Control feedback increases response speed independently of the feedback's goal and task relevance

Eitan Hemed[1, ^] & Baruch Eitam[1]

[1] Department of Psychology, School of Psychological Science, University of Haifa, Israel

[^] Corresponding author (Eitan.Hemed@gmail.com)

For code and data see https://github.com/EitanHemed/patches-papers

THIS IS A PRE-PRINT AND HAS NOT BEEN PEER-REVIEWED. IT IS VERY LIKELY THAT MINOR DETAILS WILL BE CHANGED IN FUTURE VERSIONS. SPECIFICALLY, NOT ALL STATISTICAL TESTS ON EXPERIMENTS 1 AND 3 PROVIDED CONCLUSIVE RESULTS AND ADDITIONAL DATA COLLECTION IS UNDERGOING.

Abstract

Humans live and function in dynamic environments, in which the same motor response is effective at manipulating the environment at specific times and ineffective at others. We have shown that feedback indicating control over the environment (i.e., action-effects) reinforces responses' execution. We have further shown that this type of reinforcement is highly responsive to changes in the environment. Here, we capitalize on our previous findings to gain insight into the relationship between control-based, value-free reinforcement and the agent's goals. Over a series of experiments, we increased or decreased the goal or task relevance of the control-relevant feedback our participants' received. Control-relevant feedback reinforcement occurred orthogonally to the goal-relevance of the feedback, strengthening a recent suggestion that the source of this reinforcement is a modular mechanism that depends only on confirmation of sensorimotor predictability.

Introduction

Over the last decade, value-free, neutral feedback has been shown to reinforce the preparation, selection or execution of actions (Bakbani-Elkayam et al., 2019; Eitam et al., 2013; Hemed et al., 2020, 2022; Karsh & Eitam, 2015; Karsh et al., 2016, 2020; Penton et al., 2018; Tanaka et al., 2021). Recently (Hemed, Karsh, et al., 2022), we have shown that response speed and response frequency are reinforced differently by this type of feedback. Furthermore, we hypothesized that if the above facilitation of response speed is driven by sensorimotor predictability it should be sensitive only to the conditional probability of what occurs following a motor response (and not for example to the contingency between actions and effects which also considers what occurs when no response is made). The results confirmed this hypothesis, strengthening the hypothesis we named reinforcement from sensorimotor predictability (RSP).

Here we further probe the RSP explanation for the facilitation of response speed by asking whether and to what degree is it is sensitive to the goal or task relevance of the feedback. If response time facilitation is found to be sensitive to task-relevance of the feedback then the RSP explanation should be amended or deserted. At the minimum, the mechanism it provides (i.e. the comparator model or the modular operation of sensorimotor processes) should be supplied with additional (and non- or less modular) machinery such as task-sensitive filters (e.g., intentional weighting; Memelink & Hommel, 2013).

Before presenting the current study, we briefly review relevant experiments which seems to suggest that reinforcement of response speed stems mainly from the work of sensorimotor processes.

First, when feedback is subtly spatially-perturbed or lagged compared with immediate, spatially predictable-feedback participants do not show any facilitation of response speed, yet they report having similar levels of “feeling of control” over the perturbed feedback and as well as similar levels of self-reported aspects of goal attainment (Eitam et al., 2013; Hemed et al., 2020; Karsh et al., 2016; Tanaka et al., 2021).

Second, as mentioned above, we have recently shown (Hemed, Karsh, et al., 2022) that changes in response speed are subject only to occurrences of feedback following an action, and are unaffected by any occurrences of feedback in the absence of an action (i.e., contingency degradation). The finding that no facilitation occurs when feedback appears after no action is consistent with the facilitation stemming from a sensorimotor process that relies on an efference copy to link between a motor program and 'reafferent' (incoming) sensory feedback (e.g., comparator model; Blakemore et al., 1999).

Third, we have shown that response speed was uninfluenced by whether feedback indicated attainment of negligible or substantial monetary reward, or even no reward at all – as long as some predictable sensory feedback was given (Karsh et al., 2020). This suggests that control feedback, at least in regard to its influence on response speed, serves as an input to a process that is insensitive to the magnitude of reward that was attained by executing the action.

Forth, we set out to test how this process functions in a dynamic environment, where actions gradually become effective or ineffective in controlling the environment (Hemed et al., 2020). Participants performed a choice-response reaction time task in which, on each trial, an imperative cue appeared in one of several possible locations. Pressing a key matching the spatial location of the cue led to immediate and predictable sensory feedback – the imperative cue briefly flashed and disappeared (used in the studies mentioned above; Eitam et al., 2013; Hemed et al., 2022; Karsh et al., 2016, 2020). Crucially, every fixed number of trials (i.e., cycle) feedback was either removed altogether (Experiment 1) or was spatially-perturbed (Experiment 2). The duration of cycles was manipulated between groups

(5 or 10 trials long cycles), but aside from how frequently feedback was made available or unavailable, both conditions received the exact same amount of sensory feedback. Attentional probe trials appeared randomly throughout the trials to discourage participants from keeping track when a cycle will end. The main finding was that whatever speeds up response times is a process that is sensitive to changes in the effect of the responses on the environment. Namely, we found that response speed dynamically tracked changes in the effectiveness of responses by decreasing or increasing when feedback was removed or brought back (or when spatial perturbation was applied to the location of feedback making it not fully predictable and then removed making it predictable again).

While the above work suggests that the basic result of response speed facilitation is driven by a sensorimotor process, the evidence is not decisive, as such we turn to a new functional avenue – testing the sensitivity of the above effect to manipulations of task relevance of the 'control feedback'. In other words, here we ask whether action-effects must be relevant to one's goals to serve as reinforcers. The outcomes of this exploration are also interesting given previous work on the influence of task-relevance manipulations on different sensorimotor processes related to action and motor learning (Parvin et al., 2022), a point which we will return to in the Discussion.

In the current study, we again use the choice reaction time task described above (Hemed et al., 2020) and manipulated over several experiments - either the framing of the task, the mode of response or the resulting visual feedback. In Experiment 1 we made the feedback task-relevant by attaching tangible rewards of different magnitudes to feedback occurrence (i.e., action-effects). Instead of value-free stimuli – the response feedback consisted of images of current Israeli coins and instructing participants to gain as much monetary rewards as possible. In Experiment 2 participants performed a free-choice response task, where they were asked to respond as randomly as possible to an imperative cue. In

Experiment 3 we made feedback highly irrelevant to the main task participants performed, by exposing them to feedback which was not contingent on their responses - participants received the same feedback regardless of whether they responded using the instructed key or another (see Tanaka et al., 2021).

To preview our results, the findings support the idea that control feedback reinforces actions independently of its task relevance and by implication the notion of reinforcement from sensorimotor predictability. We conclude by discussing the findings in relation to recent frameworks and studies that tested the interplay between feedback/stimuli relevance and action.

General Methods

All code and data associated with the paper are found under the GitHub repository associated with the paper (https://github.com/EitanHemed/patches-papers). The repository includes outputs of descriptive statistics in addition to the information brought in the manuscript, as well as an alternative analysis modes which reproduces the pipeline used in previous work (Hemed et al., 2020).

Task

The task in the current study is an adaptation of a choice reaction time task with four possible responses, used previously in several studies (Eitam et al., 2013; Hemed et al., 2020; Karsh et al., 2016, 2020; Tanaka et al., 2021). This task was also used on a previous work (Hemed et al., 2020). The study was approved by the University of Haifa's IRB.

