Attention probes may boost real effects and create pseudo effects: A rerun and reassessment of Hemed et al. (2020)

Abstract

This study documents the potential influence of attention probes in experimental paradigms by addressing their unintended effects on response time (RT) measurements. Attention probes, which are commonly used to assess participant engagement, may introduce task-switch costs that can confound experimental results. We show how probe-induced biases inflated findings related to reinforcement from sensorimotor predictability and generate a fictitious behavioral response to sensory-prediction error. We show that by excluding task trials immediately after attention probes, these biases can be corrected. We validate this approach in two new experiments devoid of probes. The results confirm that response reinforcement from predictable action-effects only accumulate as long as predictions hold. These finding challenges traditional (reward-based) reinforcement models by suggesting a distinct mechanism for reinforcement from sensorimotor predictability. The validated corrective method provides a practical tool for mitigating similar confounds in past and future studies.

Public Significance Statement

To make sure that participants in psychological studies are paying attention to the task at hand, researchers often use attention checks, an additional task that requires a simple response from the participants and is unrelated to what the experiment is designed to measure. Attention checks are important because random responding may mask findings of interest and/or produce pseudo-results. One principled way to verify that participants are attending to the task is to insert a very simple task intermittently and unpredictably (e.g., participants are asked to press the space bar when a yellow triangle appears on the screen) interspersed throughout the main experiment. Here we show that this reasonable, well-acknowledged practice may have deleterious effects on the measurement of participants' behavior that is caused by the well-known psychological effect known as "switch costs". When participants' response times are the key measure and the behavior of interest is indexed by small differences, the result can be dramatic and can mask real findings and generate false ones. Importantly, because of the robustness of switch costs, typical rigorous scientific practices such as replication will simply replicate the confound. We present such a case (our own) as well as a validated adjustment to the analyses which resolves the issue. We then reexamine the pattern of results to reassess the conclusions of the original study. While the corrected pattern still supports the original paper's conclusion that reinforcement by sensorimotor predictability accumulates as long as predictions hold, we found no evidence that 'losing control' is otherwise registered or affects behavior. These results suggest that the reward system may be comprised of different reinforcement circuits, some of which do not involve rewards at all and that one of these may reinforce motor programs in the case of successful sensory predictions.

Introduction

Revision of XGE-2023-1341 as invited by the action editor, Sarah Brown-Schmidt, Ph.D

The prime objective of current study is to alert the experimental-psychology community to a potential pitfall when using a behavioral secondary task of any kind when the experiment involves measuring response time on the primary task. Specifically, we illustrate how attention checks, which are designed to identify and filter out disengaged individuals, can introduce a substantial bias in response times on subsequent trials of the focal task as a result of task switching costs. We also show that this adverse effect can be mitigated, post data-collection. The second objective was to correct the scientific record and in particular our own published findings regarding how humans unintentionally evaluate whether their motor actions control the immediate environment (i.e., own-action effects) when the environment changes dynamically. To achieve these two aims, we ran a set of new experiments that did not employ attention checks, and compared their results to a re-analysis of a set of experiments to study the evaluation of response effectiveness in a dynamic environment, which included attention checks as the secondary task. We discuss the confound, the method we used to mitigate it, and the theoretical implications of the validated results of the above experiments.

Measuring engagement in behavioral research

Testing participant engagement in behavioral tasks and surveys is crucial for valid psychological measurement (Aust et al., 2013). Inattention or disengagement is a longstanding problem in attitude and self-report measures (e.g., surveys; Kung et al., 2018). Various remedies have been put forward such as inserting ‘illogical’ items (e.g., Meade & Craig, 2012) or instructions to respond in a specified manner (e.g., Oppenheimer et al., 2009) to identify inattentive responders.

Revision of XGE-2023-1341 as invited by the action editor, Sarah Brown-Schmidt, Ph.D

There is a general consensus that the validity of psychological measurement on behavioral tasks is dependent on participants' sustained attention, in particular when participants are required to complete hundreds of trials of (sometimes) tedious tasks. There are different methods for measuring and enforcing that participants are indeed paying attention to the task. Often, researchers consider that erratic performance on the main task, such as repeatedly pressing the same key (when the task does not require it), slow response times, or a large number of errors, as evidence that a participant was inattentive and should hence be filtered out (Barbosa et al., 2023; Berger & Kiefer, 2021; Siritzky et al., 2023). Although common, this practice is suboptimal because different tasks vary in difficulty, and different studies may vary in participant populations (e.g., clinical, elderly). Thus, using performance on the focal task itself makes it difficult to determine whether and when these criteria truly reflect inattention, the nature of the task itself, or their interaction.

Another approach, which is generally thought alleviate most of the concerns cited above is the use of infrequent attention check or 'attention-probe' trials (Sauter et al., 2020). In attention-probe trials participants are presented with a simple stimulus demanding a simple response, such as responding using a designated key. Since the response itself is very simple, a participant who fails to respond correctly (or does not respond at all) on more than a handful of such trials may be considered inattentive. This is the most typical way to evaluate sustained attention in diagnostic settings (e.g., the TOVA; for an application in scientific research, see Agay et al., 2014). One of the key benefits of such filtering is that it is independent of the nature of the focal task (e.g., a more difficult task) and can enable a firmer conclusion that inattention is indeed responsible for variations in performance. This practice also affords comparison across experimental paradigms and populations and facilitates a consensus around a normative threshold of exclusion due to inattention.

The problem: hidden (switch) costs

One of the main shortcomings of the attractive practice of including attention probes (or any other secondary task, for that matter) is that switching between tasks induces robust performance costs (Hazeltine, 2024; Kiesel et al., 2010; Wylie & Allport, 2000). Critically, switch costs occur even when the focal and probe tasks involve clearly distinct stimuli and responses. This means that the response times for trials on the focal task that follow the attention probe are influenced by these switch costs.

Although experimental psychologists are familiar with the task switch phenomenon (we were), they may overlook its applicability when these infrequent stimuli are construed as ‘probes’ (we did). Thus, researchers may not be at all cognizant of the danger that switch costs can influence responses times on the task of interest and potentially confound the results. The robust nature of switch costs further compounds the problem, since they will also appear in exact replications of the experiment, thus increasing other researchers’ confidence in these pseudo-effects, even if they were initially unpredicted by the underlying theory.

As demonstrated below, we have learned this lesson rather painfully, but our message here is optimistic, since once researchers are aware of this issue, most of the switch cost can be removed with relatively little loss of data from the focal task trials.

An illustration of the confound, its effects, and their mitigation: Hemed et al., 2020

We discovered this probe-generated confound after publishing a paper affected by it (Hemed et al., 2020). We then published the corrected results as a clarification to the original paper (Hemed et al., 2023). The paper with the confound (Hemed et al., 2020) explored a behavioral phenomenon in which simple reaction-time responses are speeded up by the existence of a non-varying action-effect; i.e., all responses are immediately followed by the same (visual) sensory event. We reported that responses were faster than when no effect followed responses (Eitam, Kennedy, et al., 2013; Hemed, Karsh, et al., 2022; Tanaka et al., 2021) and more interestingly, also in comparison to a response-contingent action-effect that had unpredictable features (e.g., a subtle random jitter in its location between trials; Karsh et al., 2016) or a response-contingent action-effect that was subtly delayed. This ‘feedback’- driven facilitation was weaker when even a minor (150 millisecond) delay was inserted between the response and the action-effect, and vanished completely when the delay was 450 milliseconds long (Eitam, Kennedy, et al., 2013).

This phenomenon has been found to be very robust, at least in choice reaction time tasks where participants’ chose between a number of responses to an imperative cue(s) (Bakbani-Elkayam et al., 2024; Eitam et al., 2013; Hemed et al., 2022; Karsh et al., 2016, 2020, 2021, 2023; Karsh & Eitam, 2015; Penton et al., 2018; Ren, Kaiser, et al., 2023; Tanaka et al., 2021).

As this behavioral effect was found to be sensitive to factors previously associated with other effects theoretically linked to the sensory prediction error (e.g., sensory attenuation;

Blakemore et al., 1998), speeding up due to predictable and immediate sensory feedback has been interpreted as indexing *Reinforcement from Sensorimotor Predictability* (henceforth *RSP*; Bakbani-Elkayam et al., 2024; Hemed, 2020). More specifically, the facilitation of response times is considered to be a function of the match between a forward model – a sensory prediction that is automatically generated by the motor system to facilitate smooth movement (Wolpert & Miall, 1996) – and the incoming sensation that is registered immediately after the execution of a motor command (Hemed, Karsh, et al., 2022).

With this notion in mind, Hemed and colleagues (Hemed et al., 2020) examined changes in response times in response to incoming sensations that dynamically confirmed or disconfirmed the predictions of the forward model. To do so, Hemed and colleagues (2020) used an undulating design, where during a fixed number of trials (5 or 10) participants either received contingent action-effects, or none (in the second experiment, spatially-jittered contingent action-effects replaced the no action-effects). To gauge participants' sustained attention throughout the task as well as discourage them from explicitly figuring out the alternating structure of the action-effects (by counting), Hemed and colleagues inserted attention probes in the task trials (in a semi-random fashion) where participants were instructed to press a single dedicated key when identifying the probe. The probe key was different from the four keys used to respond to the main task ('space' and 's', 'd', 'k', 'l', respectively).

Unbeknownst to us until months after the publication of Hemed et al. (2020), these probes generated substantial costs when the participants switched back to the main task trials (see Figure 1). Crucially, these costs confounded the results because we did not remove them when analyzing the response times on task trials. Specifically, the task trials were binned in the analyses based on whether they immediately followed predictable, response-contingent, action-effects or followed trials in which responses led to no action-effects (Experiment 1) or to action-effects that were not fully predictable due to the insertion of a spatial perturbation (Experiment 2). Since the responses to the attention probes never led to an action-effect (by design), the trials *following* them were always marked as trials following a no (or not fully predictable) action-effect.

