursor jumps), participants may still be able to initiate a correction and benefit from the additional effort invested into the task. However, this notion contradicts prior findings advocating online correction's automaticity (Brière & Proteau, 2011; Franklin & Wolpert, 2008; Proteau et al., 2009; Reichenbach et al., 2009; Reichenbach et al., 2014).

2. Experiment 2

The results of Experiment 1 question whether online control of cursor trajectories is wholly automatic, and therefore also questions whether reductions in online motor control under heightened anxiety can be attributed to the hypothesized automaticity-impairing mechanisms of conscious processing (Lawrence et al., 2013). Given the unexpected jump trial findings of Experiment 1, the first aim of Experiment 2 was to replicate Experiment 1 using a larger sample size and more rigorous design (i.e., via the inclusion of a control group). The second aim of Experiment 2 was to tease apart distraction and conscious processing effects. Conscious processing propensity is suggested to be a stable personality trait that can be measured via the movement specific reinvestment scale (Masters, Eves, & Maxwell, 2005). Therefore, individuals high in conscious processing propensity should consciously process movement execution under heightened state anxiety, whereas individuals low in conscious processing propensity should be distracted. The final aim of Experiment 2 was to assess correction latency (i.e., how early a correction is initiated and potentially creates a greater, and in this study more appropriate, endpoint correction magnitude) because experiment 1 exclusively assessed correction acuity (i.e., how appropriate the endpoint spatial magnitude of the correction is for the cursor jump at present). This may have been an important omission given previous findings that anxiety affects visual search strategies via higher search rates for performance-threatening stimuli and peripherally presented targets (Murray & Janelle, 2003; Wilson et al., 2009). To accurately assess correction latency, the occurrence of cursor jumps was standardised to occur at the same point in each jump trial (i.e., 7.5% into the start-to-target distance), while measuring stylus position every 2.5% of the start-to-target distance.

Based on the results of Experiment 1, we hypothesized that the heightened anxiety group would exhibit robust offline planning but reduced online correction in no-jump trials and improved online correction in jump trials compared to the low anxiety control group. We also hypothesized that lower conscious processing propensity (and thus presumed distraction) would have a negative relationship with offline planning efficacy (assessed via variability at 25% start-to-target distance in no-jump trials), and that higher conscious processing propensity (and thus presumed conscious processing) will have a negative relationship with online correction efficacy (assessed via reductions invariability between 75 and 100% start-to-target distance in no-jump trials, and endpoint constant error closer to -15 mm in the jump trials). Lastly, we hypothesized that, heightened anxiety would have a beneficial effect on correction latency in cursor jump trials.

2.1. Methods

Participants. For Experiment 2, participation incentives and inclusion criteria were identical to Experiment 1. The experiment sample comprised of 60 university students randomized into two equally sized groups (control and experimental). Post data reduction, the control group kept its n of 30 (13 females, 17 males, $M_{\text{age}} = 19.4$, $SD_{\text{age}} = 1.499$) but the experimental group n reduced to 22 (12 females, 9 males, $M_{\text{age}} = 19.545$, $SD_{\text{age}} = 2.483$). G*Power (G*Power 3.1.9.3; Faul et al., 2007) sample size estimation deemed 14

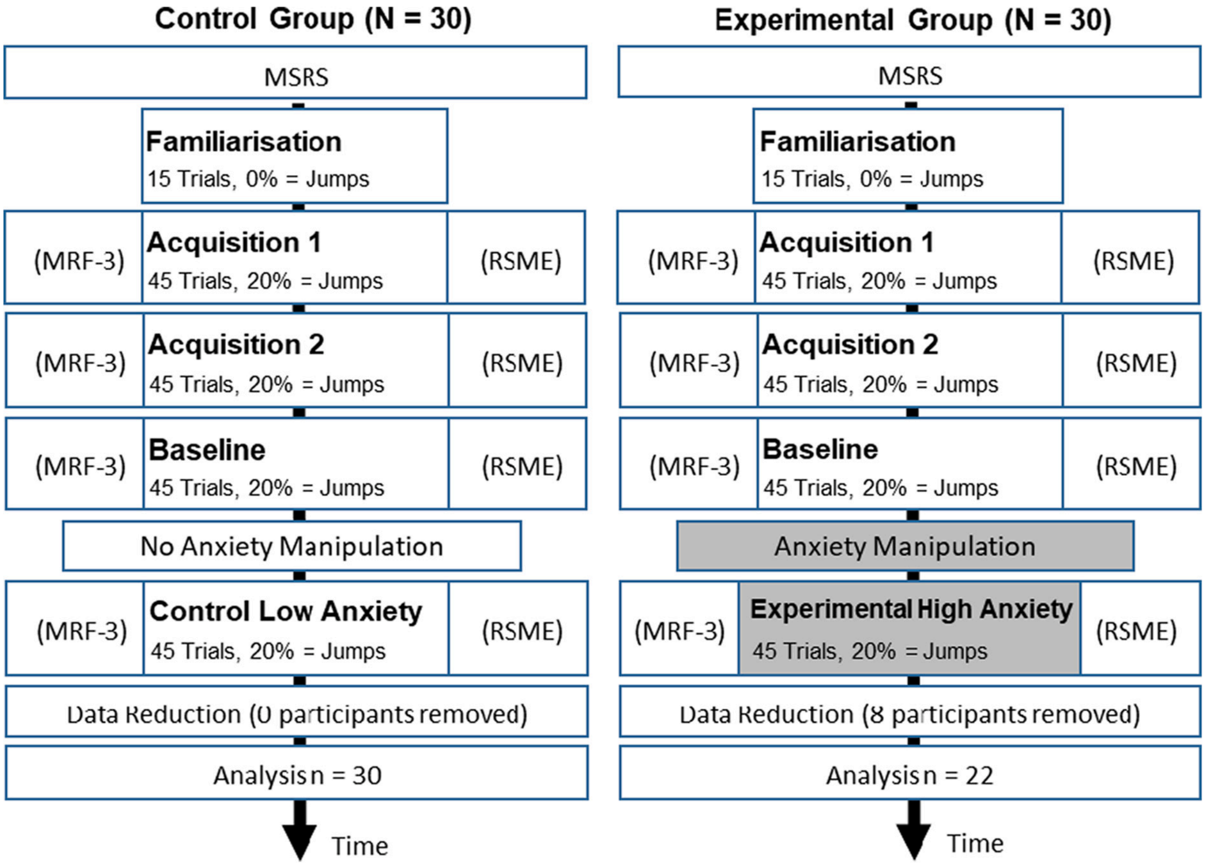

Fig. 4. Experiment 2 Schematic.
Note. The movement specific reinvestment scale (MSRS) was completed at the beginning of testing. Barring familiarisation, the mental readiness form-3 (MRF-3) was completed before every block of trials and the rating scale of mental effort (RSME) was completed after every block of trials. In the final block of trials, the control group was transferred to another block of low anxiety trials, whilst the experimental group was transferred to heightened anxiety. Experimental participants who did not increase their MRF-3 score from baseline to anxiety were excluded from further analysis during data reduction.

participants per group (control and experimental) necessary, to provide power = 0.8 for the interaction between trial block (baseline and transfer) and distance (at 25, 50, 75, and 100%) of the movement trajectory, when alpha = 0.05, correlation among repeated measures = 0.5, $\varepsilon = 1$, and $\eta_p^2 = 0.10$. Thus, final sample size exceeded minimum power requirements.

Anxiety, effort, and conscious processing scales. As in Experiment 1, cognitive state anxiety was assessed via the MRF-3 (Krane, 1994) and task mental effort was assessed via the RSME (Zijlstra, 1993). New to Experiment 2, trait conscious motor processing was assessed via the movement specific reinvestment scale (MSRS) (Masters et al., 2005). This measure comprises of 2 subscales (movement self-consciousness and conscious motor processing) comprising 5 items each. Items are answered via a 7-point Likert scale ranging from strongly disagree to strongly agree. For this experiment, only the conscious motor processing subscale was analysed since it is suggested to be more closely linked to the negative effects of anxious reinvestment (Masters et al., 2005). This subscale includes statements such as "I am always trying to think about my movements when I carry them out".

Apparatus, task, and procedure. The apparatus, task, and procedure were identical to Experiment 1, barring 5 exceptions. The first difference was that Experiment 2 contained a control group which was not transferred to heightened state anxiety in their final block of trials (see Fig. 4). Immediately before the final block of trials, participants in this group were instructed to "please continue hitting the targets as accurately as you can". Secondly, all participants completed the MSRS at the beginning of testing. Thirdly, before starting their acquisition blocks, participants completed 15 familiarisation trials which did not contain cursor jumps – to further conceal their occurrence. This was because, despite participants in Experiment 1 reportedly not noticing the cursor jumps, the experimenter observed that the endpoint error of jump trials early in testing surprised some participants. Fourthly, jump trials were never repeated, regardless of MT, to avoid implicit practice effects; at the beginning of testing, participants were told a cover story stating that a random set of trials were accepted irrespective of MT. Finally, to accurately investigate whether heightened anxiety influenced correction latency, cursor jumps occurred at 7.5% of the start-to-target distance, instead of 100 ms.[6]

Data reduction and dependent measures. Prior to data collection, it was decided that experimental group participants who did not respond to the anxiety manipulation (i.e., did not have a heightened MRF-3 worry score at high anxiety transfer compared to low anxiety baseline) would not be included in any final analysis. Therefore, 8 participants in the experimental group ($N = 30$) were excluded from all subsequent analysis, making final experimental n 22. Barring two exceptions, all subsequent data reduction and dependent measures were also identical to Experiment 1.