The visual workspace was bounded by two rows of four rectangles (see Figure 1A). The workspace was colored black. The letters S, D, K and L appeared both above and below each row of rectangles, as a reminder of the four response keys. Participants were required to use these response keys using their middle and index fingers of both hands. At the beginning of each task trial an imperative cue - a colored disc (1.4CM in diameter unless noted otherwise) - appeared from one of the rectangles at the top row. The disc descended towards the bottom of the screen at constant pace (~13.3 CM/Second) for a duration of 850MS, disappearing once it reached the bottom rectangle. Participants were required to press the matching key, based on the color and horizontal location of the imperative cue. Each trial was followed by an ITI of 700MS. Regardless of visual feedback (see below), the SOA was 1550MS.

Feedback Manipulations

We manipulated feedback by altering it cyclically, i.e., every fixed number of trials. The study included two different effectiveness-degradation manipulations, used in different experiments – FEEDBACK-ABSENCE and FEEDBACK-PERTURBATION.

Whenever effectiveness-degradation was *not* in operating and the participant responded correctly the imperative cue turned white for 100MS and then disappeared (Figure 1A). During the feedback period (100MS) the cue descended with the same speed and trajectory. If the feedback duration began less than 100MS prior to the end of the response window, the feedback period was shortened to maintain the response window duration at 850MS.

For the FEEDBACK-ABSENCE manipulation experiments, whenever effectiveness-degradation was in action, even if the participant responded correctly, there was no change to the imperative cue. For the FEEDBACK- PERTURBATION manipulation (see Figure 1B), if the participant responded correctly to the cue, it disappeared immediately, and a white disc of the same size appeared at a random angle and offset relative to the location of the cue during the response. The white disc continued to descend with respect to the new, perturbed location for the duration of the feedback period (100 MS).

Both when effectiveness-degradation manipulations were in action and when not, if the participant did not respond correctly, the imperative cue descended on the screen for the whole duration of the response window.

For half of the participants, feedback cycle was altered every 5 trials, and for the other half it was altered every 10 trials. The order of cycles (i.e., Feedback cycles on odd or even cycles) was counterbalanced between participants. The end of one cycle and the beginning of the next was not signaled to the participant in any way.

Attentional Probes

The experiments in the study included attentional probes, which replaced 1/6th of the task trials. While the attentional probes were intended to discourage subjects from ‘counting’ through the alternating cycles and predict when feedback will be altered – the rare probes can create substantial switch costs when returning to the focal task after responding to them (Brumby et al., 2013; Monsell, 2003) ; this was handled by rejecting the data from post-probe trials (see Analysis section below).

On attentional probe trials a yellow triangle (vertex ~4.2 cm) appeared in the middle of the experimental scene (Figure 1B), to which the subject was asked to respond by pressing the spacebar (i.e., a key which was not used to respond to the main task), using their thumbs. There were no action-effects in response to a keypress, regardless of the current cycle. To preclude the possibility that the probes' disappearance will be deemed as feedback, the probe remained on the screen for 1000ms regardless of response accuracy. ITI on probed trials was shortened to 550ms, to maintain the same SOA of 1550ms. Unless specified otherwise, the trials which were replaced with probed trials within the task trials was identical for all participants, predetermined by sampling 70 values from a uniform distribution.

Procedure

Experiment 1-2 were conducted in-person, and Experiment 3 was conducted remotely due to the COVID-19 pandemic. For the in-person experiments, upon arrival to the lab, participants gave their informed consent to participate in the study, were seated in a dimly lit room, viewed an instruction slide depicting the imperative cues and probes the appropriate responses, and asked to respond as accurately and quickly as possible on all trials. Then, they completed 440 trials with no breaks and continued to fill self-report questionnaires – one probing their experiences during the task, the Sense of Agency scale (Tapal et al., 2017), and a demographics' and debrief questionnaire.

Participants

Our participants were recruited through the participant recruitment system of the department of Psychology, University of Haifa. They consisted mostly of undergraduate students. Our stopping rule for sampling participants was pre-registered as reaching a conclusive Bayes Factor of above 3 or below 1/3 (Morey & Rouder, 2011) for a set of critical contrasts, specified in the following section. Based on previous findings (Cohen's d ~0.4-0.5), we estimated that this will require about 40 participants on each of tested groups (Hemed, Bakbani Elkayam, et al., 2022).

Data processing

Preprocessing

Our preprocessing included two main stages – (a) labelling and binning trials based on recent feedback (b) removing invalid trials.

Binning

The first stage is graphically depicted in Figure 1D and it is advised to consult the figure to thoroughly understand it. The binning stage was as follows:

1. For each participant we obtained the series of task trials with a correct response (i.e., not incorrect or missing), and treated it as a sequential series, skipping the rare attentional probes and incorrect responses or response omissions. Each of these trials, based on the cycle in which it was placed, was labelled as a trial on which the response led to RSP feedback (e.g., spatially-predictable) or not (e.g., spatially-perturbed, or no feedback, pending the experiment).
2. Labeling of the *Prior* value of each trial, by shifting the feedback value assigned at the previous by a lag of 1. For example, a trial immediately following a spatially-predictable feedback occurrence was labelled as $Prior_1$ (feedback on N-1), and one following no-feedback was labelled as $Prior_0$ (no feedback on N-1). *Context*, described below, can also be seen as the summation of the ***Prior*** values on trials N-3 through N-1.
3. Independently of the *Prior* bin in which a trial was placed, we assigned the *Context* value of each trial, by summing the RSP feedback occurrences assigned on trials N-4 through N-2 (i.e., 0, 1, 2 or 3). A trial labelled as $Context_0$ was one which on trials N-4 through N-2 there were no feedback occurrences (or all were spatially-unpredictable ones, in the case of Experiment 2). A trial labeled as $Context_2$ was one in which on trials N-4 through N-2 there were exactly two RSP feedback occurrences.

Using two different functions was crucial to dissociating the immediate feedback occurrences (*Prior* – trial N-1) or the less recent history (*Context* – trials N-4 through N-2), and how the two interacted. The length of the dimension was based on the length of the shortest of the two cycles (see (Hemed et al., 2018; Hemed, Bakbani Elkayam, et al., 2022); also Hemed et al., 2018).

Screening

Next, we marked trials as invalid for analysis based on the following criteria – (a) trials which contained attentional probes, (b) trials immediately following attentional probes, (c) incorrect responses or response omissions on task trials, (d) trials immediately following a task trial with no-correct response, (e) trials which were extremely fast or extremely slow based on previous studies using the task (Hemed, Bakbani Elkayam, et al., 2022).

Note that we also did not analyze the few valid trials which did not have a defined *Prior* and *Context* values, occurring at the beginning of the experimental session.

Finally, removing only trials that immediately followed a probe was shown to address the reduce most of the switch costs the induce and to produce a pattern identical to experiments in which such probes were not included while maintaining participants lack of awareness regarding the structure of the task and a sufficient number of task trials (Brumby et al., 2013; Hemed, Bakbani Elkayam, et al., 2022; Monsell, 2003). For a validation of our findings while not using attentional probes, see elsewhere (Hemed, Bakbani Elkayam, et al., 2022).

Statistical Modeling and predictions

Our statistical modeling was similar for all experiments, with small changes for Experiment 1, where we had an additional between-subject factor. Otherwise, we conducted a 2 X 2 X 4 repeated measures ANOVA model which tested the joint effects of Cycle Duration (between-subjects, 5 or 10 trials), Feedback on prior trial N-1 (within-subjects, Feedback, No-Feedback) and Context of feedback occurrences on trials *n-4* through *n-2* (within-subjects, 0, 1, 2, 3) on response speed. For Experiment 1 we added another between-subjects factor – *Feedback Type* (see below).

The essence of the RSP hypothesis is that feedback reinforces response-execution independently of performance (Eitam, Kennedy, et al., 2013; Hemed, Karsh, et al., 2022; Karsh & Eitam, 2015). Thus), our main prediction for the ANOVA is that responses will be facilitated when they follow feedback (i.e., *Prior*).