<u>The inflation of the predicted result and the generation of a pseudo-finding</u>

The introduction of switch costs into the response times of the task trials had two effects (Hemed et al., 2023). First, they somewhat increased the predicted interaction between the recent history (i.e., whether predictable effects followed trials n-4 to n-2) of action-effects (Context) and the influence of the immediately preceding response contingent and predictable action-effect (Prior) on response times. Although we predicted that a longer history of sensorimotor confirmations would increase the facilitatory influence of the most recent contingent and predictable action-effect on response times (since more precise predictions had been created and confirmed), the evidence for the magnitude of this interaction was artificially inflated by the (decreasing influence of the) switch costs. This is because trials for which the immediately prior response led to a predictable action-effect excluded the attention probe trial (thus these trials never included the maximal switch costs; see Figure 1; right panel). Put differently, when the switch costs were not removed from the analyses of task trial RTs, an *increase* in Context was confounded with a *decrease* in the probability of switch costs influencing response speed. This did not create a negative slope for *both* Prior types (and thus reinforced the interaction effect) because the switch costs dramatically tapered off one trial post switch (see again Figure 1. Right panel). As such, the task switch impacted the reaction time of the next trial the most. This combination created a second ostensive pattern; namely, the slowing down of responses when a relatively long history of contingent and predictable action-effects was followed by the absence of an action-effect (or a partially unpredictable one). While this was not predicted, it was replicated in a second experiment (see Figure 1) due to the robustness of the switch effects, and was interpreted in light of seemingly consistent evidence in the literature (e.g., Wen & Haggard, 2018 who showed the overweighting of what they termed “violations of control”).

Is RSP a new type of reinforcement or a special case of reward learning?

While the corrective analyses (Hemed et al., 2023) suggested that our theoretically predicted interaction between Context and Prior still held (but see, Ren, Kaiser, et al., 2023; Ren, Gentsch, et al., 2023) and the apparent slowing down of responses due to the violation of sensorimotor predictions disappeared, it behooved us both methodologically and theoretically to corroborate the (corrected) pattern of results.

The methodological importance of this corroboration lies in validating the effectiveness of the simple procedure we applied to minimize the influence of the switch costs caused by the insertion of attention probes which consisted of rejecting the task trials that immediately followed the probe. This method can be applied to the data of any completed study in which attention probes or (other secondary tasks) were employed and where the measure of interest is response times on

Revision of XGE-2023-1341 as invited by the action editor, Sarah Brown-Schmidt, Ph.D

the main task, when the switch costs (on the main task) have not otherwise been accounted for, and when they may have been confounded with the design factors.

The theoretical importance of validating the pattern of results is twofold: (1) the lack of evidence for slowing down in the case of violation of strong expectancies or predictions (on response-contingent action-effects) is at odds with at least some reported findings on the sense of agency and control that characterize the effect of ‘losing control’ (Jenkins & Obhi, 2022; Seubert et al., 2024; Villa et al., 2021; Wen & Haggard, 2018), (2) the “step function” pattern of the influence of a violation of predictability on RSP and its relevance for differentiating between the mechanism supporting RSP and that supporting reinforcement from reward learning.

Revision of XGE-2023-1341 as invited by the action editor, Sarah Brown-Schmidt, Ph.D

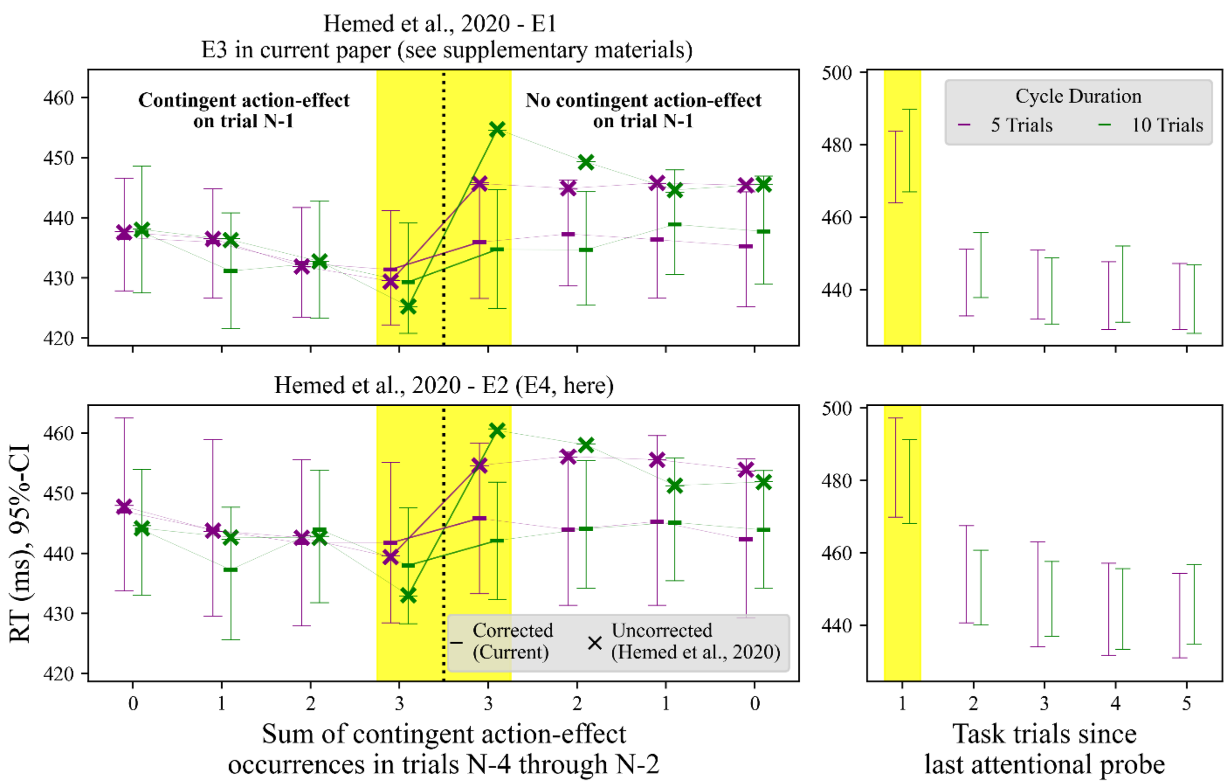

Figure 1: Confounded switch costs boost the real effects in the data and created a false one. In Hemed et al. (2020) we reported that the participants' response speed (Y-axis) tracked the accumulation of contingent, action-effects in an undulating design (left-hand side of the left-hand panels; X marks). These were excessively slow after the predictable action-effects were removed (right-hand side of the left-hand panels; X-marks). We erroneously binned trials after attentional probes as trials that did not immediately follow a contingent action-effect and it was these trials that introduced large switch costs (right-hand panels). When the trials immediately following attentional probes were removed (thick horizontal lines, left-hand panels) the apparent slowing down vanished, but the other statistical results held. Both the vanishing and the extant results were validated by Experiments 1 and 2 in the current manuscript, which did not include attentional probes.

<u>The current study</u>

To validate both the corrected results as well as the method we used to produce them (in Hemed et al., 2023) we replicated our previous work where the only difference was that the experiments did not include attention probes (to fully remove the influence of switch costs). We then compared these probe-less experiments to the results of six different experiments that *did* include attention probes (Hemed et al., 2020; Hemed & Eitam, 2022). Specifically, the results of the six experiments which included attention probes were preprocessed either to include the switch costs, or as in Hemed et al. (2023) by only rejecting the task-trials which immediately followed the switch from a probe[1].

General methods and data processing

Experiments 1 and 2 were identical except for the type of contingent action-effects the participants received. They thus constitute replications of the two experiments of our affected paper (Hemed et al., 2020) without the attentional probes.

Preregistrations of experiments 1-2 are available online (https://osf.io/b6c9a/wiki/home/); The data and analysis code are available online (https://github.com/EitanHemed/patches-papers).

[1] The *Supplementary materials* section provides the methods and results from Experiments 3 through 5, all of which included attention probes, but were otherwise identical to Experiments 1-2. Specifically, Experiments 3-4 are the re-analysis of the experiments from our previous paper (Hemed et al., 2020), and Experiment 5 is a replication of Experiment 4. In addition to Experiments 3-5, in the Meta-Analysis we analyze three experiments with an identical design from another manuscript (Hemed & Eitam, 2022).

<u>Apparatus</u>

The experiments were programmed using PsychoPy3 (Peirce et al., 2019) and deployed online via Pavlovia (Pavlovia.org), due to COVID-19. The visual and temporal parameters were based on the in-person version of the task used in our previous study (Hemed et al., 2020). Participants were recruited using the SONA system in the Department of Psychology at the University of Haifa.

<u>Task</u>

The experiments were run on the participants' personal computers, online. Hence, the stimulus size was adapted to the size of their monitors, and all size units below are specified relative to the height of the participant's monitor (henceforth, **H**). An illustration of the experimental setup can be found in Figure 2A-B. Two rows of four colored rectangles were displayed on a black background (shown in white in Figure 2). The width and height of the rectangles were 0.1H and 0.025H, respectively. Each row of rectangles was vertically centered at 0.225H above or below the center of the screen, and the rectangles were centered at 0.1875H and 0.0625H to the left or to the right of the center of the screen. A white letter (*S*, *D*, *K* or *L*) denoting the key matching each "column" of rectangles (0.025H) was centered at 0.325H above or below the midline of the screen.

A trial began with the appearance of a disc-shaped imperative cue (radius = 0.025H) in the center of one of the four top-row rectangles, and moved downwards on a straight path, at a constant speed for 850MS, which matched the duration of the response window. For 850MS, the cue descended to the matching bottom-row rectangle and then disappeared off the screen. To facilitate performance, the color of the disc also matched the color of the rectangles for that “column”. Each trial was followed by a fixed duration ITI, lasting 700MS. Participants were required to respond using the key matching the horizontal location of the cue (using the ‘S’, ‘D’, ‘K’, ‘L’ keys on their keyboard) using both their left and right index and middle fingers which were poised on the task keys. Participants were also requested to respond as quickly and as accurately as possible.