Firstly, participants completed the MSRS at the start of testing. The mean score on this scale formed a new dependent variable, used to ensure conscious processing propensity of groups was the same. More importantly however, MSRS, RSME, and MRF-3 scores were hierarchically linearly correlated with markers of offline planning (no-jump variability at 25% start-to-target distance), non-necessitated online correction (late no-jump variability increase: variability at 100% minus 75% start-to-target distance), and necessitated online correction efficacy (jump trial constant error and variability at endpoint). This was to explicate distraction and conscious processing mechanisms underlying heightened state anxiety performance changes. For conciseness and because the detrimental effects of conscious processing should primarily only occur when anxious (Masters & Maxwell, 2008), only the correlations of the experimental group are reported.

Secondly, to calculate correction initiation latency, in every block of trials and for every participant, the no-jump constant error was subtracted from jump constant error at every 2.5% of start-to-target distance to attain within participant jump trials' mean deviation from no-jump trials at each of these points. Based on the methods of Proteau et al. (2009), participants' correction initiation was identified when this deviation reached a more negative value than -1 mm and continually became more negative with every 2.5% of the start-to-target distance (i.e., suggesting the stylus progressed in the opposite direction to that of the cursor jump for the remainder of the movement). The time interval (ms) between the cursor jump and correction initiation was used to denote correction latency. From within-participant correction latency, mean group correction latency was calculated for statistical analysis. A positive effect of anxiety on correction latency would reflect corrections being initiated, overall, at a reduced latency.

Analyses. As in Experiment 1, analysis focused on the final two trial blocks; namely, the control and experimental group's baseline, and their respective low anxiety control and high anxiety experimental conditions (see Fig. 4). Data was collapsed across targets because no significant 3 target (left, middle, & right) x 2 block (baseline & control/high-anxiety) interactions were observed at endpoint. Separate 2 group (experimental & control) x 4 distance (25, 50, 75, & 100% of the start-to-target distance) ANOVAs with repeated measures on distance analysed MT, MRF-3 state anxiety, and RSME mental effort scores respectively. Separate 2 group (control & experimental) x 2 block (baseline & control/high-anxiety) x 4 distance (25, 50, 75, & 100%) ANOVAs with repeated measures on block and distance analysed constant error and variability. Separate 2 group × 2 block ANOVAs with repeated measures on block were also performed on constant error and variability at 100% distance. Note, jump and no-jump trial data was analysed identically but separately using the above analyses, except that jump trials' endpoint constant error and variability in acquisition 1, 2, and 3 were submitted to a repeated measures ANOVA to confirm correction efficacy was automatic and uninfluenced by practice. Data met the assumptions of normality. Greenhouse-Geisser corrected tests are reported when sphericity was violated. Omnibus interactions and main effects were broken down using planned repeated contrasts, Bonferroni corrected ANOVAs, and two-tailed *t*-tests. Alpha was set at 0.05. As with Experiment 1, greater importance was again assigned to the post-hoc comparisons involving hypothesized effects, than omnibus analyses.

Independent-samples t-tests compared control and experimental groups' MSRS trait conscious processing. Separate hierarchical linear regressions containing MSRS trait conscious processing propensity in step 1, RSME mental effort in step 2, and MRF-3 state

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[6] In experiment 1, cursor jumps occurring 100 ms after movement initiation, resulted in cursor jumps occurring at 6.72% into the start-target distance on average across all trial blocks.

anxiety in step 3 were performed with the following dependent variables: no-jump variability at 25% distance (indicator of offline planning efficacy); no-jump variability increase between 75% and 100% of distance (indicator of non-necessitated online correction efficacy); and jump constant error and variability, respectively, at 100% distance (together, key indicators of necessitated online correction efficacy). All variables were standardised into z-scores prior to regression analyses and assumptions of homoscedasticity and multicollinearity were met. For conciseness, regressions of only the experimental group's high anxiety transfer data are reported. Alpha was again set at 0.05.

2.2. Results

2.2.1. Monitored confounds

Conscious processing. Trait conscious motor processing of the control group ($M = 4.54$, $SD = 0.52$) was not significantly different ($t(50) = 1.51$, $p = .138$, 95% CI [−0.10, 0.73]) to that of the experimental group ($M = 4.23$, $SD = 0.97$).

State anxiety. Analysis revealed a significant main effect for block ($F(1, 50) = 51.71$, $p < .001$, $\eta_p^2 = 0.51$), group ($F(1, 50) = 15.74$, $p < .001$, $\eta_p^2 = 0.24$), and group x block interaction ($F(1, 50) = 86.21$, $p < .001$, $\eta_p^2 = 0.63$). Interaction breakdown revealed control and experimental groups' anxiety was not significantly different at baseline ($M = 3.37$, $SD = 1.63$; $M = 3.14$, $SD = 1.75$, respectively); in the subsequent trial block, the experimental group was transferred to high anxiety ($M = 6.55$, $SD = 1.87$) and reported significantly greater state anxiety than the low anxiety control group ($M = 2.93$, $SD = 1.55$). Within-group changes in state anxiety between blocks were significant for both control and experimental groups (i.e., state anxiety of the experimental group increased whilst the control group's decreased).

Mental effort. Analysis revealed a significant main effect for block ($F(1, 50) = 18.10$, $p < .001$, $\eta_p^2 = 0.27$), and a block x group interaction ($F(1, 50) = 21.99$, $p < .001$, $\eta_p^2 = 0.31$), but no main effect for group ($F(1, 50) = 1.75$, $p = .193$, $\eta_p^2 = 0.03$). Interaction breakdown revealed control and experimental groups' mental effort was not significantly different at low anxiety baseline ($M = 84.33$, $SD = 15.63$; $M = 79.91$, $SD = 18.28$, respectively), but significantly different in the subsequent block where the experimental group was transferred to high anxiety and the control group was not ($M = 83.43$, $SD = 15.96$; $M = 98.46$, $SD = 14.45$, respectively). Within-group mental effort changes were only significant for the experimental group (i.e., mental effort of the experimental group increased whilst mental effort of the control group did not).

Movement time. For no-jump trials, no significant main effect for block ($F(1, 50) = 0.21$, $p = .649$, $\eta_p^2 < 0.01$), main effect for group ($F(1, 50) = 2.55$, $p = .117$, $\eta_p^2 = 0.05$), nor block x group interaction was observed ($F(1, 50) = 1.32$, $p = .256$, $\eta_p^2 = 0.03$). For jump trials, no significant main effect for block ($F(1, 50) = 0.3.39$, $p = .075$, $\eta_p^2 = 0.06$), main effect for group ($F(1, 50) = 1.00$, $p = .480$, $\eta_p^2 = 0.01$), nor block x group interaction was observed ($F(1, 50) = 0.51$, $p = .480$, $\eta_p^2 = 0.01$). Therefore, differences between trial blocks and groups should not be a product of speed-accuracy trade-offs.

Practice effect for jump trials. As in Experiment 1, no participant reported explicitly noticing the cursor jumps, even when debriefed. Neither the control nor experimental group changed their endpoint constant error across acquisition 1, 2, and 3 ($F(2, 58) = 1.39$, $p = .257$, $\eta_p^2 = 0.05$; $F(2, 42) = 0.22$, $p = .805$, $\eta_p^2 = 0.01$, respectively for groups). Similarly, neither the control nor experimental group differed in variability across acquisition 1, 2, and, 3 ($F(2, 42) = 1.45$, $p = .246$, $\eta_p^2 = 0.07$; $F(1.60, 46.31) = 0.54$, $p = .545$, $\eta_p^2 = 0.02$, respectively for groups). Therefore, jump trial differences between trial blocks and groups should not be due to practice effects.

2.2.2. No-jump trials

No-jump constant error. Analysis found no significant 2 group × 2 block × 4 distance interaction ($F(1.86, 93.12) = 2.90$, $p = .064$, $\eta_p^2 = 0.06$). Endpoint constant error analysis revealed no significant main effect for block ($F(1, 50) = 0.20$, $p = .661$, $\eta_p^2 < 0.01$), group ($F(1, 50) = 1.47$, $p = .231$, $\eta_p^2 = 0.03$), nor block x group interaction ($F(1, 50) = 0.29$, $p = .594$, $\eta_p^2 = 0.01$).

No-jump variability. Results showed a significant omnibus 2 group × 2 block × 4 distance interaction ($F(2.32, 115.95) = 3.25$, $p = .035$, $\eta_p^2 = 0.06$) (see Fig. 5). Breakdown comparing control and experimental groups at low anxiety baseline revealed a significant distance main effect ($F(1.95, 97.59) = 212.06$, $p < .001$, $\eta_p^2 = 0.81$), and no group main effect ($F(1, 50) = 0.54$, $p = .468$, $\eta_p^2 = 0.01$), nor group x distance interaction ($F(1.95, 97.59) = 0.48$, $p = .616$, $\eta_p^2 = 0.01$). Breakdown comparing the low anxiety control and high anxiety experimental group in the final trial block revealed a significant main effect for distance ($F(2.02, 101.06) = 237.54$, $p < .001$, $\eta_p^2 = 0.83$), and group x distance interaction ($F(2.02, 101.06) = 8.50$, $p < .001$, $\eta_p^2 = 0.15$), but no main effect for group ($F(1, 50) = 2.64$, $p = .110$, $\eta_p^2 = 0.05$). Breakdown of the final trial block revealed that variability at 25% distance was significantly lower in the high anxiety experimental group compared to the low anxiety control group; concurrently, variability between 75% and 100% of distance of the low anxiety control group did not increase significantly, whereas that of the experimental group did (see Fig. 5). Analysis comparing groups' endpoint variability between baseline and the final trial block showed a significant main effect for block ($F(1, 50) = 8.34$, $p = .006$, $\eta_p^2 = 0.14$), but not group ($F(1, 50) = 0.11$, $p = .747$, $\eta_p^2 < 0.01$), nor group x block interaction ($F(1, 50) = 0.57$, $p = .455$, $\eta_p^2 = 0.01$). However, within-group breakdown showed endpoint variability for the high anxiety experimental group did not change, whereas the low anxiety control group's decreased significantly.