As for *Context*, we predicted that it will interact with *Prior* based on previous findings, not necessarily according to our theory. On an unpublished manuscript (Hemed et al., 2018; see also Hemed, Bakbani Elkayam, et al., 2022) , participants received feedback occurrences randomly, rather than in fixed cycles. On that study we uncovered that the sum of feedback occurrences three-responses back seemed to be the best model for predicting response speed. On our previous work we noted that *Prior* and *Context* interacted to create the pattern testable by the contrasts specified below.

. We predicted that the frequency of alternating feedback and no-feedback (*Cycle-Duration)* will not have a significant role in modifying response speed.

The ANOVA model was followed by a series of paired Bayesian and frequentist t-tests. For each group (e.g., 5-trials cycle duration group) we tested how response time varies when a trial follows a series of No feedback trials ($Context_0$ – no feedback occurrences on trials N-4 through N-2) vs. a series of Feedback trials (*Context3* – feedback on each of trials N-4 through N-2). We repeated this contrast separately for each group and for $Prior_0$ (No feedback on trial N-1) and $Prior_1$ (Feedback on trial N-1).

We predict that if the on the most recent trial feedback was received ($Prior_1$) then responses following a series of feedback occurrences ($Context_3$) will be considerably faster compared with responses following a series of No-feedback trials ($Context_0$). Thus, the t-tests here were one-tailed, predicting facilitation in response time.

Based on previous findings (see Hemed et al., 2022) but to a lesser degree on our theory, we predicted that when the most recent trial did not include feedback (Prior_0), there will be no change in response time between trials which follow a series of feedback occurrences (*Context*$_3$) and those which follow a series of no-feedback occurrences (*Context*$_0$). Here we used two-tailed tests, and the Bayesian t-test was crucial as it allowed to support the null hypothesis rather than not just rejecting it (Kruschke, 2013).

To be put differently, our predictions are that responses will be reinforced if recent feedback was received and will be further reinforced if This categorization and analysis approach was used by us also on a recent work where we also discuss in depth the importance of dissociating the most recent trial (*Prior*) from the less recent history (*Context*).

Note that to some extent, our predictions here deviate from the pre-registered ones, given the pre-registration occurred *prior* to discovering a confound in our previous work (Hemed, Bakbani Elkayam, et al., 2022). The main change is the novel prediction for *no* effect of context given *Prior*$_0$ which was previously found on the 10-trials group. For that matter, our analysis can be cast as exploratory relative to the pre-registration, but confirmatory relative to our recent, amended publication (Hemed, Bakbani Elkayam, et al., 2022).

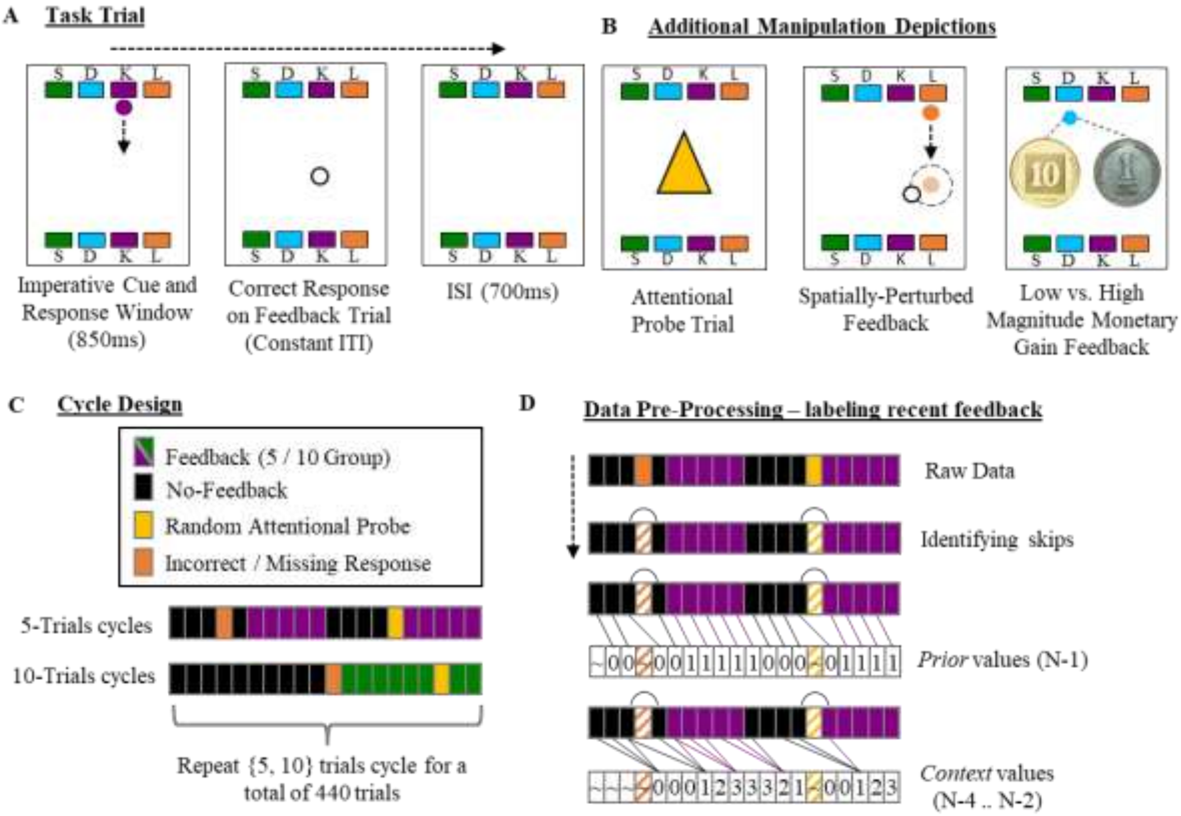

Figure 1: (A) In the task used on the current study participants' keypresses on feedback trials caused an imperative cue to flash and disappear. (B) On attentional probe trials (1/6th of all trials), an out-of-set response was required; On the Spatial-Perturbation manipulations (Experiment 3), the ‘feedback’ was randomly placed around the cue on each “No-Feedback” trial; Enlarged depiction of the images of coins used as feedback on Experiment 1. (C) Pending experimental group, feedback and no-feedback were repeatedly cycled either every 5 or 10 trials. (D) Depiction of the binning of trials based on their values on each of the two ca of Prior and Context and skipping of probed trials and non-correct responses. Purple diagonal lines indicate ‘Feedback’, black diagonal lines indicate ‘No Feedback’ (or spatial-perturbation).

Experiment 1

The experiment was pre-registered on the open science framework (https://osf.io/gcev7).

Method

Participants

We recruited 296 participants. The demographics data for 6 was not obtained. The participants were aged 17.0-45.0 (M = 24.78, SD = 4.30), 59.31% identified as female. Initially, we collected 20 participants on each between-subject condition. We continued to sample participants, given our predictions were pre-registered and our stopping rule for data sampling relied on Bayesian analyses.

Apparatus

The experiment was programmed in PsychoPy2 Version 1.83 (Peirce et al., 2019). Stimuli were presented on a BENQ XL2420T screen, set to 120Hz refresh rate. Responses were collected using a standard PC keyboard.

Design

The experiment had a 2 X 3 between-subject design, with the groups based on type of feedback (CONTROL FEEDBACK, SUBSTANTIAL OUTCOME, NEGLIGIBLE OUTCOME) and the duration of each cycle (5-TRIALS, 10-TRIALS).