The experiments were composed of two types of trials: Effect and No-Effect (Experiment 1) or Effect and Partially Unpredictable-Effect (Experiment 2). On the *Effect trials*, if the correct key was pressed during the response window, the cue changed color to white for 100MS, and then disappeared (Figure 2B). Trial duration was fixed, regardless of response accuracy or own-action effects. To maintain constant trial duration, when a correct response was given after 750MS had elapsed from the onset of the response window, the white disc was only shown for the remainder of the response window (i.e., less than 100MS). On the *No-Effect trials*, even if the correct key was pressed, the colored circle did not turn white or disappear for the duration of the response window (Figure 2A). In Experiment 2 we used Partially Predictable Action-Effects instead of No-Effect trials (Figure 2C). In these trials, a correct response also led to the disappearance of the cue and the flash (white disc) appeared at a random location and distance from the last location of the imperative cue. It was displayed and descended downwards for 100ms similarly to the spatially-predictable action-effect described above.

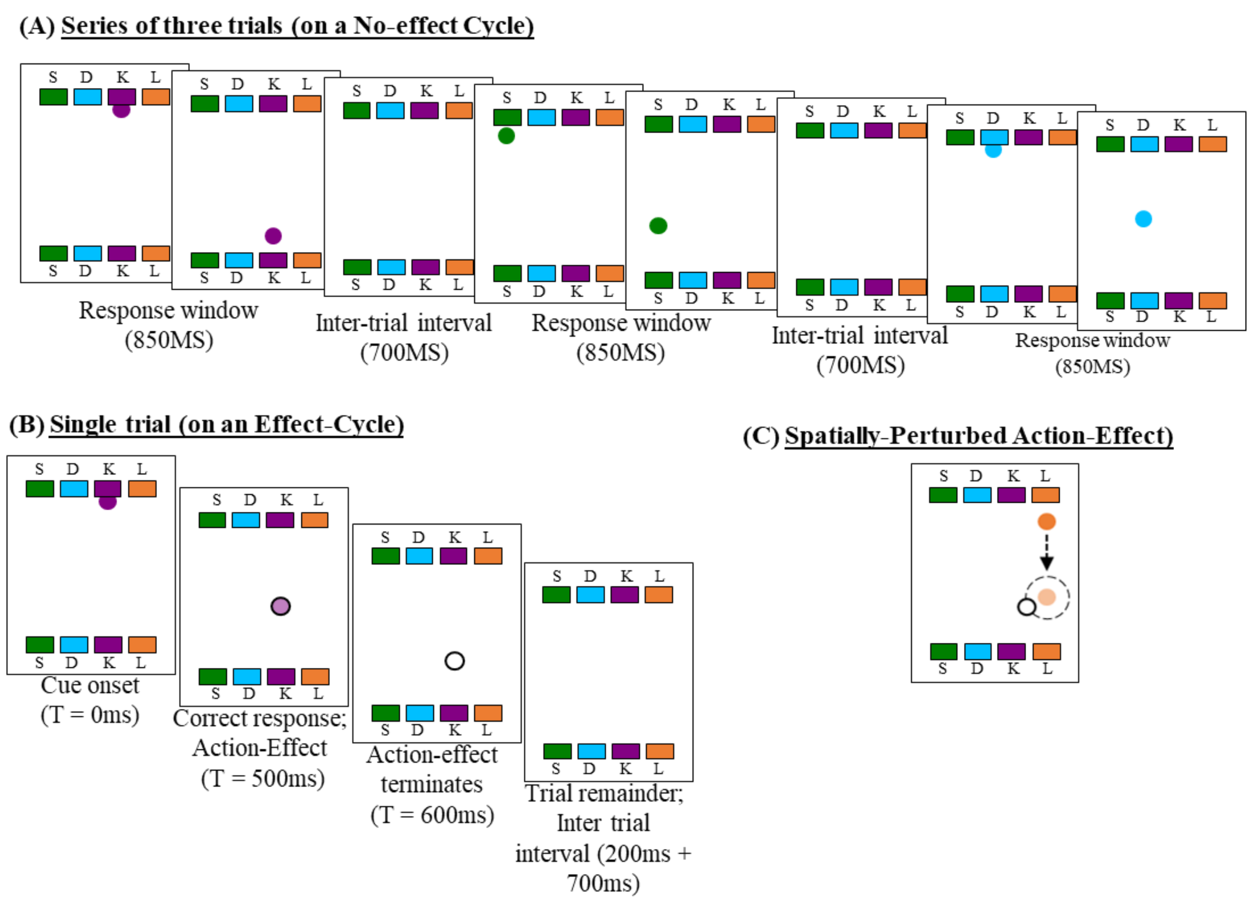

Figure 2: **Experimental task. (A) Series of task trials. Participants were asked to press the key matching the location of the cue (here, K, S and D for the three depicted trials, respectively). On the No-Effect trials (Experiment 1), the cue descended on the screen during the response window (850ms), followed by an intertrial interval of 700ms. (B) On the Effect-Cycles, pressing the key matching the location of the cue (here, K) caused the cue to turn white for 100ms and disappear for the remainder of the response window. (C) To create partial-predictability (Experiment 2), the white disc appeared at a random location around the location of the cue instead of the no visual action-effect used in Experiment 1.**

<u>Design</u>

Participants were assigned to one of two conditions. The only difference between conditions was how frequently the experiment alternated between the Effect and No-Effect cycles (i.e., by alternating every 5 or 10 trials; see Figure 3A), or Partially Predictable Action-Effect cycles (Experiment 2). For the 5-trial Cycle Duration group, the experiment alternated between Action-Effect trials and No-Effect (Partially Predictable Effect) trials (Experiment 2) every 5 trials, whereas for the 10-trial group, every 10 trials. The order of the cycles was counterbalanced. Cycle-length was not found to consistently affect the results in Hemed et al. (2020), except for the specific interaction when action-effects were removed (i.e., Prior$_0$ and Context$_{3)}$; however, they were included in the current study to maintain the original study's structure.

The experimental trials were preceded by a short practice block, during which the trials were similar to the experimental block, except for the following: (a) the imperative cue did not descend the length of the screen but appeared in the middle of each 'column', (b) the response window was 2S long, and (c) an on-screen text notification was shown throughout each trial stating which key was correct on the current trial (e.g., "Press K"). A training block was not included in our previous paper (Hemed et al., 2020), but was important to include here, given that the task was completed remotely, in the absence of the experimenter.

<u>Procedure</u>

Following an informed consent slide, the participants were shown an instruction slide for the practice phase. Then they completed a short practice block (20 trials). After a slide showing reminders about the task requirements, the participants engaged in a single experimental block that was 440 trials long. During the whole experiment, each of the four cue locations (i.e., response keys) was repeated 115 times, all in random order. Throughout the experiment, an on-screen counter displayed the participant's progress in the experiment (as the percentage of completed trials). After the experiment, the participants filled out the Sense of Agency scale (Tapal et al., 2017) and a demographics questionnaire.

<u>Data preprocessing</u>

The data were pre-processed almost identically to the procedure in Hemed et al. (2023). Preprocessing was composed of two main stages as depicted in Figure 3B: (a) binning by counting the number of action-effect occurrences obtained on previous trials and (b) the removal of invalid trials (the only deviation from Hemed et al., 2023 was that we removed trials after erroneous responses, to also exclude post-error slowing). First, we binned trials based on their recent own-action effect occurrences, as described below. This enabled us to capture the dynamic nature of the task, where the presence of an effect changed every fixed number of trials.

1. For each participant, the series of all task trials with correct responses was extracted and trials were labelled as either belonging to a Contingent-effects cycle or a No-effects/Partially (un)predictable-effects cycle.
2. Next, we assigned each of these trials the value of the Contingent-effect on trial N-1(either 0 or 1). This indicated the absence (0) or presence (1) of a fully predictable response contingent-effect on the most recent task trial for which a correct response

had also been given. Erroneous task trials (or task trials with no response) were skipped. This measure was termed *Prior*.

3. Next, we summed the number of fully predictable response contingent-effect on trials N-4, N-3 and N-2 of each trial, again only considering correct-response task trials. This measure was termed *Context* and was assigned the sum of the preceding action-effects (0, 1, 2, or 3).

The factor of what happened in the preceding trial (i.e., *Prior*; N-1) represented the immediate effect of obtaining an action-effect, and Context captured the effect of changes in the action-effect contingency incorporating less recent 'history'.

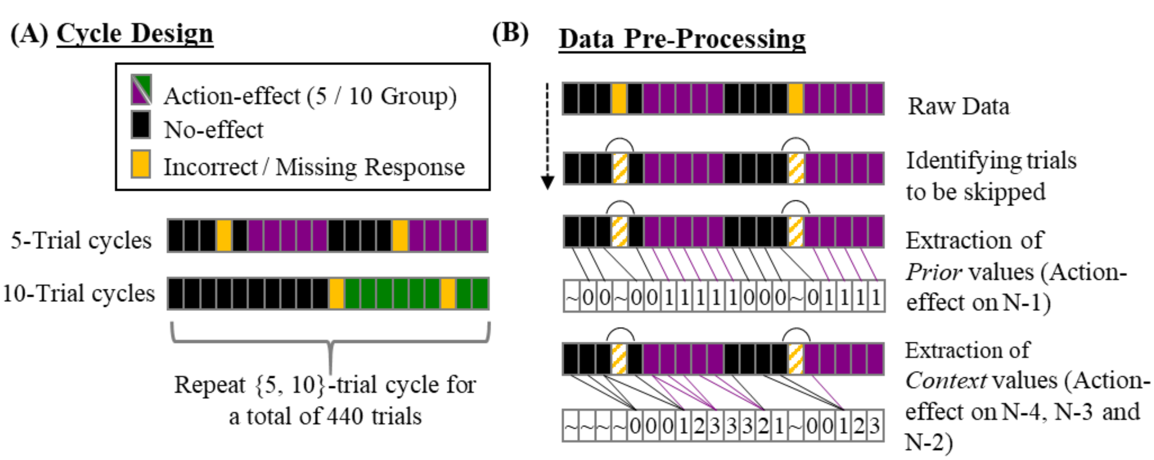

Figure 3. Design and data preprocessing. (A) Every 5 or 10 trials (manipulated between groups) the experiment alternated between Effect and No-effect cycles. (B) The binning of trials into bins of *Prior* (0 or 1 action-effect occurrences on the preceding valid trial) and *Context* (0, 1, 2, or 3 action-effect occurrences on valid trials N-4 through N-2). Invalid trials were skipped for the purpose of assigning *Prior* and *Context* values. Purple diagonal lines indicate 'Action-effect', black diagonal lines indicate 'No action-effect (or a Partially (un)predictable action-effect).