Linear hierarchical regression results for conscious processing propensity, mental effort, and state anxiety's respective predictive power of offline planning (variability at 25% distance) and non-necessitated online correction (variability increase between 70 and 100% distance) are reported in Table 1. Higher trait conscious processing significantly predicted reduced non-necessitated online correction. However, no other significant relationships were observed.

2.2.3. Jump trials

Jump constant error. No significant group x block x distance interaction was observed ($F(1.59, 79.73) = 1.46$, $p = .239$, $\eta_p^2 = 0.03$).

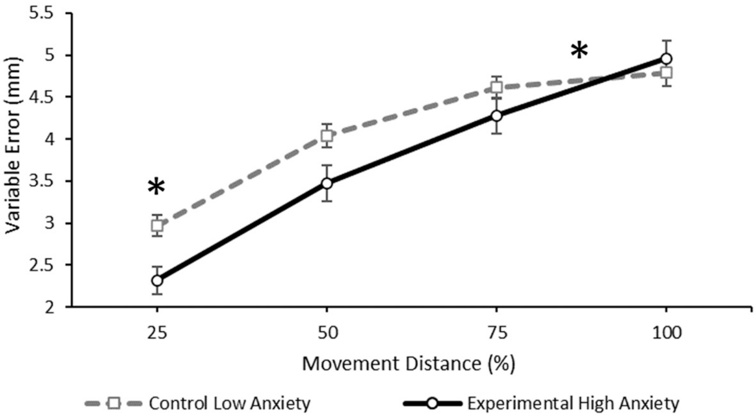

Fig. 5. Experiment 2 No-jump trial variability.
Note. Experiment 2 no-jump trial variability in the final trial block of the control (i.e., low state anxiety) and experimental (i.e., heightened state anxiety) groups at 25%, 50%, 75%, and 100% movement distance. The left-hand asterisk represents the significant difference between the low anxiety control and the experimental high anxiety groups' variability at 25% movement distance. The right-hand asterisk represents the significant interaction between the low anxiety control and the experimental high anxiety groups' variability between 75 and 100% movement distance. Error bars = ±1 standard error of the mean.

Table 1
Hierarchical linear regression predicting experimental group high anxiety transfer block error in no-jump trials.

Dependent Variables	Step (coefficient of determination) & Independent variables (95% CI)	b	SE B	p
Variability at 25% movement distance	Step 1 ($r^2 = 0.02$, $\Delta R^2 = 0.02$)			
	Conscious Processing (−0.50, 0.10)	0.15	0.15	0.50
	Step 2 ($r^2 = 0.03$, $\Delta R^2 < 0.01$)			
	Conscious Processing (−0.51, 0.09)	−0.16	0.16	0.50
	Mental Effort (−0.37, 0.33)	−0.07	0.17	0.76
	Step 3 ($r^2 = 0.08$, $\Delta R^2 = 0.06$)			
	Conscious Processing (−0.53, 0.26)	−0.10	0.18	0.69
	Mental Effort (−0.48, 0.26)	−0.17	0.20	0.50
	State Anxiety (−0.09, 0.56)	0.26	0.18	0.31
Variability increase between 75 and 100% movement distance[a]	Step 1 ($r^2 = 0.23$, $\Delta R^2 = 0.23$)			
	Conscious Processing (0.18, 0.89)	0.48	0.20	0.02
	Step 2 ($r^2 = 0.26$, $\Delta R^2 = 0.03$)			
	Conscious Processing (0.18, 0.84)	0.49	0.19	0.02
	Mental Effort (−0.31, 0.56)	−0.18	0.26	0.51
	Step 3 ($r^2 = 0.28$, $\Delta R^2 = 0.02$)			
	Conscious Processing (0.15, 0.74)	0.46	0.20	0.03
	Mental Effort (−0.43, 0.67)	−0.24	0.30	0.45
	State Anxiety (−0.69, 0.23)	0.16	0.26	0.54

Note. CI = confidence interval, conscious processing = MSRS conscious motor processing score, mental effort = RSME mental effort score, and state anxiety = MRF-3 cognitive state anxiety score. Confidence intervals and standard error based on 1000 bootstrap samples, bias corrected and accelerated.

[a] At baseline, the correlation between variability increase and CMP in step 1 was not significant ($r^2 = 0.04$, 95% CI = −0.71 to 0.26, b = −0.22, p = .26).

However, the block x group interaction was only marginally non-significant ($F(1, 50) = 3.80$, $p = .057$, $\eta_p^2 = 0.07$) (see Fig. 6) and breakdown showed the hypothesized effects. Separate 2 block × 4 distance ANOVAs for each group revealed that the control group had no main effect for block ($F(1, 29) = 0.06$, $p = .813$, $\eta_p^2 < 0.01$) but the experimental group did ($F(1,21) = 6.77$, $p = .016$, $\eta_p^2 = 0.24$), with constant error being significantly more negative, across the start-to-target distance, under heightened anxiety compared to baseline. The 2 group × 2 block ANOVA at 100% distance showed no significant block ($F(1, 50) = 3.14$, $p = .083$, $\eta_p^2 = 0.06$) nor group ($F(1, 50) = 0.05$, $p = .822$, $\eta_p^2 < 0.01$) main effect, but a significant block x group interaction ($F(1, 50) = 0.4.44$, $p = .040$, $\eta_p^2 = 0.08$). Specifically, whilst the control group did not change their outcome correction magnitude between baseline ($M = -9.49$ mm, $SD = 2.99$) and the final trial block (low anxiety, $M = -9.36$ mm, $SD = 3.43$), the experimental group significantly improved from baseline ($M = -8.46$ mm, $SD = 3.14$) to their final trial block (high anxiety, $M = -10.02$ mm, $SD = 3.04$).

Linear hierarchical regression investigating conscious processing propensity, mental effort, and state anxiety's respective predictive power of jump trials' endpoint constant error revealed no significant relationships (see Table 2).

Jump variability. No significant group x block x distance interaction ($F(1.82, 90.77) = 0.68$, $p = .496$, $\eta_p^2 = 0.01$) or block x group interaction ($F(1, 50) = 1.11$, $p = .298$, $\eta_p^2 = 0.02$) was found. Correspondingly, analysis of outcome variability found no significant

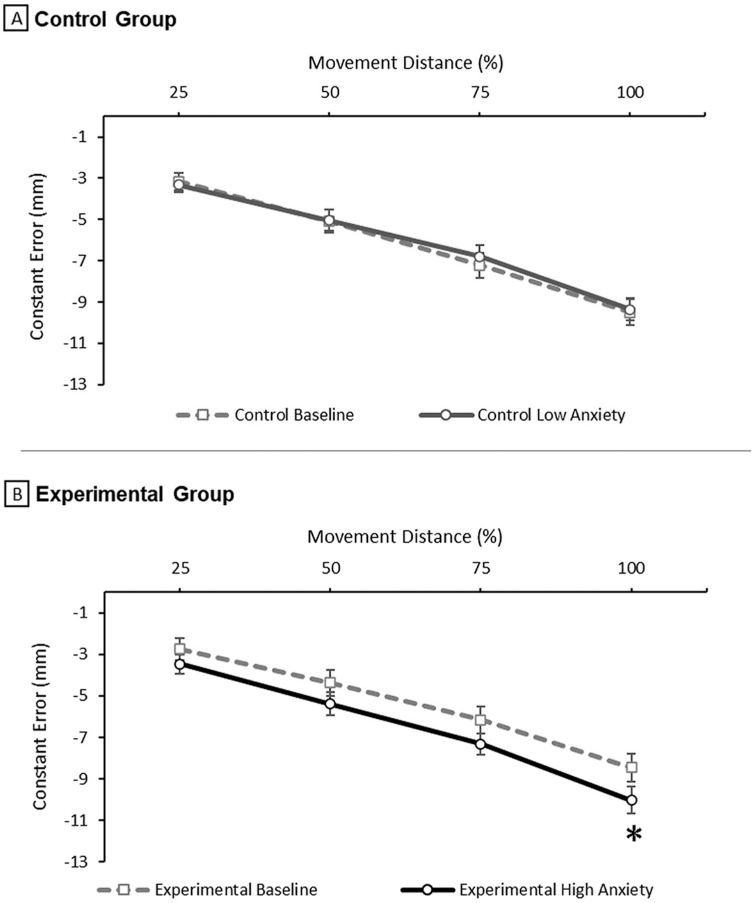

Fig. 6. Experiment 2 Jump trial constant error.

Note. Panel A (top) shows Experiment 2's control group jump trial constant error at low anxiety baseline and final trial block (again comprising of low state anxiety). Panel B (bottom) shows Experiment 2's experimental group jump trial constant error at low anxiety baseline and the final block of trials comprising a high anxiety transfer. The asterisk represents the significantly greater experimental group high anxiety endpoint correction at 100% movement distance compared to their baseline. Error bars = +/−1standard error of the mean.

main effect for block ($F(1, 50) = 1.10, p = .299, \eta_p^2 = 0.02$), group ($F(1, 50) = 0.38, p = .542, \eta_p^2 = 0.01$) nor group x block interaction ($F(1, 50) = 1.36$, $p = .249$, $\eta_p^2 = 0.03$).

Linear hierarchical regression investigating conscious processing propensity, mental effort, and state anxiety's respective predictive power of jump trials' endpoint variability revealed no significant relationships (see Table 2).