Task

The feedback image was the standard blank white disc for the CONTROL FEEDBACK group. For the monetary gain conditions, the image depicted an Israeli coin. The coin value was either relatively high (~$0.3) or low (~$0.03), for the SUBSTANTIAL OUTCOME and NEGLIGIBLE OUTCOME conditions, respectively. Sitting distance from the monitor was not controlled for but was approximately 60CM.

Procedure

The procedure is identical to the one specified under General Methods, except that participants in the monetary gain conditions were also instructed that they can earn up to 50% of the value of the coins that they will view during the experiment. Following the experiment and questionnaires, participants on the SUBSTANTIAL OUTCOME or NEGLIGIBLE OUTCOME conditions received additional bonus sums based on their performance (proportion of correct responses times ~$9.2 or ~$0.92, respectively).

Results

As described in the introduction, previously some of us (Karsh et al., 2020) have shown that reinforcement of response-execution is insensitive to the reward magnitude which is signaled by feedback. Thus, as monetary gain was not predicted to influence response speed, we predicted that *Feedback-Type* will have no main effect on response speed or will interact with other factors to influence it.

Pre-Processing

The amounts specified below refer only to the portion of task trials, excluding all probe trials (16.6% of raw data). Note that data from a participant or a specific trial can be invalid due to more than one reason. We removed task trials with incorrect (8.82%) or missing (1.01%) responses, task trials with extremely fast (RT < 100, 0.5%) or slow RTs (RT > 750, 1.68%). Next we removed the data from a total of 30 participants (10.14% out of 296) where their accuracy on task trials was below < 80% (N = 19), accuracy on attentional probe trials was below < 50% (N = 5), or where less than 80% of the trials were valid in terms of either RT, accuracy or both (N = 28). In total, 17.44% of the task trials were removed, by filtering whole participants' data or individual trials.. Note although not ‘invalid’ per-se we did not analyze trials which followed attentional probes or task-trials which did not contain correct responses, to avoid post-error slowing and task-switching costs (see above).

Statistical Analysis

We used a 3 X 2 X 4 X 2 mixed ANOVA to analyze response time data. The between-subject factors were Feedback Type (CONTROL FEEDBACK, SUBSTANTIAL OUTCOME or NEGLIBILE OUTCOME) and Cycle Duration (5 or 10 trials). The within-subject factors were Context of feedback occurrences on trials N-4 through N-2 (0, 1, 2, 3) and Occurrence of feedback on trial N-1 (0, No-1). The ANOVA's cell means, and 95% confidence intervals are shown in Figure 2.

We found a significant medium-sized effect of *Prior* on response time [F(1, 260) = 23.20, p = 0.001, Partial Eta-Sq. = 0.08] replicating our previous findings (Eitam, Kennedy, et al., 2013; Hemed, Karsh, et al., 2022). Additionally, we found a small effect for *Context* [F(3, 771) = 6.23, p = 0.001, Partial Eta-Sq. = 0.02], but no effect of Cycle-Duration [F(1, 260) = 0.01, p = 0.908, Partial Eta-Sq. < 0.01]. We found no effect of Feedback Type [F(2, 260) = 0.88, p = 0.414, Partial Eta-Sq. = 0.01], replicating previous findings (Karsh et al., 2020) .

As per interactions, there was a significant yet small interaction between *Prior* and *Context* [F(3, 736) = 22.11, p = 0.001, Partial Eta-Sq. = 0.08], replicating other findings (Hemed, Bakbani Elkayam, et al., 2022). There was an interaction between *Context* and Cycle-Duration [F(3, 771) = 3.16, p = 0.024, Partial Eta-Sq. = 0.01], a marginally significant interaction between *Context* and Feedback-Type [F(6, 771) = 2.08, p = 0.055, Partial Eta-Sq. = 0.02] and the 3-way interaction between Prior, Context and Cycle-Duration, however, was significant [F(3, 736) = 4.00, p = 0.009, Partial Eta-Sq. = 0.01], yet of very small size. All other interactions were not significant - *Prior* X Feedback [F(2, 260) = 1.13, p = 0.326, Partial Eta-Sq. = 0.01], *Prior* X Cycle Duration [F(1, 260) = 1.69, p = 0.195, Partial Eta-Sq.

= 0.01], the three-way interaction of *Prior X* Cycle Duration X Feedback Type [$F(2, 260) = 0.18$, $p = 0.837$, Partial Eta-Sq. < 0.01], Context X Cycle Duration X Feedback Type [$F(6, 771) = 0.76$, $p = 0.601$, Partial Eta-Sq. = 0.01], and the four-way interaction between all factors was too non-significant [$F(6, 736) = 1.21$, $p = 0.299$, Partial Eta-Sq. = 0.01].

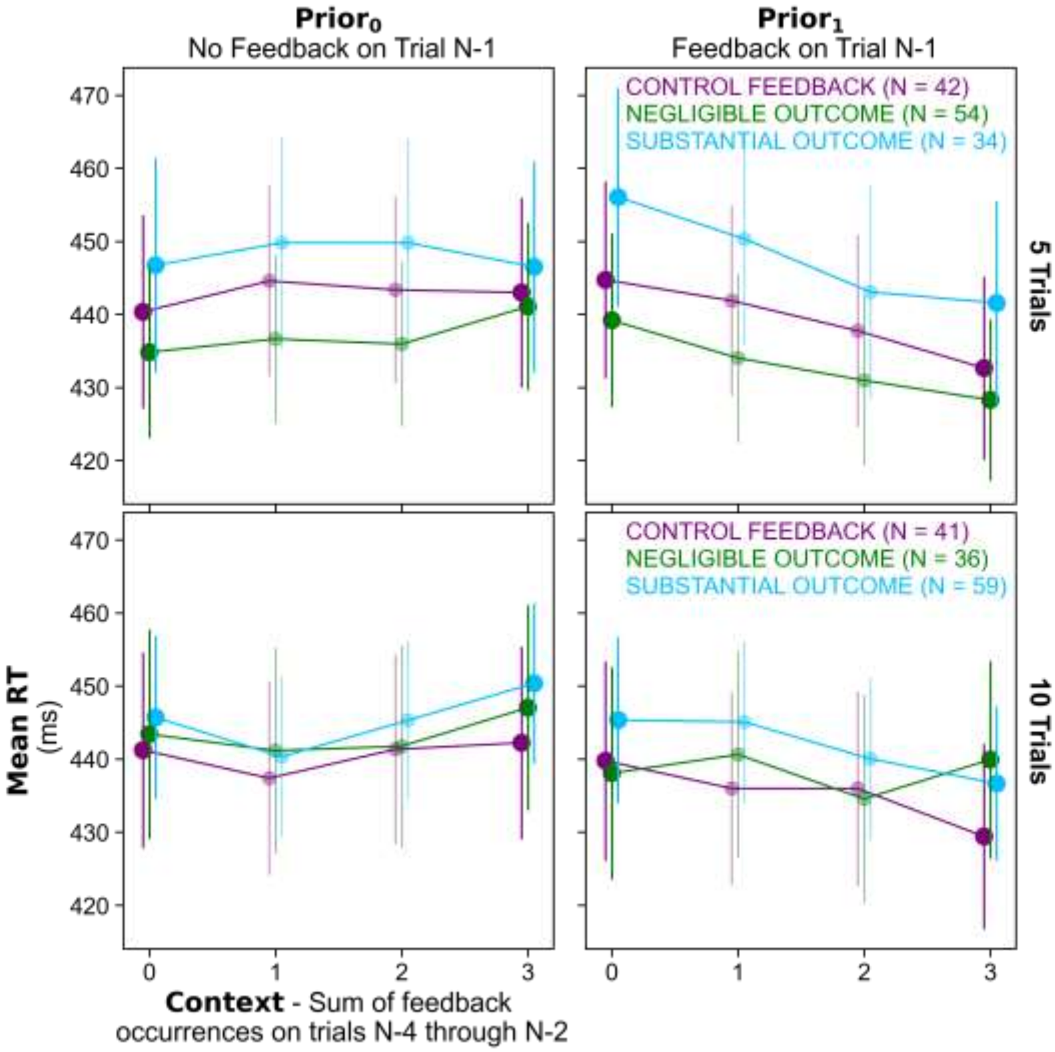

Figure 4: Experiment 2, response speed as function of feedback type and occurrences. Prior (action-effect on trial N-1) speeds up responding (compare left and right panels) but is modulated by the recent Context (sum of action-effects on trials N-4 through N-2; see X-axis). The influence of Cycle Duration (upper vs. lower panels) or Feedback-Type (separate lines) is minute and inconsistent in comparison. The 0 and 3 values on the X-axis are accentuated as they are used for the planned contrasts.