After binning the trials based on the action-effects on preceding respective trials, we employed a second stage of preprocessing. In this stage, we marked trials as invalid (i.e., trials that should not be analyzed), based on several criteria:

1. Task trials where no correct response was given (i.e., response omission or incorrect response), and the trials immediately following them (since they could reflect post-error slowing).
2. Task trials with a correct response, but extremely slow or fast response times. While we did not analyze extremely fast (<100MS) or slow (>750MS) correct responses which accounted for ~1-2% of all the responses in each experiment (as specified below), they were not considered incorrect responses (because they led to an action-effect based on the type of cycle they were in). Thus, trials immediately following an outlier due to slow or fast responding were not removed.
3. Trials that could not have defined *Prior* and *Context* values (e.g., the first trial in a session) were not analyzed.

Statistical modeling

Statistical modeling was the same for both experiments. First, we ran a 2 X 2 X 4 repeated measures ANOVA that tested the joint effects on response time of *Cycle Duration* (5 or 10 trials), Action-effect on the prior trial (N-1; Action-Effect, No-effect) and *Context* of Action-effect occurrences on trials *N-4* through *N-2* (i.e., either 0, 1, 2 or 3 occurrences). The ANOVA was run with Cycle Duration as a between-subject factor, and the effect of *Prior* action-effects and *Context* as within-subject factors.

After the ANOVA, we used pairwise contrasts to further quantify the pattern of change in response speed by using a frequentist t-test with a Bayesian t-test. In line with our previous work, we quantified the dynamic changes in action-effect contingencies by comparing the differences in response time between the two poles of Context (i.e., Context3 vs. Context0), depending on whether participants had just experienced a contingent and fully predictable action-effect; i.e., on Trial N-1 (Prior1), or had not experienced one in the immediately preceding trial (Prior 0). Thus, four contrasts, two for each of the Cycle-Duration groups, were tested in each experiment.

Transparency and Openness

The experiments were pre-registered on the Open Science Framework (OSF; https://osf.io/b6c9a/wiki/home/) where we report all data exclusions, manipulations, and measures in the study. Upon pre-registration we committed to collecting data from 60 valid participants (i.e., who completed the experiment) for each experiment, and then continued to sample participants until the Bayesian analysis of the four key contrasts produced conclusive results (see below, $BF_{1:0} < 0.33$ or $BF_{1:0} > 3$). All statistical analyses were performed using Python's *robusta* package 0.0.4 (Hemed, 2022), except for the meta-analysis (for which custom code was written). Plotting was mostly done using *Seaborn* (Waskom, 2021). The experimental materials are available upon request.

Specifics of the two individual experiments

Experiment 1 and 2 differed solely in the feedback used on the cycles complementing the fully predictable contingent action effect cycles. The no- effect and partially- predictable effect manipulations were both used in Hemed et al., 2020, as well as in other studies (e.g., Karsh et al., 2016; Tanaka et al., 2021). In Experiment 1, there was no change to the imperative cue given response (see Figure 2A). In Experiment 2 the feedback appeared at a spatially unpredictable location surrounding the location of the cue upon response (see Figure 2C).

Experiment 1

In Experiment 1, we used the task described above, which was previously shown to capture the reinforcement of behavior by immediate, fully-predictable behavior-contingent action-effects; namely, RSP. Our key goal was to test whether the approach taken by Hemed et al. (2023) for removing the lion’s share of probe-induced switch costs could be validated by simply removing the attention checks.

Methods

Participants

We recruited 127 participants. One participant failed to provide demographic data. Participants’ gender demographics were collected using the radio-button style question included in the demographic questionnaire. When presented with the prompt 'I’m a-' the dropdown listed *Female,*

Male, *Other* and *Rather not say*. In the 5-trial group, there were 63 participants, aged 19 to 35 ($M = 24.79$, $SD = 3.50$). Of these, 48 responded Female and 15 responded Male. In the 10-trial group, there were 63 participants, aged 18 to 44 ($M = 24.65$, $SD = 5.29$). Of these, 50 responded Female, and 13 responded Male. No information on race, religion or ethnicity was collected.

<u>Results</u>

Data preparation and screening

All data from the training block were discarded prior to screening or analysis. Note that data from a participant or a specific trial could be invalid for more than one reason (i.e., an extremely slow and incorrect response). We removed task trials with incorrect (7.33%) or missing (4.85%) responses, as well as task trials with extremely fast ($RT < 100$, 0.28%) or slow RTs ($RT > 750$, 0.97%). Next, we removed the data from a total of 17 participants (14.29% out of 119) whose accuracy on task trials was below < 80% ($N = 15$), or because fewer than 80% of their task trials were valid in terms of either RT, accuracy or both ($N = 17$). In total, 21.26% of the task trials were thus removed, by filtering a participant's data in full or a participant's individual trials. Note that of the remaining data, we only selected trials that immediately followed a correct response trial.

Analysis

The results are shown in Figure 4. First, we looked at the action-effect on trial N-1 (*Prior*) on the response time of trial N, which showed significant facilitation [$F(1, 100) = 26.14$, $p = 0.001$, Partial Eta-Sq. = 0.21], replicating the response time facilitation found in previous studies (Eitam, Kennedy, et al., 2013; Hemed et al., 2020) and most importantly, the results obtained using the filtering of task trials immediately following an attention probe trial (Hemed et al., 2023). We also found that the effect of *Context* [F(3, 292) = 4.98, p = 0.002, Partial Eta-Sq. = 0.05] was significant, such that the number of predictable action-effect occurrences over recent trials was associated with a faster response time. Most notably, and again validating the corrected pattern described in Hemed et al (2023), the effect of *Context* was qualified by a *Prior* X *Context* interaction [F(3, 277) = 8.09, p = 0.001, Partial Eta-Sq. = 0.07], such that *Context* only affected response speed when the action-effect was given on the previous trial (*Prior*$_1$).

The effect of *Cycle Duration* was not significant [F(1, 100) = 0.07, p = 0.786, Partial Eta-Sq. < 0.01]. None of the other interactions was significant either: *Prior X Cycle Duration* [F(1, 100) = 2.03, p = 0.157, Partial Eta-Sq. = 0.02], *Context* X *Cycle-Duration* [F(3, 292) = 0.17, p = 0.912, Partial Eta-Sq. < 0.01], or their 3-way interaction [F(3, 277) = 1.78, p = 0.155, Partial Eta-Sq. = 0.02].

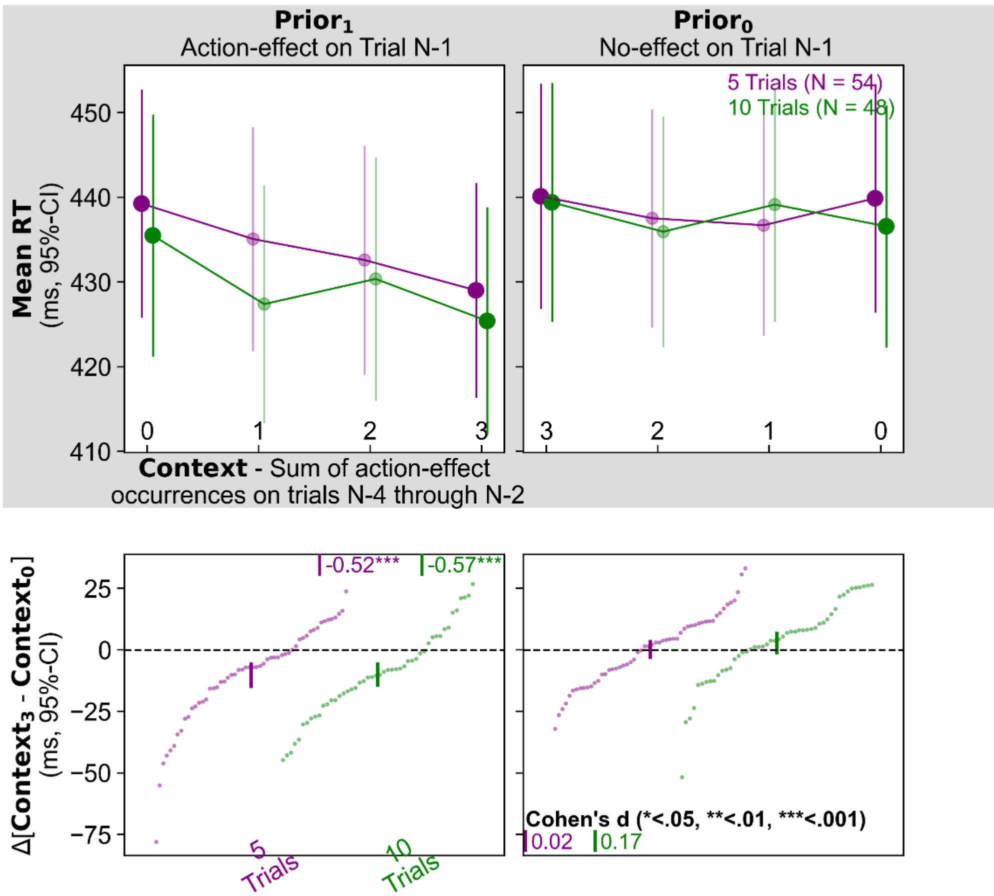

Figure 4: Experiment 1. Previous behavior-contingent occurrences (Context) facilitate response speed only if the immediately previous behavior (Prior) led to a fully predictable action-effect. The frequency at which fully predictable and contingent action-effects waxed or waned (Cycle Duration) did not influence these dynamics. Top panel – Mean response time by Prior (left vs. right panels), Context (X-axis) and Cycle Duration (Separate colors). Bottom panel – individual differences in response time (RT-Context3 minus RT-Context0), ordered by value. Negative values denote faster response speeds when behavior-contingent and fully predictable action-effects accumulated (Context3) vs. no such action-effects in recent trials (Context0). The vertical line denotes the 95% confidence interval centered on the group means. Digits indicate Cohen's-d values for the difference, and the significance of the difference (* < .05, ** <.01, *<.001).**