Correction latency. No significant main effect for block ($F(1, 50) = 0.90, p = .347, \eta_p^2 = 0.02$), main effect for group ($F(1, 50) = 1.53$, $p = .222$, $\eta_p^2 = 0.03$) nor block x group interaction was observed ($F(1, 50) < 0.01, p = .963, \eta_p^2 < 0.01$). The control and experimental group correction latency did not differ significantly at baseline ($M = 191.20$ ms, $SD = 39.75$; $M = 200.50$ ms, $SD = 29.72$, respectively) nor the final trial block ($M = 185.97$ ms, $SD = 34.36$; $M = 194.73$ ms, SD = 23.87, respectively). Latencies also did not differ significantly between low and high anxiety conditions.

2.3. Discussion

The purpose of Experiment 2 was threefold. Firstly, it endeavoured to replicate the findings of Experiment 1 whilst utilising a control group and a larger sample. Secondly, it aimed to investigate the effect of heightened state anxiety on the temporal latency of jump-trial corrections. Finally, it aimed to use individuals' trait conscious processing propensity to approximate distraction and conscious processing's respective effects on offline planning and online correction. A successful anxiety manipulation enabled Experiment 2 to fulfil these aims; the experimental group reported heightened state anxiety and mental effort in the transfer block, whilst the control group did not.

As hypothesized, no-jump trials under heightened anxiety (experimental group) seemed to exhibit increased offline planning and reduced online correction compared to the low anxiety (control group) trials. Jump trials on the other hand seemed to exhibit increased online correction in the experimental group between low anxiety baseline and heightened anxiety transfer, but not in the

Table 2
Hierarchical linear regression predicting experimental group high anxiety transfer block error in jump trials.

Dependent Variables	Step (coefficient of determination) & Independent variables (95% CI)	b	SE B	p
Constant error at 100% movement distance	Step 1 ($r^2 < 0.01$, $\Delta R^2 < 0.01$)			
	Conscious Processing (−0.34, 0.53)	0.02	0.19	0.88
	Step 2 ($r^2 < 0.01$, $\Delta R^2 = 0.01$)			
	Conscious Processing (−0.41, 0.58)	0.02	0.20	0.91
	Mental Effort (−0.50, 0.45)	−0.04	0.22	0.85
	Step 3 ($r^2 < 0.01$, $\Delta R^2 = 0.01$)			
	Conscious Processing (−0.46, 0.39)	0.01	0.30	0.98
	Mental Effort (−0.86, 0.77)	−0.02	0.27	0.94
	State Anxiety (−1.12, 0.58)	−0.06	0.48	0.89
Variability at 100% movement distance	Step 1 ($r^2 = 0.04$, $\Delta R^2 = 0.04$)			
	Conscious Processing (−0.80, 0.33)	−0.20	0.26	0.47
	Step 2 ($r^2 = 0.10$, $\Delta R^2 = 0.06$)			
	Conscious Processing (−0.77, 0.31)	−0.18	0.27	0.54
	Mental Effort (−0.22, 0.72)	0.25	0.19	0.19
	Step 3 ($r^2 = 0.11$, $\Delta R^2 = 0.01$)			
	Conscious Processing (−0.78, 0.27)	−0.16	0.31	0.62
	Mental Effort (−0.54, 1.05)	0.21	0.25	0.37
	State Anxiety (−0.58, 0.43)	0.11	0.27	0.61

Note. CI = confidence interval, Conscious Processing = MSRS conscious motor processing score, mental effort = RSME mental effort score, and state anxiety = MRF-3 cognitive state anxiety score. Confidence intervals and standard error based on 1000 bootstrap samples, bias corrected and accelerated.

control group. Concurrent analysis of the temporal latencies of jump trials showed no significant differences between the low anxiety control and the heightened anxiety experimental group. To explicate the distraction/conscious processing mechanisms underpinning the aforementioned findings, the experimental group's high anxiety transfer block data was used to correlate trait conscious processing propensity with indexes of no-jump offline planning, no-jump online correction, and jump online correction efficacy. Results revealed that trait conscious processing propensity positively predicted variability increases between 75 and 100% distance (i.e. reduced online correction); no other correlations were significant.

Overall, the results of Experiment 2 suggest that online correction in no-jump trials may be reduced because individuals high in conscious processing propensity, plan offline to attenuate their online correction during movement execution (perhaps in an effort to gain greater explicit control) but overturn this plan online if the need for correction is great enough (i.e., the application of an adjusted control strategy in light of the available and usable sensory feedback, also see Elliott et al., 2017). Concurrently, the additional cognitive resources mobilised to compensate for anxious distraction may benefit the spatial acuity of cursor jump corrections (i.e., when the need for a correction is great). Additionally, this change in online correction efficacy depending on anxiety, cognitive resources, and correction demands suggests that online limb corrections may not operate as automatically as previously thought (Brière & Proteau, 2011; Proteau et al., 2009; Reichenbach et al., 2014; Veyrat-Masson et al., 2010).

3. Experiment 3

Fiedler (2011) highlighted the importance of replicating findings with multiple stimuli to ensure they are a product of the phenomenon, instead of the specific set of stimuli within a single experiment. Therefore, the aim of Experiment 3 was to use self-control depletion to specifically exacerbate distraction-based anxiety mechanisms over conscious processing. The strength model of self-control suggests that initial acts of central executive-based self-control (e.g., overriding impulsive and automatic behavioural responses), consume a limited global resource (Muraven & Baumeister, 2000; Muraven, Tice, & Baumeister, 1998). When this resource is depleted, individuals are suggested to be in a state of 'ego-depletion', and thereby likely exhibit impaired performance in subsequent acts requiring self-control. Notably for the present study, the mobilisation of cognitive resources, to compensate for the deleterious effects of heightened state anxiety, is proposed to be a depletable self-control act (Englert & Bertrams, 2015a). Therefore, differences between an anxious group with self-control resources intact and anxious self-control depleted group would likely be due to distraction-based mechanisms. Concurrently, self-control depletion may prevent individuals from consciously processing movements, by reducing the resources required to direct attention to movement processes.

Experiment 3 aimed to use a self-control depleting transcription task to exacerbate distraction effects (i.e., by creating a group which should be more susceptible to distraction and less susceptible to conscious processing when self-control depleted). Transcription-based self-control tasks have repeatedly been shown to be particularly successful at inducing self-control depletion and impaired performance under anxious and distracting conditions (Englert & Bertrams, 2012; Englert & Bertrams, 2015b; Englert & Bertrams, 2016; Englert, Zwemmer, Bertrams, & Oudejans, 2015). In these tasks, participants have to omit specific letters in their transcription of a standard piece of text which should require the volitional exertion of self-control to combat usually automatic and well-rehearsed writing habits. We hypothesized that self-control depletion following a transcription task would make participants perceive the aiming task as more effortful, impair offline planning, impair non-necessitated online correction, and impair necessitated online correction; suggesting that online correction processes similarly to offline planning processes, are indeed resource-reliant, as suggested by the findings of Experiment 2.

3.1. Methods

Participants. Experiment 3 participation incentives and inclusion criteria were identical to Experiment 1 and 2. The experiment sample comprised of 62 university students randomized into two groups (self-control full and self-control depleted). Post data reduction, the self-control full group n reduced to 23 (14 females, 9 males, $M_{age} = 22.478$, $SD_{age} = 6.022$) and the self-control depleted group n reduced to 29 (16 females, 13 males, $M_{age} = 21.131$, $SD_{age} = 3.502$). The effect size of self-control depletion is a contentious topic (Carter & McCullough, 2014; Etherton et al., 2018; Vadillo, Gold, & Osman, 2016; Wolff, Baumann, & Englert, 2018); assuming a medium effect size of $\eta_p^2 = 0.06$, G*Power (G*Power 3.1.9.3; Faul et al., 2007) sample size estimation deemed 24 participants per group (self-control full and self-control depleted) necessary, to provide power = 0.8 for the interaction between trial block (baseline and transfer) and distance (at 25, 50, 75, and 100%) of the movement trajectory, when alpha = 0.05, correlation among repeated measures = 0.5, and $\varepsilon = 1$. Therefore, the final sample size of experiment 1 should be adequate overall.

Measures. A total of five new questionnaires were administered in Experiment 3 in addition to those administered in Experiment 2. These assessed: state self-efficacy (Bertrams, Englert, & Dickhäuser, 2010), trait narcissism (Ames, Rose, & Anderson, 2006), trait cognitive anxiety (Smith, Smoll, Cumming, & Grossbard, 2006), trait self-control strength (Tangney, Baumeister, & Boone, 2004), and trait self-control depletion sensitivity (Salmon, Adriaanse, De Vet, Fennis, & De Ridder, 2014). However, for conciseness and because no significant differences were observed ($p > .05$), their results are not reported.