Next, we tested the contrast of [$Context_3$-$Context_0$], separately for each of the combinations of Feedback Type Groups and Cycle duration groups, pending whether feedback was received on the previous trial (*Prior1)* or not (*Prior0).*

Figure 3 displays individual means for each contrast, group means including CIs and sequential analysis Bayes factors (the result of a Bayesian t-test calculated incrementally for each additional sample, to examine the robustness of the pattern).

5-Trials Cycle Duration groups

As predicted, receiving feedback on trial N-1 ($Prior_1$), after a streak of previous Feedback trials ($Context_3$) compared with after a previous streak of No-Feedback trials ($Context_0$) significantly facilitated response speed for both the CONTROL FEEDBACK group [-12.07 MS (16.20); $t(41) = -4.77$, $p < 0.001$, Cohen's d = -0.74, (-1.07, -0.39), BF1:0 = 1817.6096], the NEGLIGIBLE-OUTCOME [-10.90 MS (20.28); $t(53) = -3.91$, $p < 0.001$, Cohen's d = -0.53, (-0.82, -0.25), BF1:0 = 187.1466] group and the SUBSTANTIAL-OUTCOME group [-14.49 MS (22.27); $t(33) = -3.74$, $p < 0.001$, Cohen's d = -0.64, (-1.01, -0.27), BF1:0 = 87.3405].

Receiving no-feedback on trial N-1 ($Prior_0$), a streak of previous Feedback trials ($Context_3$) compared with after a previous streak of No-Feedback trials ($Context_0$) did not significantly slow down response speed for the CONTROL FEEDBACK group [2.65 MS (17.41); $t(41) = 0.97$, $p = 0.335$, Cohen's d = 0.15, (-0.15, 0.45), BF1:0 = 0.2599], and the SUBSTANTIAL-OUTCOME group [-0.22 MS (20.80); $t(33) = -0.06$, $p = 0.951$, Cohen's d = -0.01, (-0.35, 0.33), BF1:0 = 0.1840], and the NEGLIGIBLE-OUTCOME group [6.26 MS (18.65); $t(53) = 2.44$, $p = 0.018$, Cohen's d = 0.33, (0.06, 0.61), BF1:0 = 2.2165], although with inconclusive Bayesian support for the latter.

10-Trials Cycle Duration groups

In partial accordance with our hypothesis, given feedback on trial N-1 ($Prior_1$), a streak of previous Feedback trials ($Context_3$) compared with after a previous streak of No-Feedback trials ($Context_0$) significantly facilitated response speed for both the CONTROL FEEDBACK group [-10.35 MS (18.54); $t(40) = -3.53$, $p < 0.001$, Cohen's d = -0.55, (-0.88, -0.22), BF1:0 = 57.8646] and the SUBSTANTIAL-OUTCOME group [-8.68 MS (21.29); $t(58) = -3.11$, $p = 0.001$, Cohen's d = -0.40, (-0.67, -0.14), BF1:0 = 20.6184], but there we found no facilitation for the NEGLIGIBLE-OUTCOME group [1.86 MS (14.82); $t(35) =$

0.74, p = 0.769, Cohen's d = 0.12, (-0.20, 0.45), BF1:0 = 0.1104] with support for the null hypothesis.

Given no-feedback on trial N-1 ($Prior_0$), a streak of previous Feedback trials ($Context_3$) compared with after a previous streak of No-Feedback trials ($Context_0$) did not significantly slow down response speed for the CONTROL FEEDBACK group [1.00 MS (23.55); t(40) = 0.27, p = 0.789, Cohen's d = 0.04, (-0.26, 0.35), BF1:0 = 0.1745], the NEGLIGIBLE-OUTCOME [3.62 MS (20.41); t(35) = 1.05, p = 0.301, Cohen's d = 0.17, (-0.16, 0.50), BF1:0 = 0.2976] group or the SUBSTANTIAL-OUTCOME group [4.67 MS (22.14); t(58) = 1.61, p = 0.114, Cohen's d = 0.21, (-0.05, 0.47), BF1:0 = 0.4773]. Note that for the 10-trials groups we found mild inhibition of response time, rather than no change.