Revision of XGE-2023-1341 as invited by the action editor, Sarah Brown-Schmidt, Ph.D

To quantify the evidence for the pattern described above, four specific contrasts were selected because of their centrality to testing the extent to which the evaluation of effectiveness was sensitive to sudden changes (see also, Hemed et al., 2020; Hemed et al., 2023). Specifically, we tested how the poles of *Context* values influenced response time by examining the contrast of response time at [$Context_3$ – $Context_0$] independently when the trial followed an action-effect or did not follow one ($Prior_0$ and $Prior_1$). Consistent with Hemed et al. (2020) we did so separately for the 5- and 10-trial groups. For the Bayesian analysis we selected an uninformed ('default') prior (Cauchy $\chi 0 = 0$, $\gamma = 0.707$).

When responses led to fully predictable action-effects, a recent history of responses leading to these action-effects ($Context_3$) further speeded up responding compared to a response leading to a fully predictable sensation with a proximal history of responses not followed by such an effect ($Context_0$).

The influence of immediate history on the RT facilitation of a response that continued to deliver predictable effects was supported, with a statistically significant difference in response speed for both the 5-Trial Cycle Duration group and very strong Bayesian support [-10.24 MS (19.54); $t(53) = -3.82$, $p < 0.001$, Cohen's $d = -0.52$, (-0.80, -0.23), BF1:0 = 140.9214] and 10-trial groups [-10.09 MS (17.55); $t(47) = -3.94$, $p < 0.001$, Cohen's $d = -0.57$, (-0.87, -0.26), BF1:0 = 190.2249].

If the reinforcing properties of contingent and fully predictable action-effects had merely accumulated, a similar slope would be expected even when the most recent (prior) trial did not lead to this action-effect. The results of Hemed et al. (2020) were ambiguous as to whether this was indeed the case due to the pseudo-slowing down effect created by switch costs, yet when these costs were removed, Hemed et al., 2023, found no such slope (as well as no evidence for slowing down due to the violation of a precise sensorimotor prediction). As shown in Figure 4, there was no evidence of this type of slope in the current study. To quantify the evidence for or against its existence, we tested the influence of *Context* (i.e., local history) on the RT difference of responses that did not immediately follow a contingent and fully predictable action-effect.

Based on Hemed et al. (2023) we predicted that when a response failed to produce even a highly consistent (predictable) effect regardless of whether previous responses predictably led to action effects, (Context_3) or not (Context_0) would not matter. In other words, we expected that reinforcement from sensorimotor predictability would only accumulate as long as prediction was monotonously increasing.

As predicted, *Context* had no effect when the most recent response failed to lead to this effect, with moderate to strong Bayesian support, which was replicated in both Cycle Duration groups [5-long; 0.24 MS (14.31); $t(53) = 0.12$, $p = 0.903$, Cohen's $d = 0.02$, (-0.25, 0.28), $BF_{1:0}$ = 0.1495; and 10-long; 2.83 MS (16.24); $t(47) = 1.19$, $p = 0.238$, Cohen's $d = 0.17$, (-0.11, 0.46), $BF_{1:0}$ = 0.3059].

To summarize, the results obtained by Hemed et al. 2023 by removing those task trials that were the most affected by switch costs were replicated in full when attention probes were not used. The observed pattern was meaningfully different from the one reported by Hemed and colleagues in 2020, in that while the influence of the recent action-effects was indeed fully contingent on having that response continue to lead to the same effect, we can now confidently conclude that the history of sensorimotor reinforcement does not accumulate or otherwise modulate response times when the prediction is violated. Specifically, as long as predictable and contingent action-effects keep coming, the faster the responding (up to N-4), but even in a single instance of their absence, the history of consistent action-effects stops mattering.

Discussion

Viewed in light of the two goals stated above, Experiment 1 both corroborated the method Hemed et al. (2023) used to remove the influence of the switch costs induced by the probes as well as the pattern of results that emerged when these costs were removed. These results can be interpreted to show that the reinforcement by own-action effects (i.e., fully predictable and response contingent) depends crucially on whether the most recent behavior was effective. Since we examine these results more fully in the General Discussion, we simply stress here that this pattern is different than what would be expected for reinforcement from rewards, whose influence would indeed rapidly decay (exponentially; but not on the first omission of the reward; cf. Parker et al., 2016).

To fully meet both goals as well as more firmly tie the pattern of results to the predictability of the action-effects, we designed and ran Experiment 2, which was identical to Experiment 2 in Hemed et al. (2020) but without the secondary task (i.e., responding to the attention probes). Further, in Experiment 2, rather than participants' responses leading to no action-effect as was the case in Experiment 1, responses either led to fully or partially predictable action-effects.

Experiment 2

Experiment 2 was conducted as a test of the replicability of the pattern of results obtained in Experiment 1, as well as to rebut a possible alternative explanation for the results of that experiment. Experiment 2 was identical to Experiment 1, apart from the fact that the participants' responses always led to an action-effect. To do so, we replaced the No action-effect trials (i.e., No-effects cycles) with Partially predictable action-effects. The latter was created by subtly (and randomly) perturbing the location in which the contingent effect would appear (i.e., where own-action effects appeared at a random location relative to the imperative cue), thus making them partially unpredictable (for previous uses of this manipulation with the current task see (Karsh et al., 2016).

Methods

Participants

We recruited 282 participants. The participants' gender demographics were collected using a radio-button style question included in the questionnaire, under the prompt 'I'm a –' and chose from a dropdown list of *Female*, *Male*, *Other* and *Rather not say*. In the 5-trial group, there were 144 participants, aged 18 to 45 (M = 26.44, SD = 6.10). Of these, 97 responded Female, 46 responded Male, and 1 did not respond to the question. In the 10-trial group, there were 138 participants, aged 18 to 45 (M = 25.88, SD = 5.51). Of these, 89 responded Female, 45 responded Male, 2 responded Rather not say, and 2 did not respond to the question. No information on race, religion, ethnicity was collected.

Stimuli and design

On the fully predictable-effect cycles, correct responses caused the imperative cue to turn white for 100MS (similarly to Experiment 1). In the partially (un)predictable-effect (Spatial-Perturbation) Cycles, the white disc appeared immediately, but its center was offset by a random distance and angle relative to the center of the imperative cue, as shown in Figure 3C. The distance was 1.75-2.25 times the cue diameter, and the angles were 0-360°. Distance and angle were selected randomly on each trial from a uniform distribution. Otherwise, Experiment 2 was identical to Experiment 1.

Results

Data preparation and screening

Data from the training block were discarded prior to pre-processing. Note that data from a participant or a specific trial could be invalid for more than one reason. We removed task trials with incorrect (8.18%) or missing (2.35%) responses, as well as task trials with extremely fast (RT < 100, 0.18%) or slow RTs (RT > 750, 0.65%). Next, we removed the data from a total of 29 participants (10.28% out of 282) whose accuracy on task trials was below < 80% (N = 27), or where fewer than 80% of the trials were valid in terms of either RT, accuracy or both (N = 29). In total, 17.57% of the task trials were removed, by filtering whole participants' data or individual trials.

Analysis

The data and analysis are shown in Figure 5. As in Experiment 1, again validating the corrective method employed by Hemed et al. (2023), we found that the Spatially Predictable action-effect on trial N-1 (*Prior*) decreased the response time [F(1, 251) = 30.70, p = 0.001, Partial Eta-Sq. = 0.11], as did a recent history of contingent and fully predictable action-effects (*Context*) [F(3, 741) = 4.76, p = 0.003, Partial Eta-Sq. = 0.02]. Unlike Experiment 1 and Hemed et al. (2020), this facilitation was not qualified by a *Prior* X *Context* interaction [F(3, 741) = 1.27, p = 0.285, Partial Eta-Sq. = 0.01]. No other significant effects were found: Cycle Duration [F(1, 251) = 0.53, p = 0.467, Partial Eta-Sq. < 0.01], Prior X Cycle-Duration [F(1, 251) = 0.09, p = 0.762, Partial Eta-Sq. < 0.01], Context X Cycle-Duration [F(3, 741) = 0.76, p = 0.513, Partial Eta-Sq. < 0.01], or a 3-way interaction [F(3, 741) = 0.61, p = 0.603, Partial Eta-Sq. < 0.01].