The apparatus, task and procedure of Experiment 3 were identical to those of Experiment 2, with the following exceptions. Firstly, the order of all trait personality questionnaires at the beginning of testing was randomized across participants to reduce systematic questionnaire fatigue. Immediately following the baseline block of trials, participants' self-control was experimentally manipulated by instructing them to transcribe a piece of text from one sheet of paper onto another for six minutes (for a study schematic, see Fig. 7). The self-control depletion group was instructed to transcribe the text as quickly and accurately as possible whilst always omitting the letters 'e' and 'a' from all transcribed words. The self-control full group were instructed to transcribe the text as quickly and accurately as possible with no additional criterions given.[7] The transcribed piece of text in the present experiment was generated from dictionary example sentences for 50 random words generated via an online tool ("http://randomwordgenerator.com", n.d); no parameters other than word number was entered into the random word generator (i.e., 1). For every generated word, the first listed example sentence provided in the dictionary was used. Generated words for which there was no example sentence ($n = 6$), or in situations where two independent researchers deemed example sentences not neutral within the context of the study ($n = 7$), were not included in the final text. Male names in the text were replaced with 'he' or 'his', whilst female names were replaced with 'she' or 'her' ($n = 3$).[8] The final text contained: 37 sentences, 252 words, 1176 characters excluding spaces. The letters 'a' and 'e' were the most frequent letters in the text (87 and 155 instances, respectively) and were, therefore, the ones to be omitted in the depletion condition. Sentence order was not randomized between participants and sentences were arranged as one large paragraph in single-spaced Arial font size 12.[9] Immediately following the transcription task, all participants completed a self-efficacy check questionnaire. The self-control depletion manipulation's effectiveness in the present experiment was assessed via high anxiety transfer RSME (Zijlstra, 1993). This was because individuals with depleted self-control resources should find the task more taxing and therefore report increased mental effort required by the aiming task under heightened anxiety transfer. Using the RSME in this way should also measure self-control depletion's *direct effect on participants during the task*, rather than indirectly before it as is contemporary within self-control depletion literature (for examples see Englert & Bertrams, 2012; Englert & Bertrams, 2015b; Englert & Bertrams, 2016; Englert et al., 2015).

Data reduction and dependent measures. Again, it was decided prior to data collection that participants who do not respond to the anxiety manipulation (i.e., did not have a heightened MRF-3 score in the heightened anxiety trial block compared to baseline) are removed from subsequent analysis. Therefore, 8 participants in the self-control full group ($N = 31$), and 2 participants in the self-control depleted group (N = 31) were excluded from all subsequent analysis, leaving final group n at 23 and 29, respectively. Data reduction and dependent measures of Experiment 3 were identical to Experiment 2, barring the addition of new dependent variables including number of words transcribed, number of errors in transcription, and 'a's/'e's omitted. Errors in transcription were defined as: (1) the incorrect spelling of a word; (2) incorrect punctuation; (3) omission of a word, punctuation, or passage of text; and (4) when the self-control depleted group did not omit a/e in a word. The 'a's and 'e's of omitted words or passages in the text were not counted towards the number of 'a's and 'e's omitted by participants.

Analyses. The analysis for the two groups in Experiment 3 was identical to Experiment 2, except for the addition of independent

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[7] Instructions were worded in an autonomy depriving manner for the self-control depleted group, and autonomy neutral manner for the control group (Englert & Bertrams, 2015b).Experimental Group Instructions: "You now have to transcribe the following text by hand on a separate sheet of paper. You have to transcribe the text as quickly as possible and you also have to avoid mistakes. Finally, you have to always omit the letters 'e' and 'a' while transcribing the text. You must follow these instructions and you have to work on the task until the experimenter stops you."Control Group Instructions: "Transcribe the following text by hand on a separate sheet of paper. Transcribe the text as quickly as possible and avoid making mistakes. Please work on the task until the experimenter stops you."

[8] Names were considered to reduce the neutrality of the text because of the associations they may hold.

[9] Arranging the sentences as a solid paragraph should require inhibition of surrounding text, scanning for current segment and make switching between sentences more taxing.

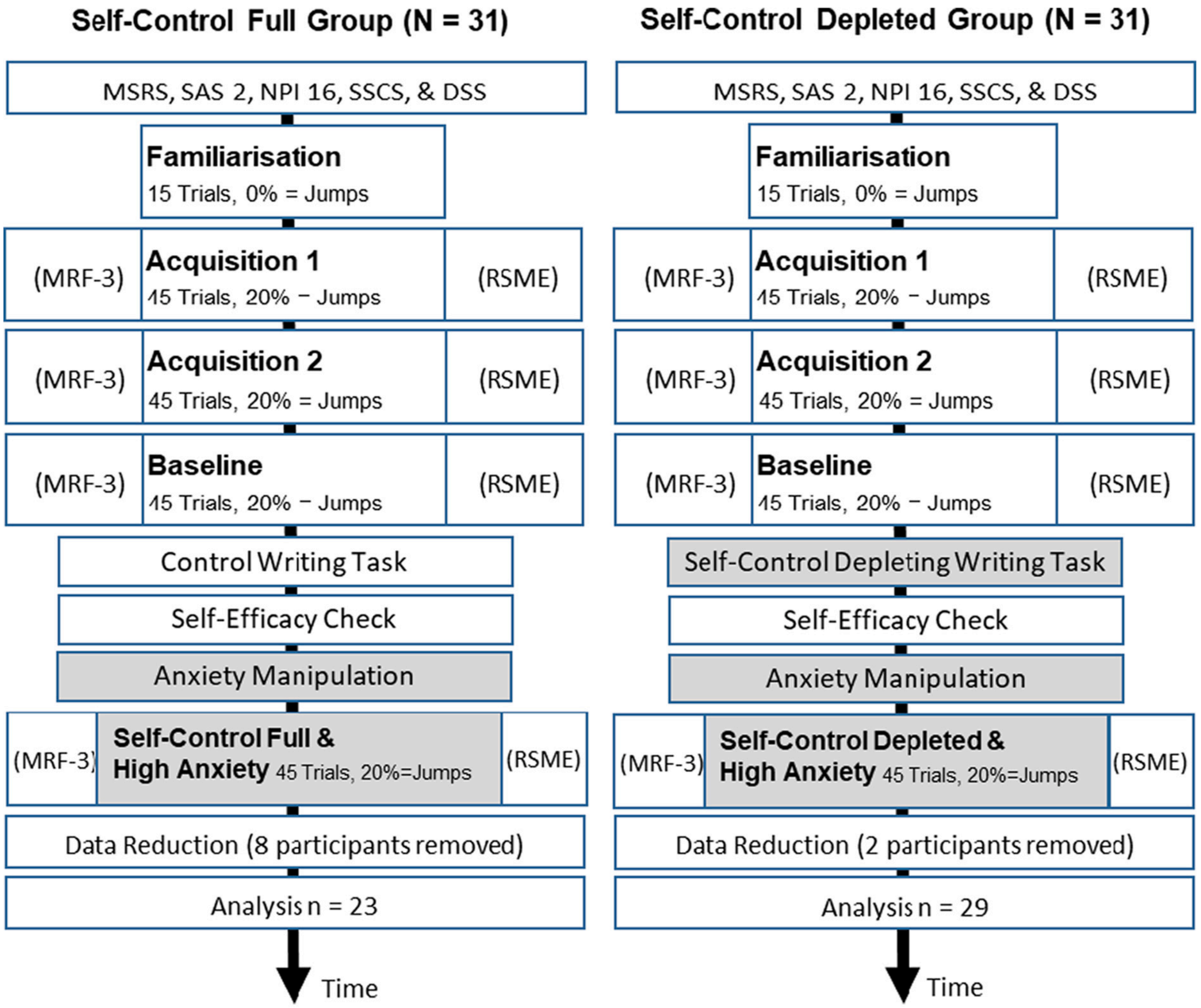

Fig. 7. Experiment 3 Schematic.

Note. Trait personality questionnaires were completed by participants in a randomized order at the beginning of testing. Barring familiarisation, the mental readiness form-3 (MRF-3) was completed before every block of trials) and the rating scale of mental effort (RSME) was completed after every block of trials. In the final block of trials both, the self-control full group and self-control depleted, groups were transferred to heightened anxiety. Participants who did not increase their MRF-3 score from baseline to anxiety were excluded from further analysis during data reduction.

samples *t*-tests for words transcribed, errors in transcription, and A/Es omitted,[10] and the omission of the regression analysis involving trait conscious processing (reinvestment) propensity.[11] Again, the assumption of normality was met by the data and violations to Sphericity were controlled for via Greenhouse-Geisser correction factors.

3.2. Results

3.2.1. Monitored confounds

Conscious processing. Trait conscious motor processing (MSRS) of the self-control full group ($M = 4.27$, $SD = 0.90$) was not significantly different ($t(50) = 0.43$, $p = .666$, 95% CI [−0.44, 0.68]) to the self-control depleted group ($M = 4.15$, $SD = 1.07$).

State anxiety. Trait cognitive anxiety (SAS-2) of the self-control full group (M = 2.90, SD = 2.84) was not significantly different ($t(50) = 0.27$, $p = .79$, 95% CI[−0.41, 0.53]) to the self-control depleted group ($M = 2.84$, $SD = 0.92$). State cognitive anxiety (MRF-3)

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[10] A significant target (left, middle & right) x block (baseline & transfer) interaction was observed for self-control full group's no-jump endpoint VE. However, to be in line with Experiment 1 and 2, and because no significant target x block x group interactions were observed, target data was again collapsed for analyses.

[11] This regression analysis was performed, but likely because of the relatively low sample size for regression analyses (VanVoorhis & Morgan, 2007) and potentially smaller effect size for self-control depletion (Carter & McCullough, 2014) resulted in no significant relations nor clear trends.

analysis revealed a significant main effect for block ($F(1, 50) = 165.32, p < .001, \eta_p^2 = 0.77$), but no significant main effect for group ($F(1, 50) = 1.30, p = .568, \eta_p^2 = 0.01$), nor group x block interaction ($F(1, 50) = 1.98, p = .165, {}_p^2 = 0.04$). As expected, self-control full and self-control depleted groups' anxiety was not significantly different at baseline ($M = 3.65$, $SD = 1.95$; $M = 3.69$, $SD = 2.29$, respectively) nor high anxiety transfer where both groups had their anxiety manipulated in this experiment ($M = 6.26$, $SD = 1.79$; $M = 6.58$, $SD = 2.51$, respectively). Within-group changes in state anxiety between blocks were significant for both groups.