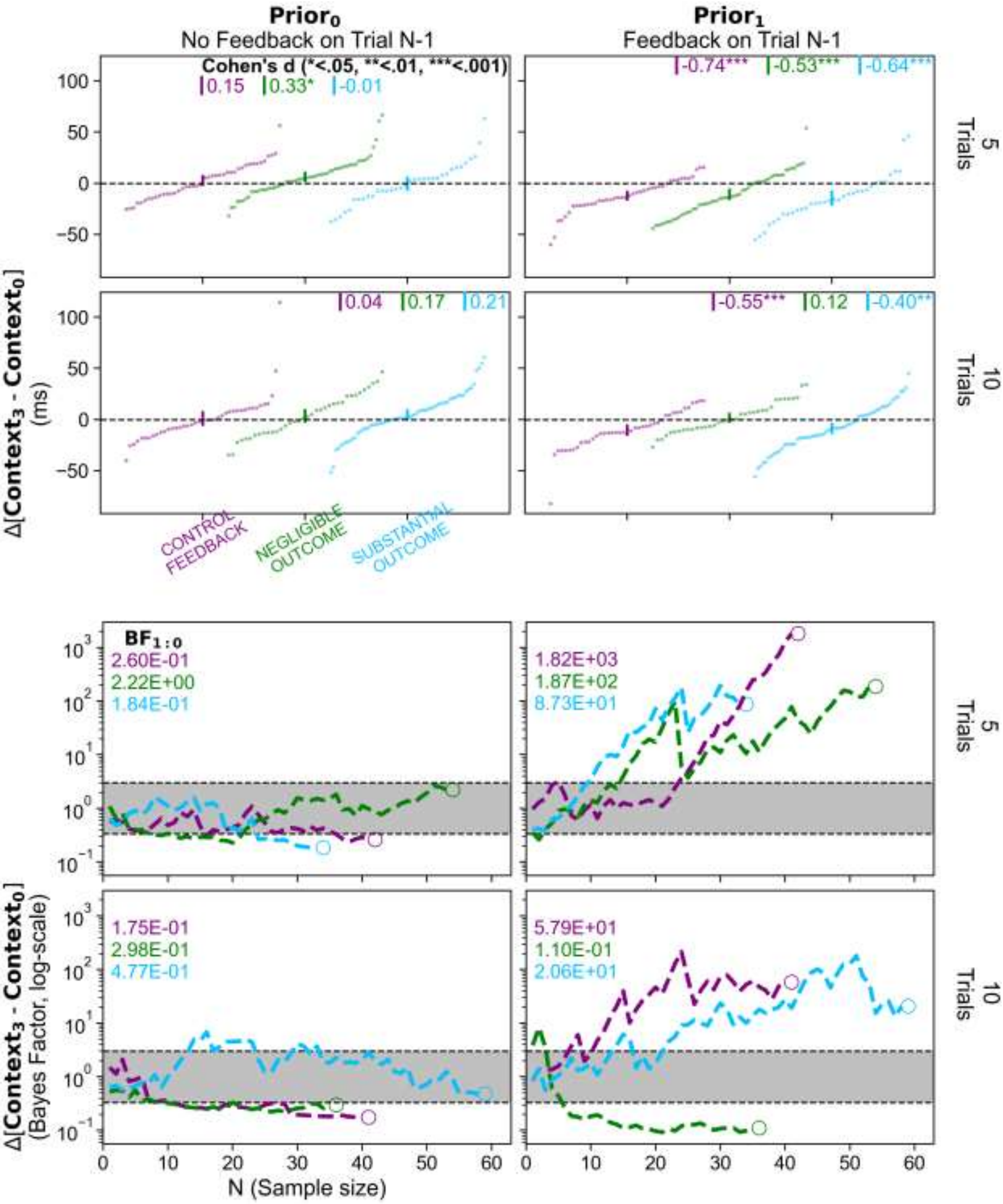

Figure 3: Experiment 1, pairwise contrasts for [Context $_3$ - Context $_0$]. Column denotes 'Prior' (feedback occurrence on trial N-1), color denotes experimental group. Rows indicate Cycle Duration. ***Top*** *– 95%-CI of mean difference. Scatter indicates individual means (sorted to minimize overlap). Annotation indicates Cohen-d effect size.* ***Bottom*** *– Bayes factors obtained using sequential Bayesian t-tests. Missing Bayes factors values on sequential Bayesian t-tests (e.g., when N ~ < 3) are not plotted. Shaded area indicates inconclusive result (1/3 < BF < 3). The large point indicates the terminal Bayes factor on the sequential analysis.*

Discussion

Our analyses, shows that recent RSP feedback (*Prior*) facilitated response speed. However, the full pattern was uncovered using the two contrasts - if feedback appeared sequentially also on less recent trials (*Context)* response speed sped up. . In contrast, following a no-feedback ($Prior_0$) occurrence there is support for no change in response speed pending previous trials.

On the 5-trials groups, this pattern was found for all Feedback-Type groups. However, for the 10-trials groups, this pattern was found only for the CONTROL-FEEDBACK group, with middling support for the SUBSTANTIAL-OUTCOME group, and support for the null on the NEGLIBILE-OUTCOME GROUP. Crucially, although nominally different there was no significant or strong difference between the grand-average response speed of different Feedback-Type groups. This shows that there is very little difference between the effect of feedback about the 'effect' of a motor program and feedback about the reward (outcome) resulting from this response, even in a dynamic environment where feedback availability constantly changes. This finding also corroborates, in a dynamic setting that is changing trial by trial, recent work (Karsh et al., 2020), where we have shown that the evaluation of action’s effectiveness is unaffected by substantial rewards.

In other words, the results of Experiment 1 suggest that the relevance of the feedback in terms of information it carries about outcomes, or 'outcome relevance' (Eitam & Higgins, 2010), does not modulate the effect of 'control feedback' or reinforcement from sensorimotor predictability (RSP; Hemed et al., 2022).

Experiment 2

In Experiment 2 feedback was independent of response accuracy and hence was task-irrelevant. Instead of the spatially mapped cued responding used in Experiment 1, in Experiment 2 participants could freely choose out of the set of response keys on each trial (i.e., all keys except for the attentional probe response key). Thus, Experiment 2 also served as a control experiment, as critique that could be forwarded against Experiment 1 is that the feedback served not only as 'control feedback', but also as performance feedback because participants were required to press the correct key to receive feedback and hence it was in fact – task relevant (and hence in a real sense outcome relevant). The experiment was pre-registered on the open science framework (https://osf.io/gcev7), but as for Experiment 1, it was pre-registered prior to our knowledge of the confound specified above, and hence our statistical predictions here have changed

Method

In Experiment 2 we used a free-choice variation of the task described above, similar to a one previously used on other studies (Karsh & Eitam, 2015; Karsh et al., 2016; Penton et al., 2018). The task design follows the one specified under the General Methods section, save for several changes.

The imperative cue was colored red on all trials and appeared in the center of the visual workspace and did not move during the response window. The response window on task trials was 1000MS. The duration of the feedback period was 150MS, and the imperative cue was shown for the remainder of the response window following the presentation of feedback (i.e., it turned white during the feedback period and then was turned red again for the remainder of the trial). The two rows of rectangles defining the visual workspace were all colored grey. Participants were instructed to select one of the task keys at random on each trial, and respond using it as quickly as possible, unless the attentional probe was displayed. They were asked to avoid repeating predictable patterns such as responding using the task keys by their order on the keyboard (S-D-K-L).

Participants

We recruited 79 participants. The participants were aged 17.0-44.0 (M = 24.76, SD = 4.72), 63.29% identified as female.

Results

Pre-Processing

The amounts specified below refer only to the portion of task trials, excluding all probe trials (16.6% of raw data). Note that data from a participant or a specific trial can be invalid due to more than one reason. We removed task trials with incorrect (1.04%) or missing (1.01%) responses, task trials with extremely fast ($RT < 100$, 1.04%) or slow RTs ($RT > 850$, 3.26%). Next we removed the data from a total of 4 participants (5.06% out of 79) where their accuracy on task trials was below < 80% (N = 0), accuracy on attentional probe trials was below < 50% (N = 1), or where less than 80% of the trials were valid in terms of either RT, accuracy or both (N = 3). In total, 9.26% of the task trials were removed, by filtering whole participants' data or individual trials.

Statistical Analysis

<u>ANOVA</u>

Contrary to our prediction and previous findings, we found that there was no significant effect of *Prior* [$F(1, 73) = 1.95$, $p = 0.166$, Partial Eta-Sq. = 0.03] or *Context* [$F(3, 210) = 0.38$, $p = 0.759$, Partial Eta-Sq. = 0.01]. As predicted there was no effect of Cycle Duration [$F(1, 73) = 0.44$, $p = 0.508$, Partial Eta-Sq. = 0.01]. Out of the two-way interactions only *Prior* X *Context* (as predicted) was statistically significant [F(3, 198) =

6.38, p = 0.001, Partial Eta-Sq. = 0.08] . The interactions of *Prior* and *Cycle Duration* [F(1, 73) = 1.70, p = 0.197, Partial Eta-Sq. = 0.02] or *Context* and *Cycle-Duration* [F(3, 210) = 0.82, p = 0.480, Partial Eta-Sq. = 0.01] were not significant. The three-way interaction between *Prior*, *Context*, and *Cycle-Duration* was statistically significant with a small effect size [F(3, 198) = 4.57, p = 0.005, Partial Eta-Sq. = 0.06]. The ANOVA's cell means, and 95% confidence intervals are shown in the two top panels of Figure 4.

Pairwise contrasts

Next we analyzed the key contrast of [$Context_3$ - $Context_0$] separately for each of the *Cycle-Duration* groups, under $Prior_0$ and $Prior_1$. On Figure 4's mid and bottom panels we display the individual means for each contrast, group means including CIs and sequential analysis Bayes factors.

In partial accordance with the results from Experiment 1, feedback on trial N-1 ($Prior_1$), a streak of previous Feedback trials ($Context_3$) significantly facilitated response speed compared with after a previous streak of No-Feedback trials ($Context_0$). This was true for the 5-trials group [-14.12 MS (23.43); t(36) = -3.62, p < 0.001, Cohen's d = -0.59, (-0.94, -0.24), BF1:0 = 67.9124] but this pattern did not replicate for the 10-Trials group [0.88 MS (35.91); t(37) = 0.15, p = 0.559, Cohen's d = 0.02, (-0.29, 0.34), BF1:0 = 0.1564], with conclusive Bayes-factor support for both results.