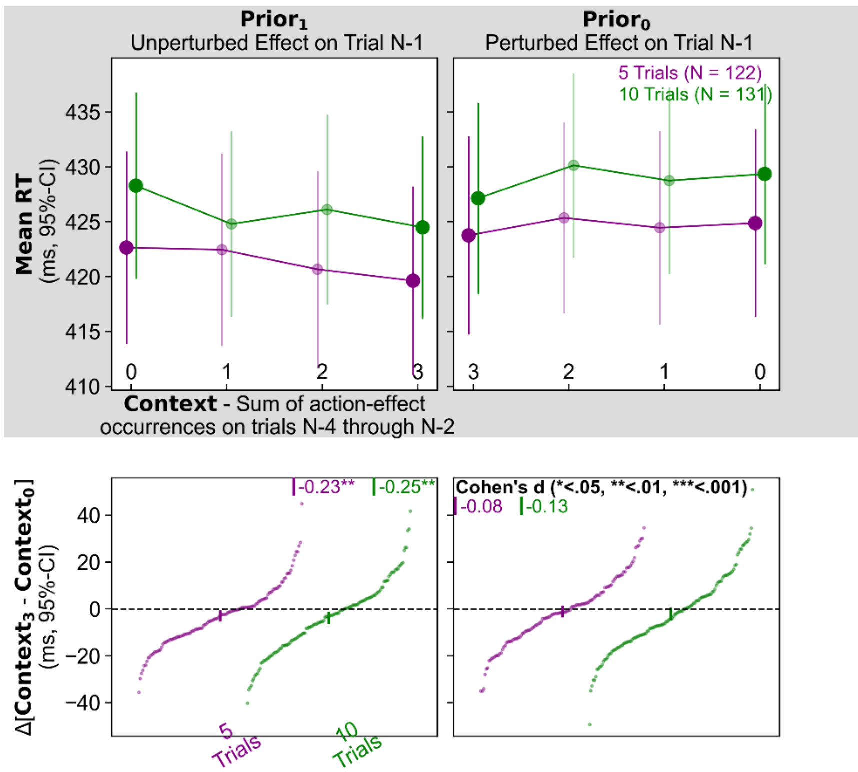

Figure 5: Experiment 2. A proximal history of actions leading to fully predictable effects (*Context*) facilitated response speed, but only if responses continued to have fully predictable effects on the most recent trial (*Prior*). Changes in the frequency of fully/partially predictable action-effects (*Cycle-Duration*) did not affect response speed. <u>Top panel</u> – Mean response time by Prior (left vs. right panels), Context (X-axis) and Cycle Duration (Separate colors). <u>Bottom panel</u> – Differences in response time as a function of proximal history (RT-Context$_3$ minus RT-Context$_0$). Negative values led to faster responses when predictable action-effect occurrences accumulated (Context$_3$) compared to the accumulation of only partially predictable ones (Context$_0$). The vertical line denotes the 95% confidence interval centered on the group means. Scatter indicates individual means for the differences in response time. Digits and symbols are Cohen's-d values for the difference, and significance levels (* < .05, ** <.01, *<.001).**

As in Experiment 1, we explored the pattern described above using four specific (pre-registered) contrasts; namely, by examining the contrast of response time at [Context3 – Context0] independently when the trial was preceded by a response which led to a fully predictable action-effect or only led to a partially predictable one ($Prior_1$ and $Prior_0$, respectively). This was done separately for the 5- and 10-Trial conditions. As in the previous experiment, for the Bayesian analysis we used an uninformed (‘default’) prior (Cauchy $\chi0 = 0$, $\gamma = 0.707$).

Our predictions were exactly the same as in Experiment 1 (except for replacing the lack of contingent action-effect with a partially predictable one). We again found that responses following a sequence of responses which led to a fully predictable action-effect (Context3) were faster than when following a sequence of responses leading to a partially predictable action-effect ($Context_0$), as long as the immediately previous response also led to a fully predictable action-effect ($Prior_1$). This was replicated for both the 5-trial cycle group [-3.02 MS (13.02); $t(121) = -2.55$, $p = 0.006$, Cohen's $d = -0.23$, (-0.41, -0.05), BF1:0 = 4.4247], and for the 10-trial cycle group [-3.80 MS (15.20); $t(130) = -2.85$, $p = 0.003$, Cohen's $d = -0.25$, (-0.42, -0.07), BF1:0 = 9.1835], both with conclusive support for a difference between conditions.

As predicted, and again validating the method used by Hemed and colleagues (2023) for removing the maximal cost of switching back from attention probes to task trials, no such influence of context ($Context_3$ – $Context_0$) was found when the immediately previous response only led to a partially predictable effect ($Prior_0$) with conclusive support for the lack of a difference on the 5-Trial Cycle group [-1.12 MS (14.36); $t(121) = -0.86$, $p = 0.393$, Cohen's $d = -0.08$, (-0.26, 0.10), BF1:0 = 0.1439] and the 10 trial Cycle group [-2.22 MS (16.47); $t(130) = -1.53$, $p = 0.128$, Cohen's $d = -0.13$, (-0.31, 0.04), BF1:0 = 0.3041].

<u>Discussion</u>

Experiment 2 was conducted to complete the validation of the method used by Hemed et al. (2023) as well as to strengthen the conclusion that RSP or sensory predictability is the key factor in facilitating response times (i.e., Reinforcement). It provided strong support for our hypothesis that the increasing predictability of response-contingent action-effects is proportional to the increase in the speed of responding and that this influence of sensory predictability is itself fully contingent on the full predictability of the most recent contingent action-effect.

General Discussion

First and foremost, the current study is an example of how the best of intentions to achieve scientific rigor can wreak havoc on the results of one's experiments. Attempting to systematically filter out participants (as well as demotivate them from explicitly uncovering the simple dynamics in which the response contingent action-effects predictably waxed and waned), we unwittingly introduced switch costs. This robust and hidden effect dramatically influenced the results of our published study (Hemed et al., 2020). Re-running the two experiments in that study without the attention probes showed that our post-hoc corrective method (Hemed et al., 2023) that consists of the removal of the first task trial post attention probe by and large removed the influence of switch costs, as was also evident from the meta-analysis (see Supplementary Materials).

Our results should prompt researchers whose experiment involved a secondary task (e.g., to probe participants' attention or task engagement) and whose measure of interest was the response time to the primary task, to reanalyze their results in the way we suggested in Hemed et al., 2023. The difference from the original results may be minimal, especially if the effect of interest is much larger than the cost of switching back to the main task, but it may also be large, especially when the effect of interest is a few times smaller than this cost, as in our case (see Figure 1). It is hard to estimate how many published studies are affected by a similar issue since researchers may use different terms for these secondary tasks or not even report them, but we suspect that this is not an uncommon practice, especially given the increase in online behavioral experiments. At the very least, we have highlighted this risk, so that future researchers will not make the mistake we committed.

Sensorimotor predictability, its violation, and reinforcement

Now that we have firmly established the validity of the pattern of results that emerged after remedying the findings reported in Hemed et al. (2023) we can move on to discuss its theoretical significance.

The main finding in the current experiment is that the recent history of contingency (i.e., trials N-4 through N-2; i.e., *Context*) does *not* modulate RSP once the most recent response does not lead to an action effect (or to a fully predictable one). Previously (Hemed et al., 2020), we reached a similar conclusion but only for the relatively unpredictable environment (the 5-trial group) which we qualified as such, but the current study establishes that this is case regardless of the environment’s predictability.

Revision of XGE-2023-1341 as invited by the action editor, Sarah Brown-Schmidt, Ph.D

It is surprising that the strength of expectation of an effect (i.e. its response-contingency; Experiment 1), or the abrupt shift from fully predictable effects (within the time window of interest) to only partially predictable ones (Experiment 2) fails to generate a measurable influence on the reinforcement of actions previously used to control the environment, especially given the substantial evidence that a reduction in control (and 'surprise' in general) uniquely attracts attention (e.g., Wen & Haggard, 2018).

One possible explanation for this apparent discrepancy is that the allocation of attention may reflect the working of a less modular (or more general purpose) process which is highly sensitive to the task-instructed relevance of 'control' (Eitam, Yeshurun, et al., 2013; Eitam & Higgins, 2010, 2016; Hemed, Karsh, et al., 2022; Hemed, Mark-Tavger, et al., 2022; Hemed & Eitam, 2022). Yet even if a modular or specialized process is involved, one could potentially expect a phenomenon akin to unintentional post-error slowing when strong (action-effect) expectations or predictions are violated (e.g., Logan & Crump, 2010); namely, 'post prediction-error slowing', which is how we interpreted the (now refuted) finding in Hemed et al. (2020) for the 10-trial group.

The lack of any behavioral evidence for the mental-registration that such a violation occurred leads us to adopt the working hypothesis that when value-neutral ('rewardless') sensorimotor predictions are involved, there is no *executive* monitoring of their violations, which presumably leads to the observed slowing down in the post-error case. This speculation raises the interesting question of whether such post-prediction error slowing can be found in less modular (but still, specialized or dedicated) processes such as those that produce reward prediction errors.

The above null result is also consistent with the positive result (replicated here) that the effect of the most recent response is very heavily weighted. That is, the influence of 'historical' predictable response-contingent action-effects (*Context*) on response times is fully dependent on whether the most recent behavior led to a fully predictable action-effect (*Prior*). In a sense, in the case of a failed (sensorimotor) prediction – history is 'washed out'.

A recent study (Ren, Gentsch, et al., 2023) also found that the speeding up of responses based on value neutral (reward-less) effects depended on the outcome of the most recent trial. However, in that study, the local (recent-history; our Context variable) stability of the environment was not manipulated. Instead, the researchers manipulated the overall contingency of response-effects per block (which was 48 trials long). Interestingly, this overall probability of action-effects did not influence response speed - only the most recent history did - suggesting a short-term memory-component of response-effects (or alternatively, a high learning rate).

In Ren and colleagues (2023), the changes in response speed were accompanied by changes in response frequency, which we previously found to reflect mainly high-level cognitions (i.e., explicit, general purpose causal-judgments of action-effects; Hemed et al., 2022). This somewhat complicates the comparison between these studies as it suggests that a different ('explicit' or 'deliberate') process may have also played a role in their study.

More generally, the vast majority of previous works looking at abrupt changes in the effects of an action on the environment have been conducted on non-human animals and has focused by and large on reward prediction error as well as its influence on future action-selection (Howard et al., 2017; Morris et al., 2006). These studies suggest that the (reward-learning) experience in more recent trials has exponentially greater influence on reward prediction (error) for the current action (Glimcher, 2011; Parker et al., 2016).