Mental effort. Mental effort (RSME) analysis revealed a significant main effect for block ($F(1, 50) = 21.65, p < .001, \eta_p^2 = 0.30$), but no significant main effect for group ($F(1, 50) = 0.81, p = .373, \eta_p^2 = 0.02$), nor block x group interaction ($F(1, 50) = 0.50, p = .485, \eta_p^2 = 0.01$). However, despite the non-significant omnibus interaction, as hypothesized, the self-control depleted and self-control full groups' mental effort was not significantly different at baseline ($M = 77.48$, $SD = 19.32$; $M = 74.22$, $SD = 21.87$, respectively), but at high anxiety transfer, the self-control depleted group reported significantly greater mental effort than the self-control full group ($M = 90.93$, $SD = 18.42$; $M = 84.13$, $SD = 28.42$, respectively). Within-group mental effort changes were significant for both groups.

Self-control depletion transcription. The self-control full group ($M = 155.00$, $SD = 27.73$) transcribed significantly more words ($t(50) = 3.15$, $p = .003$, 95% CI[8.95, 40.44]) than the self-control depleted group ($M = 130.31$, $SD = 28.36$). Number of errors was also significantly greater ($t(50) = -2.07$, $p = .044$, 95% CI[−4.12, −0.06]) for the self-control depletion group ($M = 4.48$, $SD = 3.71$) than for the self-control full group ($M = 2.39$, $SD = 3.71$). The self-control depletion group omitted 98% of 'a's & 'e's in their transcribed text ($M = 123.52$, $SD = 27.46$) whilst the self-control full group omitted none ($M = 0$, $SD = 0$).

Movement time. For no-jump trials, analysis revealed no significant main effect for block ($F(1, 50) = 0.15, p = .698, \eta^2 < 0.01$), main effect for group ($F(1, 50) = 0.03$, $p = .856$, $\eta_p^2 < 0.01$), nor block x group interaction ($F(1, 50) = 0.48$, $p = .484$, $\eta_p^2 = 0.01$). For jump trials, analysis again revealed no significant main effect for block ($F(1, 50) = 1.99, p = .164, \eta_p^2 = 0.04$), main effect for group ($F(1, 50) = 0.76$, $p = .387$, $\eta_p^2 = 0.02$), nor block x group interaction ($F(1, 50) = 0.04$, $p = .849$, $\eta_p^2 < 0.01$).

Practice effect for jump trials. Again, no participant reported explicitly noticing the cursor jumps, even when debriefed. Constant error of neither the self-control full nor self-control depleted groups changed their outcome correction magnitude across acquisition 1, 2, and 3 ($F(2, 44) = 0.10, p = .905, \eta_p^2 = 0.01$; $F(2, 56) = 0.41, p = .669, \eta_p^2 = 0.01$, respectively for each group). However, variability of both self-control full and self-control depleted groups demonstrated a significant effect across acquisition 1, 2, and 3 ($F(2, 44) = 26.04$, $p = .004$, $\eta_p^2 = 0.23$; $F(2, 56) = 0.5.29$, $p = .008$, $\eta_p^2 = 0.16$). However, in both groups, repeated contrasts breakdown of variability revealed that although acquisition 1 had significantly more variability than acquisition 2, acquisition 3 was not significantly different from acquisition 2. Therefore, this finding was deemed not to delegitimise differences observed between baseline and high anxiety transfer.

3.2.2. No-jump trials

No-jump constant error. Analysis found no significant 2 group × 2 block × 4 distance, nor 2 block × 4 distance interaction ($F(1.59, 79.51) = 0.21, p = .757, \eta_p^2 < 0.01$; $F(1.60, 81.42) = 1.62, p = .208, \eta_p^2 = 0.03$, respectively). Endpoint constant error analysis revealed no significant main effect for block ($F(1, 50) = 0.05, p = .821, \eta_p^2 < 0.01$), group ($F(1, 50) = 0.01, p = .930, \eta_p^2 < 0.01$), nor block x group interaction ($F(1, 50) = 0.06$, $p = .802$, $\eta_p^2 = 0.00$).

No-jump variability. Contrary to expectations, analysis found no significant 2 group × 2 block × 4 distance, nor 2 block × 4 distance interaction ($F(1.85, 92.29) = 0.39, p = .661, \eta_p^2 = 0.01$; $F(1.85, 92.29) = 1.54, p = .221, \eta_p^2 = 0.03$, respectively). Similarly, neither the baseline nor high anxiety transfer block, respectively, had significant 2 group × 4 distance interactions. However, post-hoc breakdown revealed that at high anxiety transfer, whilst the self-control depleted group increased their variability significantly as a function of distance, the self-control full group did not increase significantly between 75% and 100% of distance (see Fig. 8). Variability at 25% of distance did not differ significantly between groups or blocks. Endpoint variability analysis revealed a significant main effect for block

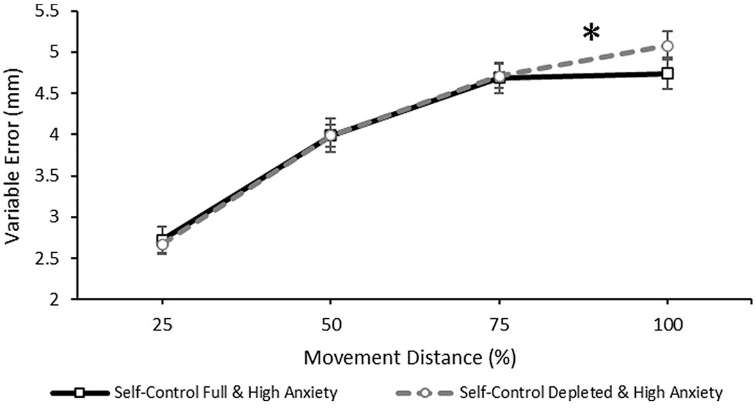

Fig. 8. Experiment 3 No-jump trial variability.
Note. Experiment 3 no-jump variability of self-control full and self-control depleted groups at transfer at 25, 50, 75, and 100% movement distance. The asterisk represents the significant interaction between the self-control full and self-control depleted groups' variability between 75 and 100% distance. Error bars = +/−1standard error of the mean.

($F(1, 50) = 8.63$, $p = .005$, $\eta_p^2 = 0.15$), but no main effect for group ($F(1, 50) = 0.54$, $p = .465$, $\eta_p^2 = 0.01$) nor group x block interaction ($F(1, 50) = 1.39$, $p = .245$, $\eta_p^2 = 0.03$). Breakdown revealed both groups decreased their endpoint variability in high anxiety transfer compared to baseline, but only the self-control full group did so significantly (see Fig. 8).

3.2.3. Jump trials

Jump constant error. Although no significant group x block x distance interaction was observed ($F(1.60, 79.83) = 0.42$, $p = .615$, $\eta_p^2 = 0.01$), a significant block x distance interaction was ($F(1.60, 79.83) = 11.46$, $p < .001$ $\eta_p^2 = 0.19$). Within-group breakdown showed that self-control full and self-control depleted groups had no significant main effect for block ($F(1,22) = 0.14$, $p = .711$, $\eta_p^2 = 0.01$; $F(1, 28) < 0.01$, $p = .964$, $\eta_p^2 < 0.01$, respectively) but did have significant block x distance interactions ($F(1.50, 33.07) = 6.78$, $p = .007$, $\eta_p^2 = 0.24$; $F(1.65, 46.18) = 4.70$, $p = .019$, $\eta_p^2 = 0.14$, respectively). Repeated contrast breakdown showed that these interactions took place between 75 and 100% distance for both groups (see Fig. 9). Endpoint constant error showed a significant block ($F(1, 50) = 8.24$, $p = .006$, $\eta_p^2 = 0.14$) and group ($F(1, 50) = 4.81$, $p = .033$, $\eta_p^2 = 0.09$) main effect, but no significant block x group interaction ($F(1, 50) = 0.46$, $p = .503$, $\eta_p^2 = 0.01$). At baseline, self-control full ($M = -9.87$ mm, $SD = 2.63$) and self-control depleted ($M = -8.56$ mm, $SD = 3.19$) groups did not differ significantly, but at high anxiety transfer self-control full ($M = -11.12$ mm, $SD = 2.33$) had a significantly greater correction magnitude than the self-control depleted group ($M = -9.33$ mm, $SD = 2.97$). Note, the self-control full group significantly increased its endpoint correction magnitude from baseline to high anxiety transfer, whereas the self-control depleted group did not (see Fig. 9).