Given no-feedback on trial N-1 ($Prior_0$), a streak of previous Feedback trials ($Context_3$) compared with after a previous streak of No-Feedback trials ($Context_0$) significantly inhibited response speed for either the 5-trials [19.47 MS (27.49); t(36) = 4.25, p < 0.001, Cohen's d = 0.70, (0.33, 1.05), BF1:0 = 177.1149], but for the 10-trials group there was no change in response speed as predicted not replicate for the 10-Trials group [3.58 MS (33.98); t(37) = 0.64, p = 0.525, Cohen's d = 0.10, (-0.22, 0.42), BF1:0 = 0.2116] with conclusive Bayes-factor support for both results.

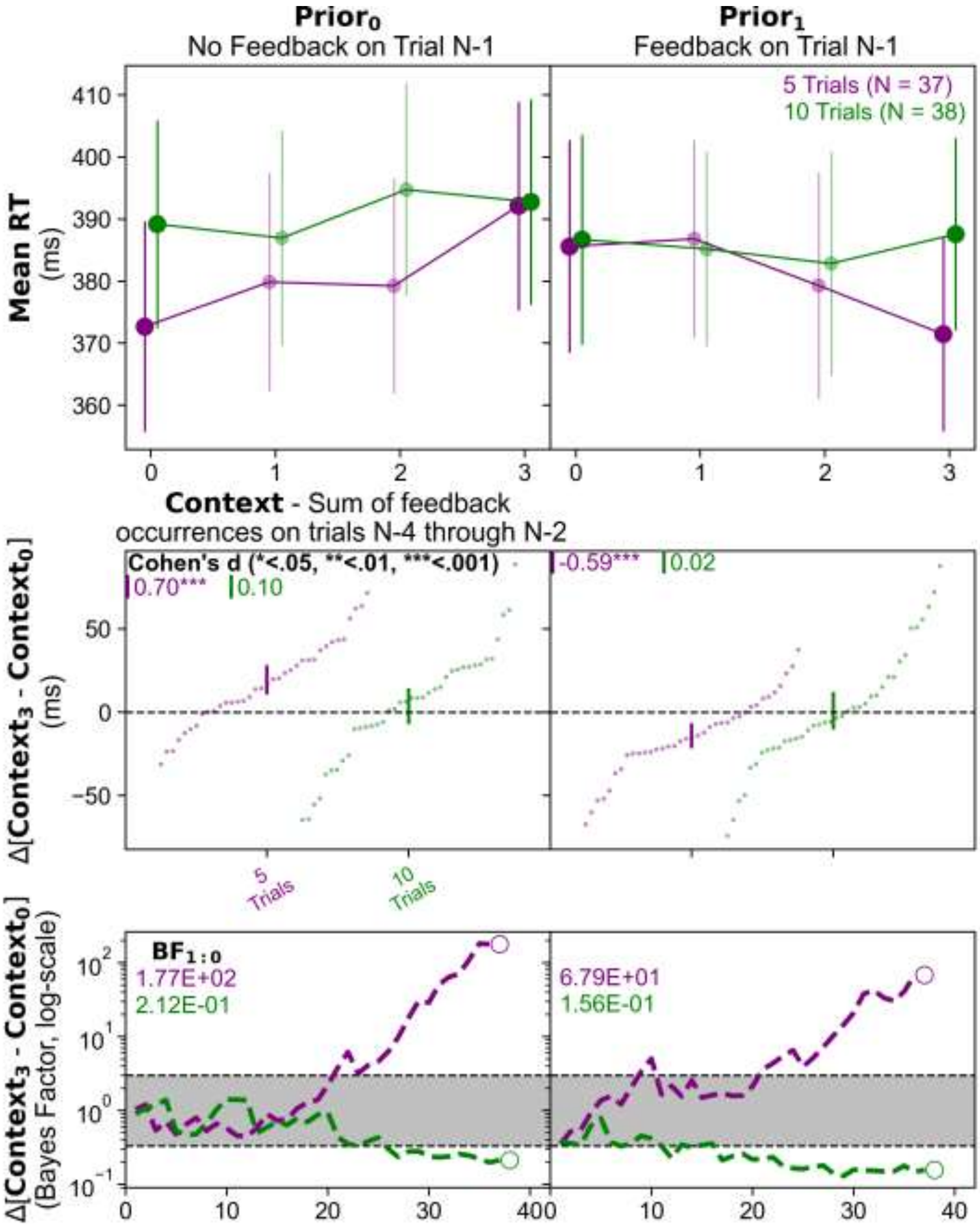

Figure 4: Experiment 2, Response speed as function of feedback occurrences. Top – Response Time by ANOVA terms. Mid - contrasts for RT on [*Context* $_3$ *– Context* $_0$]*, 95%-CI of mean difference. Bottom – Sequential Bayesian t-tests on the contrast of* [*Context* $_3$ *– Context* $_0$]*.*

Experiment 3

Experiment 3 built on an online validation the paradigm (see Hemed et al., 2022). In this experiment we sought to make the action-effects fully task-irrelevant by being maximally explicit about the lack of the response-feedback. While in Experiment 2 participants responded freely by choosing a random key out of the task key set, in Experiment 3 participants again viewed the imperative cue instructing them to respond using a specific key. However, even if they did not respond the correct key, but rather responded with any of the other active task keys they received the response-effect which matched the cycle they were on. Additionally, the lack of contingency between the key selection and the resulting action-effects was emphasized verbally in a practice phase in which participants were required to respond using the incorrect key on a large portion of the trials. The experiment was pre-registered on the open science framework (https://osf.io/gcev7). As for the two previous experiments, Experiment 3 was pre-registered prior to the uncovering of the confound induced by attentional probes, and our amended pre-processing.

Method

In Experiment 3 we used the task described under the General Methods section, with several changes. The experiment was prepared using PsychoPy3 and conducted online rather than in person, due to COVID-19 restrictions. In the current experiment responding with any of the task-keys, yielded the matching feedback on task-trials (i.e., participants were not required to select the correct key to receive feedback, similarly, to Experiment 1a). Here we used the Spatial-Perturbation effectiveness-degradation, also used elsewhere (Hemed et al., 2020; Karsh et al., 2016). In addition to the standard instructions urging participants to respond as quickly and accurately as possible, we instructed them that the cue will flash in white from time to time, and *that the flashes are irrelevant to their performance and can be ignored*.

Next, participants performed a 20 trials-long training block to demonstrate the irrelevance of the feedback to the accuracy of their responses. During practice trials an on-screen text notification was shown throughout each trial, stating which key is correct on the current trial and which key the participant should respond with. On each of the practice trials, there was a 40% chance that the participant will be instructed to respond using an incorrect task key (e.g., “The correct key is S; Respond using L”) to drive home the message that a predictable response-effect will also follow an incorrect response (on spatially predictable cycles). Following a slide with reminders of the task objectives, participants continued to perform an experimental block of 440 trials, as on previous experiments in this work. Throughout the experiment an on-screen counter displayed the percentage of trials completed so far (updated each trial).

Participants

We recruited 235 participants. The participants were aged 18.0-45.0 (M = 25.32, SD = 5.39), 74.04% identified as female.