We found an even stronger influence of the most recent trial, in that the washout of the reinforcing effects of sensory effects was practically instantaneous (i.e., it was modulated almost exclusively by whether the most recent action led to a predictable effect or not). While it could be the case that the difference in the weight assigned to the outcome of the most recent action stemmed from differences in population, method and measures, a more plausible explanation is that that reinforcement from sensorimotor predictability may involve a sub-system of reinforcement that is computationally different from the one that produces the more familiar reward prediction error, as found in the animal studies mentioned above (i.e., reinforcement from reward). This result could prompt substantial changes to our current theory of RSP, which still makes allusions to the reward from controlling the environment (e.g., Rovee & Rovee, 1969; Skinner, 1965; White, 1959). More generally, this opens the door to broadening the conventional understanding of the brain's reward system to include reinforcers which are not rewards, or a reinforcement system and furthers the effort to 'open the black box' of motivation (Custers et al., 2025).

A role for precision? Not here

In Hemed and colleagues (2020) the manipulation of the cycle length was aimed at testing whether stronger (or more precise) predictions would modulate the influence of an immediately prior action-effect. In the current study, there were no quantitative or qualitative differences in the pattern of the groups that differed in their Cycle Duration (i.e., 5-trials vs. 10-trials).

To further explore the potential difference between environments, we conducted a meta-analysis in which we analyzed the follow-up contrasts (i.e., $Context_3$ VS. $Context_0$; see Figures S1 and S2 in Supplementary Materials). The aggregated results also fail to support any differences between the 5 and 10-trial groups, in either Experiment 1 or 2 (which did not include an attention probe). This again confirms the lack of a difference between the results of groups for which cycles alternated on a different number of trials, as in the corrected results by Hemed et al. (2020); (Hemed et al., 2023)[2]. It seems safe to conclude that the slowing down due to the violation of the prediction (i.e., $Context_3$ – $Context_0$, given $Prior_0$) which we previously reported for the 10-trial group resulted in full from the task-switching between the attention probes and the main task.

Still, it is surprising that we found no influence of the frequency of changes in the environment (i.e., Cycle-Duration) on RSP. From a Bayesian perspective (Ernst & Banks, 2002; Friston, 2011; Yon & Frith, 2021) and given that the environment was less predictable for the 5-trial group, these participants should have assigned lower weights to their predictions as to the effects of the response they were currently executing (i.e., prior belief). This would mean

[2] The same is true for experiments that did include attention probes when analyzed without the switch costs of the attention probes in Experiments 3-5 here (see supplementary materials), as well as in Hemed and Eitam (2022).

attributing higher credibility to recently obtained information, such as the reafferent associated with the just-executed response (i.e., the posterior belief). This can also be construed as a larger forgetting rate for less recent events, and higher learning rate (from novel ones) and should have resulted in a stronger effect for the most recent trial (i.e., *Prior*, in our analyses), for the group experiencing more frequent changes (i.e., the 5-trial cycle group). However, we found no support for this supposition (i.e., for the interaction between the effect of the most recent trial; i.e., the *Prior*, and the frequency of changes; i.e., *Cycle-Duration*).

An alternative way of thinking about this (suggested by an anonymous reviewer of this paper) is that the 5-trial condition better reflects the rate of change that the relevant components of the motor system are optimized for and hence, is further evidence that RSP is indeed related to that system. Studies have shown that agents in noisy environments place higher weights on incoming information than prior beliefs in order to adapt to frequent changes in the environment. Thus, in the 5-trial cycle environment, the degree of noise is such that the *balance* between priors and likelihood maximizes a sensorimotor 'match' to the signal that drives this unique form of reinforcement (Lawson et al., 2021; Yon, 2021). Apparently, agents may even actively increase the variance of their actions when kinematic predictions are violated once or twice in succession (Wiegel et al., 2022), possibly for purposes of exploring the environment.

Nevertheless, there may be some support for the original precision hypothesis in the fact that the response speed in the 10-trial cycle group was substantially slower on the post-probe trials (see Introduction). This is clear from the difference between the current analysis (which excluded post-probe trials) and the previous one (which did not; cf. Hemed et al., 2020; see middle vs.

bottom panels in the Metanalysis figures in the Supplementary Materials). This comparatively larger task switch cost could have been reflective of disruption at the level of action-selection which operates on more abstract representations (compared to the facilitation that occurs in our working theoretical model through facilitation of response execution which is the product of reinforcement of lower level and more detailed motor programs; Hemed et al., 2022). We further speculate that these costs could reflect the need to adjust strong priors resulting to the less variable environment compared to that encountered by the 5-trial group. This possibility is interesting in light of the argument that "stubborn" processes, which limit the integration of incoming information, are adaptive in certain contexts (Dallmann, 2017). Unintentionally, our previous and erroneous analysis may have demonstrated how differences in task-switch costs may serve as a new avenue for testing the effect of sensorimotor predictions on different levels of representation of behavior.

Limitations and Constraints on Generality

One major limitation in the current report is the fact that we used two different manipulations throughout this work to manipulate control over the environment. One involved removing the action-effects altogether (Experiment 1) while in the other, the action-effects simply became spatially unpredictable (Experiment 2). De facto, both led to the same behavioral result qualitatively. However, in the current paper, the effect sizes for RSP (i.e., $\text{Context}_3 - \text{Context}_0$, given Prior_1; see Figure S1 in the supplementary materials) were somewhat weaker when the spatial-perturbation manipulation was used (e.g., Experiment 1 vs. Experiment 2). Note though that since RSP is assumed to rely on a comparator-like model, the two manipulations *should* lead to similar results as neither lack of action-effect or unpredictable action effect allows for credit assignment (i.e., that a specific action caused a predictable action-effect).

Another limitation of the current study is that we did not include attention probes. Given that there were no attention probes to deter participants from counting up to the change to the next cycle, in theory participants could have figured out when they would stop receiving predictable action-effects and thus possibly tamper with the measurement of RSP. This was more likely in the 5-trial group than in the 10-trial group, since there were more frequent alternations. However, the results of the debrief questionnaire confirmed that this was not a credible threat[3]. Thus, it is extremely unlikely that our results were due to the detection of the timing of the change inaction-effects. Further, the results of Experiments 1 and 2 were entirely in line with the results of Experiments 3-5 (which did include attention probes; see middle panel of meta-analysis in the supplementary materials section).

Finally, caution should be exercised by attempting to generalize our findings to other contexts. This is because we used the millisecond timing of keypresses as a proxy for execution (and reinforcement), without considering other aspects of the motor action such as duration, force applied, etc. Further, although there are good reasons to believe that this evaluation of behavioral

[3] We debriefed the participants after the experiments. The questionnaire specifically asked whether a specific event occurred on every fixed number of trials. In Experiment 1 only 8 out of the 127 (~6%) participants (6 in the 5-trial group, 2 in the 10-trials group) reported the exact duration of their respective cycles controlling the feedback they received. In Experiment 2, only a single participant in the 5-trial group (out of 282 participants) did so.

effects on the environment was computed using only the most recent actions, the undulating design we used might have artificially contributed to the patterns observed in our data (Hemed et al., 2017; Parker et al., 2016; Ren, Gentsch, et al., 2023).

Coda

We were motivated to find a plausible answer to the question of how reinforcement of behavior changes when control over the environment waxes and wanes. We attempted to answer this same question in a previous publication (Hemed et al., 2020), and evidently only partially succeeded. Here, we provide a more accurate picture where, and as we previously found, behaviors are reinforced when their control over the environment increases, but this reinforcement fully depends on the results of the most recent behavior. Once control over the environment is lost, even briefly, this facilitation is all but gone, independently of how frequently control increases or decreases. In the process, we identified a confound that may mislead other researchers and provide a validated method to overcome it even after the data have been collected.

On a meta-scientific level, we are thankful for the opportunity to further amend the record and the scientific literature more generally. To a large extent, detecting this confound led us to write the current paper. Our experience with erring and its correction has been startling and humbling but also very rewarding, since we experienced self-correcting science in action. We hope that our ultimately positive outcome will motivate researchers to scrutinize their findings and come forward when finding anything amiss. The normalization of self-correction will promote transparency and hopefully more accurate and efficient scientific progress.

References

Agay, N., Yechiam, E., Carmel, Z., & Levkovitz, Y. (2014). Methylphenidate enhances cognitive performance in adults with poor baseline capacities regardless of attention-deficit/hyperactivity disorder diagnosis. *Journal of Clinical Psychopharmacology*, *34*(2). https://doi.org/10.1097/JCP.0000000000000076

Aust, F., Diedenhofen, B., Ullrich, S., & Musch, J. (2013). Seriousness checks are useful to improve data validity in online research. *Behavior Research Methods*, *45*(2). https://doi.org/10.3758/s13428-012-0265-2

Bakbani-Elkayam, S., Dolev-Amit, T., Hemed, E., Zilcha-Mano, S., & Eitam, B. (2024). Intact modulation of response vigor in major depressive disorder. *Motivation and Emotion*, 1–13.

Barbosa, J., Stein, H., Zorowitz, S., Niv, Y., Summerfield, C., Soto-Faraco, S., & Hyafil, A. (2023). A practical guide for studying human behavior in the lab. *Behavior Research Methods*, *55*(1), 58–76.

Berger, A., & Kiefer, M. (2021). Comparison of Different Response Time Outlier Exclusion Methods: A Simulation Study. *Frontiers in Psychology*, *12*. https://doi.org/10.3389/fpsyg.2021.675558

Blakemore, S. J., Wolpert, D. M., & Frith, C. D. (1998). Central cancellation of self-produced tickle sensation. *Nature Neuroscience*. https://doi.org/10.1038/2870

Custers, R., Eitam, B., & Higgins, E. T. (2025). The unboxing has already begun: One motivation construct at a time. *Behavioral and Brain Sciences*, *48*, e29.

Dallmann, J. (2017). *When Obstinacy is a Better (Cognitive) Policy*. Ann Arbor, MI: Michigan Publishing, University of Michigan Library.

Eitam, B., & Higgins, E. T. (2010). Motivation in mental accessibility: Relevance of a representation (ROAR) as a new framework. *Social and Personality Psychology Compass*, *4*(10), 951–967.

Eitam, B., & Higgins, E. T. (2016). From reaction ('priming') to motivated selection: Changing conceptualizations of accessibility. *Current Opinion in Psychology*, *12*, 58–62.