Jump variability. No significant group x block x distance interaction ($F(1.80, 90.20) = 1.91$, $p = .158$, $\eta_p^2 = 0.04$) nor block x group interaction ($F(1, 50) = 1.56$, $p = .218$, $\eta_p^2 = 0.03$) were found. Correspondingly, analysis of outcome variability found no significant

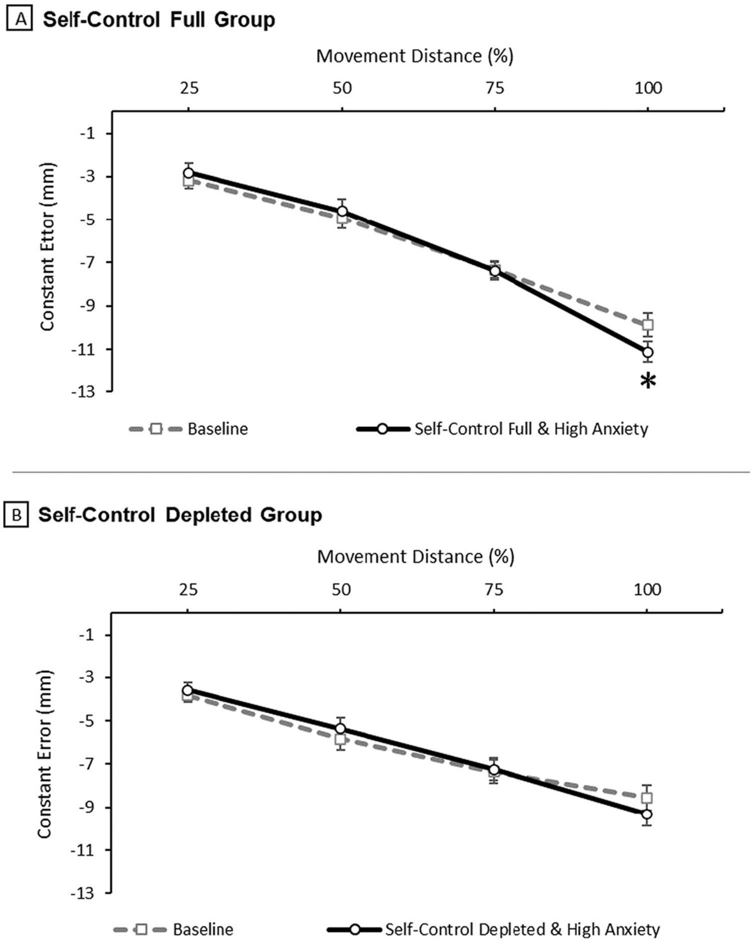

Fig. 9. Experiment 3 Jump trial constant error.

Note. Panel A (top) shows Experiment 3's self-control full group jump trial constant error at low anxiety baseline and high anxiety transfer (with self-control resources intact in both trial blocks). The asterisk represents the significantly greater high anxiety endpoint correction of the self-control full group at 100% movement distance compared to their baseline. Panel B (bottom) shows Experiment 3's self-control depleted group jump trial constant error at low anxiety baseline (with self-control resources intact) and transfer to heightened state anxiety (with self-control resources depleted). Error bars = +/−1standard error of the mean.

main effect for block ($F(1, 50) = 0.01$, $p = .940$, $\eta_p^2 < 0.01$), group ($F(1, 50) = 3.05$, $p = .087$, $\eta_p^2 = 0.06$) nor group x block interaction ($F(1, 50) = 2.55$, $p = .117$, $\eta_p^2 = 0.05$).

Correction latency. No significant main effect for block ($F(1, 50) = 0.07$, $p = .786$, $\eta_p^2 < 0.01$), main effect for group ($F(1, 50) = 0.53$, $p = .471$, $\eta_p^2 = 0.01$) nor block x group interaction was observed ($F(1, 50) = 1.04$, $p = .314$, $\eta_p^2 = 0.02$). I.e. self-control full and self-control depleted group correction latency did not differ significantly at baseline ($M = 196.60$ ms, $SD = 26.25$; $M = 195.71$ ms, $SD = 41.03$, respectively) nor high anxiety transfer ($M = 188.73$ ms, $SD = 19.88$; $M = 200.25$ ms, $SD = 40.28$). Latencies did also not differ significantly within-groups between blocks.

3.3. Discussion

Regulating cognitive resources to counteract the distraction-based effects of anxiety, is proposedly initiated through limited-in-quantity self-control (Englert & Bertrams, 2015a). Therefore, when individuals are depleted of self-control resources, compensatory mobilisation of cognitive resources should be more difficult, and distraction-based effects on motor performance should be amplified. The primary aim of Experiment 3 was to use self-control depletion to specifically highlight distraction effects, to further differentiate how offline planning and online correction processes are affected by anxiety mechanisms.

Our investigation into this aim was facilitated by a successful anxiety manipulation; both groups reported significantly increased state anxiety in the high anxiety transfer block. Likewise, the self-control depletion manipulation appeared effective in demanding differing levels of self-control from participants. The self-control depleted group, which had to overwrite automatic writing habits to omit the letters ‘e’ and ‘a’ in their transcription, wrote significantly fewer words and made significantly more errors than the self-control full group. Accordingly, the self-control depleted group subsequently reported significantly greater mental effort *during* the high anxiety transfer block, than the self-control full group. This suggests the self-control depleted group’s ability to use self-control to mobilise anxiety-compensatory cognitive resources was successfully reduced by the transcription task and made the aiming task more difficult/effortful. Additional extraneous variables that may account for group differences also exhibited no significant differences, namely: self-efficacy, narcissism, trait cognitive anxiety, trait conscious processing, trait self-control resources, and trait self-control depletion sensitivity.

Although no significant differences in offline planning were observed, breakdown of online correction results revealed our hypothesized effects as significant. In no-jump trials, the self-control depleted group displayed significantly reduced online correction, compared to the self-control full group. Concurrently, in jump trials, the self-control depleted group did not increase its correction acuity under heightened anxiety, whereas the self-control full group did. This suggests that, as hypothesized, online correction of cursor trajectories were impaired in the self-control depleted group, possibly because they had insufficient self-control available to mobilise sufficient cognitive resources. Overall, Experiment 3 lends support to the conclusions drawn from experiments 1 and 2; elements within the online processing of reafferent cursor information for error detection and correction, likely require attentional resources to function.

3.3.1. General discussion

A wealth of literature has rationalised heightened state anxiety’s effect on outcome performance (e.g., target accuracy) via distraction or conscious processing mechanisms (Beilock & Carr, 2001; Gucciardi & Dimmock, 2008; Hardy et al., 1996; Murray & Janelle, 2003; Nieuwenhuys et al., 2008). However, it is far from clear whether these mechanisms are indeed mutually exclusive, and whether they differentially affect proposedly effortful offline movement planning and proposedly automatic online movement corrections (Allsop et al., 2017; Roberts et al., 2018). For the first time: (1) heightened state anxiety’s effect on online movement correction was directly tested in cursor jump trials; (2) heightened anxiety’s effect on jump trials’ correction initiation latency, as well as endpoint acuity, was examined; (3) participants’ trait conscious processing propensity was used to predict distraction and conscious processing effects on movement planning and correction; and (4) participants were experimentally self-control depleted to further differentiate between distraction and conscious processing effects.

3.3.2. Monitored confounds

Facilitating this study’s aims were successful anxiety manipulations and absence of numerous key confounds. Firstly, across all three experiments, participants reported significantly greater cognitive state anxiety and invested mental effort in heightened anxiety transfer, compared to low anxiety baseline blocks, suggesting our state anxiety manipulation was successful. Secondly, jump and no-jump trials’ respective MT featured no significant differences between low anxiety baseline and high anxiety transfer; therefore, observed changes in error between these trial blocks should not be down to speed-accuracy trade-offs. Thirdly, in line with past literature, jump trials’ correction endpoint variability and constant error did not change across acquisition blocks (Brière & Proteau, 2011; Proteau et al., 2009; Veyrat-Masson et al., 2010); thus, subsequent changes in jump corrections between low anxiety baseline and high anxiety transfer should not be down to practice effects. Finally, in Experiment 3 the self-control depleted group transcribed significantly less text, made significantly more errors, and importantly reported significantly more invested mental effort in the high anxiety transfer block compared to the self-control full group; thus suggesting the self-control depletion manipulation was successful.

3.3.3. Performance measures and theoretical implications

The present study directly tested online correction efficacy under heightened state anxiety via cursor jumps. No-jump trials required a cursor to be moved rapidly, through and past, a target 24 cm away from a start position. Jump trials were identical but featured a large lateral cursor perturbation early in movement. Therefore, in no-jump trials, participants were free to use a combination of offline planning and online correction to attain their level of outcome performance. In jump trials however, because of cursor perturbations' infrequent, consciously undetected, and random occurrence in peripheral vision, offline planning was impertinent, and online correction was necessitated for good outcome performance. In no-jump trials, improved offline planning was inferred from reduced variability at 25% of distance, and improved online correction was inferred from greater reductions (i.e., levelling off) in variability between 75% and 100% of distance (Khan et al., 2004, 2006). In jump trials, improved online correction was inferred via reduced endpoint constant error *and* variability (Veyrat-Masson et al., 2010).

In line with numerous past studies (Allsop et al., 2017; Cassell et al., 2018; Lawrence et al., 2013; Roberts et al., 2018), we initially hypothesized that heightened state anxiety would reduce online correction in no-jump and jump trials, whilst increasing or not changing offline planning in no-jump trials. Although, no-jump trials in Experiment 1 seemed to exhibit maintained offline planning and reduced online correction, jump trials surprisingly exhibited increased online correction. Experiment 2 replicated this pattern of results while using a considerably larger sample size (i.e., ~4× greater) and a more robust design (i.e., randomized controlled trial design). No-jump trials exhibited increased offline planning and reduced online correction, while jump trials again exhibited increased online correction. Additionally, Experiment 2 demonstrated that reductions in no-jump trial online correction were predicted by higher trait conscious processing propensity and that jump trial corrections were not initiated with reduced latency but seemingly increased spatial acuity. In Experiment 3, when participants were experimentally depleted of their self-control, and were therefore less able to mobilise anxiety-compensating cognitive resources, online correction acuity was reduced in jump and no-jump trials. The findings of experiments 1, 2, and 3 provide the first direct evidence to conjecturing that online correction processes of limb/cursor trajectories may not be wholly automatic (Roberts et al., 2018). Were online correction processes indeed wholly automatic and outside of attention, it should not have been affected by changes in anxiety and cognitive resource.