Eitam, B., Kennedy, P. M., & Higgins, E. T. (2013). Motivation from control. *Experimental Brain Research*. https://doi.org/10.1007/s00221-012-3370-7

Eitam, B., Yeshurun, Y., & Hassan, K. (2013). Blinded by irrelevance: Pure irrelevance induced "blindness". *Journal of Experimental Psychology: Human Perception and Performance*, *39*(3), 611.

Ernst, M. O., & Banks, M. S. (2002). Humans integrate visual and haptic information in a statistically optimal fashion. *Nature*. https://doi.org/10.1038/415429a

Friston, K. (2011). What is optimal about motor control? *Neuron*, *72*(3), 488–498.

Glimcher, P. W. (2011). Understanding dopamine and reinforcement learning: The dopamine reward prediction error hypothesis. *Proceedings of the National Academy of Sciences*, *108*(Supplement 3), 15647–15654.

Hazeltine, E. (2024). What are we measuring when we measure task switch costs? In *Current Opinion in Behavioral Sciences* (Vol. 56). https://doi.org/10.1016/j.cobeha.2024.101352

Hemed, E. (2022). *robusta: Statistical hypothesis testing in Python, using R* [Computer software]. https://github.com/EitanHemed/robusta

Hemed, E., Bakbani-Elkayam, S., Teodorescu, A. R., Yona, L., & Eitam, B. (2020). Evaluation of an action's effectiveness by the motor system in a dynamic environment. *Journal of Experimental Psychology: General*, *149*(5), 935–948. https://doi.org/10.1037/xge0000692

Hemed, E., & Eitam, B. (2022). *Control feedback increases response speed independently of the feedback's goal- and task-relevance*. https://github.com/EitanHemed/patches-papers

Hemed, E., Elkayam, S. B., Teodorescu, A. R., Yona, L., & Eitam, B. (2023). Clarification to Hemed et al. (2020). *Journal of Experimental Psychology: General*, *152*(3). https://doi.org/10.1037/xge0001394

Hemed, E., Karsh, N., Mark-Tavger, I., & Eitam, B. (2022). Motivation (s) from control: Response-effect contingency and confirmation of sensorimotor predictions reinforce different levels of selection. *Experimental Brain Research*, 1–27.

Hemed, E., Mark-Tavger, I., Hertz, U., Bakbani-Elkayam, S., & Eitam, B. (2022). Automatically controlled: Task irrelevance fully cancels otherwise automatic imitation. *Journal of Experimental Psychology: General*, *151*(5), 996.

Howard, C. D., Li, H., Geddes, C. E., & Jin, X. (2017). Dynamic nigrostriatal dopamine biases action selection. *Neuron*, *93*(6), 1436–1450.

Jenkins, M., & Obhi, S. S. (2022). Mistakes strengthen the temporal binding effect in the context of goal-directed actions. *Experimental Brain Research*, *240*(7), 2191–2203.

Karsh, N., Ahmad, Z., Erez, F., & Hadad, B. S. (2023). An effect that counts: Temporally contiguous action effect enhances motor performance. *Psychonomic Bulletin and Review*. https://doi.org/10.3758/s13423-023-02387-y

Karsh, N., & Eitam, B. (2015). I control therefore I do: Judgments of agency influence action selection. *Cognition*. https://doi.org/10.1016/j.cognition.2015.02.002

Karsh, N., Eitam, B., Mark, I., & Higgins, E. T. (2016). Bootstrapping agency: How control-relevant information affects motivation. *Journal of Experimental Psychology: General*, *145*(10), 1333–1350. https://doi.org/10.1037/xge0000212

Karsh, N., Haklay, I., Raijman, N., & Custers, A. L. andRuud. (2021). *Control Alters Risk-Taking: The Motivating Impact of Action-Effectiveness in Different Risk Contexts*.

Karsh, N., Hemed, E., Nafcha, O., Elkayam, S. B., Custers, R., & Eitam, B. (2020). The Differential Impact of a Response's Effectiveness and its Monetary Value on Response-Selection. *Scientific Reports*, *10*(1), 1–12. https://doi.org/10.1038/s41598-020-60385-9

Kiesel, A., Steinhauser, M., Wendt, M., Falkenstein, M., Jost, K., Philipp, A. M., & Koch, I. (2010). Control and interference in task switching—A review. *Psychological Bulletin*, *136*(5), 849.

Kung, F. Y. H., Kwok, N., & Brown, D. J. (2018). Are Attention Check Questions a Threat to Scale Validity? *Applied Psychology, 67*(2). https://doi.org/10.1111/apps.12108

Lawson, R. P., Bisby, J., Nord, C. L., Burgess, N., & Rees, G. (2021). The Computational, Pharmacological, and Physiological Determinants of Sensory Learning under Uncertainty. *Current Biology*, *31*(1). https://doi.org/10.1016/j.cub.2020.10.043

Logan, G. D., & Crump, M. J. C. (2010). Cognitive illusions of authorship reveal hierarchical error detection in skilled typists. *Science*, *330*(6004), 683–686.

Meade, A. W., & Craig, S. B. (2012). Identifying careless responses in survey data. *Psychological Methods*, *17*(3). https://doi.org/10.1037/a0028085

Morris, G., Nevet, A., Arkadir, D., Vaadia, E., & Bergman, H. (2006). Midbrain dopamine neurons encode decisions for future action. *Nature Neuroscience*, *9*(8), 1057.

Oppenheimer, D. M., Meyvis, T., & Davidenko, N. (2009). Instructional manipulation checks: Detecting satisficing to increase statistical power. *Journal of Experimental Social Psychology*, *45*(4). https://doi.org/10.1016/j.jesp.2009.03.009

Parker, N. F., Cameron, C. M., Taliaferro, J. P., Lee, J., Choi, J. Y., Davidson, T. J., Daw, N. D., & Witten, I. B. (2016). Reward and choice encoding in terminals of midbrain dopamine neurons depends on striatal target. *Nature Neuroscience*, *19*(6), 845.

Peirce, J., Gray, J. R., Simpson, S., MacAskill, M., Höchenberger, R., Sogo, H., Kastman, E., & Lindeløv, J. K. (2019). PsychoPy2: Experiments in behavior made easy. *Behavior Research Methods*, 1–9.

Penton, T., Wang, X., Coll, M.-P., Catmur, C., & Bird, G. (2018). The influence of action–outcome contingency on motivation from control. *Experimental Brain Research*, 1–11.

Ren, Q., Gentsch, A., Kaiser, J., & Schütz-Bosbach, S. (2023). Ready to go: Higher sense of agency enhances action readiness and reduces response inhibition. *Cognition*, *237*. https://doi.org/10.1016/j.cognition.2023.105456

Ren, Q., Kaiser, J., Gentsch, A., & Schütz-Bosbach, S. (2023). Prepared to stop: How sense of agency in a preceding trial modulates inhibitory control in the current trial. *Cerebral Cortex*, *33*(13). https://doi.org/10.1093/cercor/bhad141

Rovee, C. K., & Rovee, D. T. (1969). Conjugate reinforcement of infant exploratory behavior. *Journal of Experimental Child Psychology*, *8*(1), 33–39.

Sauter, M., Draschkow, D., & Mack, W. (2020). Building, hosting and recruiting: A brief introduction to running behavioral experiments online. *Brain Sciences*, *10*(4), 251.

Seubert, O., van der Wel, R., Reis, M., Pfister, R., & Schwarz, K. A. (2024). The one exception: The impact of statistical regularities on explicit sense of agency. *Journal of Experimental Psychology: Human Perception and Performance*.

Siritzky, E. M., Cox, P. H., Nadler, S. M., Grady, J. N., Kravitz, D. J., & Mitroff, S. R. (2023). Standard experimental paradigm designs and data exclusion practices in cognitive psychology can inadvertently introduce systematic “shadow” biases in participant samples. *Cognitive Research: Principles and Implications*, *8*(1), 66.

Skinner, B. F. (1965). *Science and human behavior* (Issue 92904). Simon and Schuster.

Tanaka, T., Watanabe, K., & Tanaka, K. (2021). Immediate action effects motivate actions based on the stimulus–response relationship. *Experimental Brain Research*, *239*(1), 67–78.

Tapal, A., Oren, E., Dar, R., & Eitam, B. (2017). The Sense of Agency Scale: A Measure of Consciously Perceived Control over One's Mind, Body, and the Immediate Environment. *Frontiers in Psychology*, *8*, 1552. https://doi.org/10.3389/fpsyg.2017.01552

Villa, R., Tidoni, E., Porciello, G., & Aglioti, S. M. (2021). Freedom to act enhances the sense of agency, while movement and goal-related prediction errors reduce it. *Psychological Research*, *85*(3), 987–1004.

Waskom, M. L. (2021). Seaborn: Statistical data visualization. *Journal of Open Source Software*, *6*(60), 3021.

Wen, W., & Haggard, P. (2018). Control changes the way we look at the world. *Journal of Cognitive Neuroscience*, *30*(4), 603–619.

White, R. W. (1959). Motivation reconsidered: The concept of competence. *Psychological Review*, *66*(5), 297.

Wiegel, P., Spedden, M. E., Ramsenthaler, C., Beck, M. M., & Lundbye-Jensen, J. (2022). Trial-to-trial variability and cortical processing depend on recent outcomes during human reinforcement motor learning. *Neuroscience*, *501*, 85–102.

Wolpert, D. M., & Miall, R. C. (1996). Forward Models for Physiological Motor Control. *Neural Networks*, *9*(8), 1265–1279.

Wylie, G., & Allport, A. (2000). Task switching and the measurement of "switch costs." *Psychological Research*, *63*, 212–233.

Yon, D. (2021). Prediction and Learning: Understanding Uncertainty. *Current Biology*, *31*(1). https://doi.org/10.1016/j.cub.2020.10.052

Yon, D., & Frith, C. D. (2021). Precision and the Bayesian brain. *Current Biology*, *31*(17), R1026–R1032.