Prior research has demonstrated that online cursor-based corrections exhibit numerous characteristics synonymous with automaticity. Online corrections have been shown to initiate without the knowledge of participants (Proteau et al., 2009), effective from the first trial (Brière & Proteau, 2011; Proteau et al., 2009), independent of cued attention (Reichenbach et al., 2014), and be uninhibitable (Franklin & Wolpert, 2008). Although at first glance our findings appear at odds with these findings, they may actually complement them. Firstly, we propose that heightened anxiety can make individuals adopt an offline planning-focused strategy wherein they plan 'not' to make otherwise consistent/automatic-like online corrections (i.e., they attenuate online correction initiation because of the task constraints, see Bennett et al., 2012; Elliott et al., 2014; Khan & Franks, 2003). Secondly, individuals' propensity to adopt this strategy may be an attempt to gain more explicit control over movements and are positively predicted by conscious processing propensity. A similar strategy has been observed by Khan, Elliott, Coull, Chua, and Lyons (2002) when individuals were uncertain about upcoming visual conditions. Thirdly, when the need for correction is great enough (e.g., via artificial cursor jumps), participants' strategy to attenuate online corrections may be automatically overturned online. Previous studies (e.g., Franklin & Wolpert, 2008) may have found online correction to be uninhibitable and give the impression of automaticity, because the need for correction created by cursor jumps was too great. Lastly, endogenously allocated (i.e., within-participant) cognitive resources may facilitate *optimal* online spatial processing. The availability of these resources may benefit correction efficacy when individuals' offline planning-focused strategy is overturned. Previous studies (e.g., Reichenbach et al., 2014) may not have observed changes in online correction efficacy, despite manipulating attention allocation, because large endogenous increases in attention specifically aimed at improving the spatial characteristics of performance are necessary to alter the efficacy of online corrections based on reafferent limb information. Anxious distraction-compensatory mechanisms may elicit such an endogenous change.

Admittedly, the notion that this strategy shift can be reversed online is speculative. Yet, in the face of limited research specifically on anxiety-induced endogenous resource shifts, this certainly warrants further scrutiny. A contrasting explanation may involve the impulse control phases of online control (Elliott et al., 2017) via the efference copies which store representations of the motor commands used to initiate a movement (Miall & Wolpert, 1996; Wolpert, Ghahramani, & Jordan, 1995; Wolpert & Kawato, 1998). From these representations, feed-forward models can generate expected trajectories to inform online correction. It is conceivable that anxiety-induced distraction may divert resources away from the creation and/or utilisation of these efference copies, or that anxiety-induced conscious processing may induce the breakdown of automatic efference copy creation and/or utilisation. Regardless of the precise distraction/conscious processing mechanisms, an impaired efference copy (and therefore impaired feed-forward model) in no-jump trials may cause underestimation in error: resulting in reduced no-jump online correction under heightened anxiety, even when sufficient cognitive resources are available. In jump trials on the other hand, cursor perturbations likely produce more error than inherent noise in the motor system, and therefore the need for corrections may be more apparent/less likely to be underestimated. Concurrently, impaired efference copy processes may reduce the salience of planning-based trajectory expectations and allow online corrections to organise themselves more freely and appropriately to cursor perturbations specifically: provided sufficient cognitive resources are available. Additionally, in line with Paillard and Amblard (1985) two semi-independent visual channel model, the online control of movement direction operates in peripheral vision and early in the movement trajectory. Therefore, the online control processes responsible for the increased online corrections seen in the cursor jump trials appear to occur in the impulse control phases of the multiple processes model of limb control (Elliott et al., 2017).

Although the present set of experiments have gone some way in elucidating the distraction and conscious processing mechanisms which underlie changes in offline planning and online correction under heightened anxiety, further investigation is still needed. Our results in Experiment 2 suggest that individuals high in conscious processing propensity are more likely to attenuate their online correction when the need for correction is low (i.e., in no-jump trials). Concurrently, manipulation of self-control in Experiment 3, demonstrates that changes in online correction under heightened anxiety may be down to a change in cognitive resources. The present findings, along with those of Roberts et al. (2018), strongly suggest that conscious processing and distraction effects extend beyond the dichotomous theorising of Lawrence et al. (2013). Both offline planning and online correction can seemingly benefit from increased cognitive resources mobilised by distraction-compensatory mechanisms. Therefore, conversely to the theorising of Lawrence et al. (2013), distraction may be able to affect online correction and conscious processing may be able to affect offline planning. It is also conceivable that distraction and conscious processing effects can take place concurrently, especially if conscious processing/reinvestment is a coping strategy for distraction. In such instances, conscious processing might either act as an additional source of 'distraction' which combined with already present sources of worry/distraction overloads attention and impairs motor control and performance (for similar proposals see Nieuwenhuys & Oudejans, 2012) or result in distracted/ill-conceived attempts to engage in conscious processing/reinvestment.

3.3.4. Potential limitations and future directions

To date, a key limitation of investigations into anxiety's effect on offline planning and online correction, is that reductions in outcome performance were not observed under heightened anxiety (Allsop et al., 2017; Roberts et al., 2018). This suggests that, based on distraction theories, the deleterious effects of heightened state anxiety are overcome by participants via the mobilisation of additional cognitive resources (Eysenck et al., 2007). Therefore, whilst the motor control strategies underlying maintained/improved outcome performance under heightened anxiety have been elucidated by past studies and the present experiments, the motor control changes which underlie distraction-based performance failure are not. Firstly, the two-dimensional aiming tasks utilised by the current and previous investigations (e.g., Allsop et al., 2017) may not be cognitively demanding enough for the introduction of anxiety to overload cognitive capacity. Secondly, laboratory-based and relatively artificial anxiety manipulations (e.g. monetary prize-draws and leaderboards) may not increase anxiety sufficiently nor reliably enough to overload cognitive capacity. Evidence to support these points stems from Experiment 2 and 3 when non-anxious participants were not removed from high-anxiety groups prior to analysis. Specifically, effect sizes diminished and analyses concerning online control became non-significant ($p > .05$). Future studies are encouraged to increase task difficulty and attempt to utilise more ecologically valid anxiety manipulations to overload participants' attentional capacity and clearly induce a reduction in outcome performance.

Relatedly, it is uncertain how anxiety-induced conscious processing mechanisms manifest themselves on offline planning and online correction when outcome performance is impaired. It also cannot be said for certain whether prior studies investigating anxiety's effect on motor control (e.g., Allsop et al., 2017) observed conscious processing effects at all: it is possible that both impairments in online correction and improvement in offline planning under anxiety were the product of resource-based distraction mechanisms. Future studies are encouraged to include conditions with and without opportunities to generate rule-based declarative knowledge because this should determine conscious processing-induced deautomatisation of motor control (Bellomo, Cooke, & Hardy, 2018; Masters & Maxwell, 2008).

Another limitation of the present study is its computer-based upper-limb aiming task's simplicity and low ecological validity when compared to the complex sport skills which are commonly performed under anxiety (e.g., a penalty kick in soccer). Consequently, a three-dimensional target-directed aiming task (e.g., Carrozzo, McIntyre, Zago, & Lacquaniti, 1999) should be utilised in future research to increase generalisability and potentially reveal new effects on offline planning via the use of ellipsoids (Hansen, Elliott, & Khan, 2008). Specifically, previous investigations have not observed a negative effect on offline planning efficacy when anxiety was heightened (e.g., Allsop et al., 2017; Lawrence et al., 2013; Roberts et al., 2018). It is conceivable that the present and previous use of primarily vision-based two-dimensional aiming tasks is not sufficiently complex for anxiety to overload cognitive capacity, as proposed necessary by the attentional control theory for performance decrements to occur (Eysenck et al., 2007). The additional degrees of freedom offered by three-dimensional tasks and potentially greater relevance of proprioceptive information in addition to vision, may prove demanding enough on cognitive demands to for the additional demands of anxiety to overload capacity and demonstrate negative effects of anxiety on offline planning processes. Such research with greater generalisability may also facilitate a step towards applied implications, wherein practitioners may wish to consider whether athletes who have a high propensity to consciously process movement (thus possibly more likely to interfere with online correction) are suited to highly dynamic and continuous tasks (e.g., kayaking or downhill mountain biking).

4. Conclusion

The present study aimed to elucidate how anxiety-induced distraction and conscious processing mechanisms affect the offline planning and online correction of movements. Results reaffirm that heightened anxiety can benefit offline planning. Conversely to past suggestions however, when necessitated by cursor jumps, online correction was not 'impaired' and instead exhibited improved spatial acuity under heightened state anxiety (provided that sufficient compensatory cognitive resources were mobilised/available). The implications of this finding are threefold: firstly, both offline and online correction (in the current task, this is likely to be associated with the impulse phases of online control) appear to benefit from increased cognitive resources mobilised by distraction-compensatory mechanisms; therefore secondly, the dichotomous reasoning of Lawrence et al. (2013), that distraction mechanisms only affect offline planning and conscious processing mechanisms only affect online correction, is questionable; and finally, data provide strong evidence against the labelling of online cursor/limb trajectory correction processes as wholly automatic and attention free. Were this form of online correction truly automatic and outside of attention, correction efficacy should not have changed within or between participant groups, across the same conditions. We propose that the impulse phases associated with the online correction of limb trajectories be defined as highly consistent, but inhibitable by visuomotor performance strategies and improved by increases in endogenously allocated cognitive resources focused on optimal spatial functioning. The precise distraction and conscious processing mechanisms that underlie anxious movement planning and correction require further investigation.

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All participants gave informed consent prior to participating and gave consent for their anonymised data to be published.

CRediT authorship contribution statement

Gavin P. Lawrence: Conceptualization, Methodology, Writing – review & editing. **Robin Owen:** Conceptualization, Methodology, Investigation, Data curation, Formal analysis, Writing – original draft, Visualization, Writing – review & editing. **Victoria M. Gottwald:** Conceptualization, Methodology, Writing – review & editing. **Michael A. Khan:** Writing – review & editing.

Declaration of Competing Interest

The authors have no conflict of interest to declare.

Data availability

Data will be made available on request.

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