Results

Pre-Processing

The amounts specified below refer only to the portion of task trials, excluding all probe trials (16.6% of raw data). Note that data from a participant or a specific trial can be invalid due to more than one reason. We removed task trials with incorrect (7.01%) or missing (2.29%) responses, task trials with extremely fast (RT < 100, 0.1%) or slow RTs (RT > 750, 1.09%). Next we removed the data from a total of 39 participants (16.60% out of 235) where their accuracy on task trials was below < 80% (N = 12), accuracy on attentional probe trials was below < 50% (N = 24), or where less than 80% of the trials were valid in terms of either

RT, accuracy or both (N = 35). In total, 23.33% of the task trials were removed, by filtering whole participants' data or individual trials.

Statistical Analysis

ANOVA

In line with our predictions, we found a main effect of Prior [$F(1, 194) = 14.23$, $p = 0.001$, Partial Eta-Sq. = 0.07] and of Context [$F(3, 557) = 3.51$, $p = 0.017$, Partial Eta-Sq. = 0.02]. *Cycle-Duration* had no effect on response time as predicted [$F(1, 194) = 0.06$, $p = 0.807$, Partial Eta-Sq. < 0.01]. There was a significant interaction between *Prior* and *Context* [$F(3, 558) = 4.82$, $p = 0.003$, Partial Eta-Sq. = 0.02] . Otherwise, the interactions were nonsignificant – *Prior* X *Cycle Duration* [$F(1, 194) = 0.28$, $p = 0.598$, Partial Eta-Sq. < 0.01], *Context* X *Cycle Duration* [$F(3, 557) = 0.62$, $p = 0.594$, Partial Eta-Sq. < 0.01] and the three-way interaction of *Prior* X *Context* X *Cycle Duration* [$F(3, 558) = 0.94$, $p = 0.417$, Partial Eta-Sq. = 0.01]]. The ANOVA's cell means, and 95% confidence intervals are shown in the two top panels of Figure 5.

Pairwise contrasts

The contrasts are depicted in the two bottom rows of panels on Figure 5. Given feedback on trial N-1 ($Prior_1$), a streak of previous Feedback trials ($Context_3$) compared with after a previous streak of No-Feedback trials ($Context_0$) significantly facilitated response speed for the 5-trials group [-9.74 MS (16.81); $t(88) = -5.44$, $p < 0.001$, Cohen's $d = -0.58$, (-0.80, -0.35), BF1:0 = 57190.6308] as we predicted, but with inconclusive Bayes factor for the 10-trials group [-4.22 MS (22.52); $t(106) = -1.93$, $p = 0.028$, Cohen's $d = -0.19$, (-0.38, 0.01), BF1:0 = 1.2376].

Given no-feedback on trial N-1 ($Prior_0$), we found as predicted that a streak of previous Feedback trials ($Context_3$) compared with after a previous streak of No-Feedback trials (*Context*$_0$) did not significantly change response speed. That was found for both the 5-trials [1.70 MS (16.38); $t(88) = 0.97$, $p = 0.334$, Cohen's d = 0.10, (-0.11, 0.31), BF1:0 = 0.1847], and the 10-Trials group [0.19 MS (19.57); $t(106) = 0.10$, $p = 0.922$, Cohen's d = 0.01, (-0.18, 0.20), BF1:0 = 0.1077], with conclusive Bayes factor.

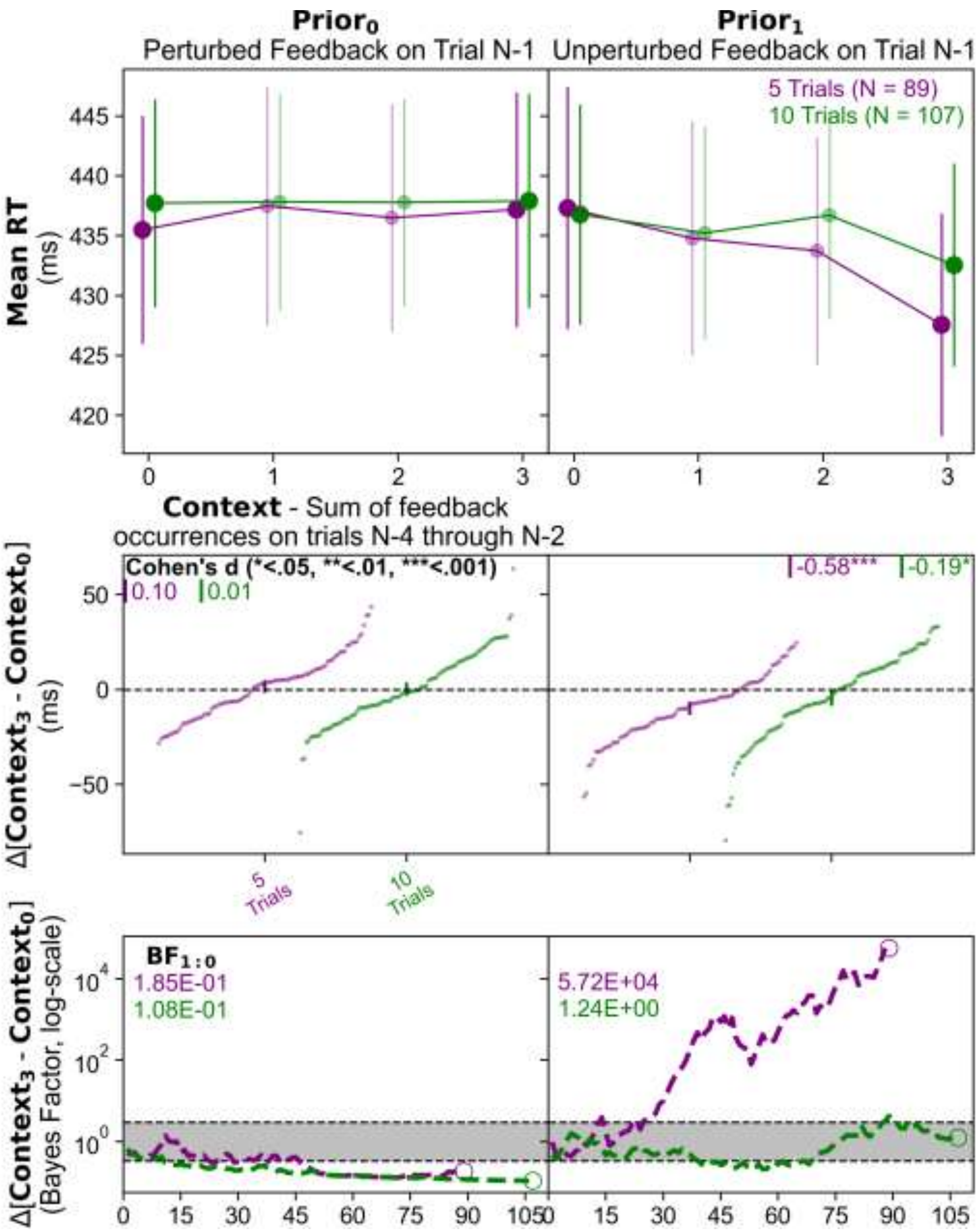

Figure 5: Response speed as function of feedback occurrences. Top – Response Time by ANOVA terms. Mid - contrasts for RT o [*Context*$_3$ – *Context*$_0$], *95%-CI of mean difference.* ***Bottom*** *– Sequential Bayesian t-tests on the contrast of* [*Context*$_3$ – *Context*$_0$].

General Discussion

This study builds on recent work, in which we used a similar task to explore the change in reinforcement pending dynamic changes in feedback. Here we examined whether and how changes in the task-relevance of own-action effects influence the phenomenon of reinforcement from sensorimotor predictability (RSP).

Generally, we find that RSP is independent of the relevance of the sensory information in the sense of its association with tangible outcomes (Experiment 1) or task relevance (Experiments 2 & 3).

On the one hand, this result is surprising as so called 'automatic' processes have been repeatedly shown to be fully dependent on the task relevance of the 'stimuli' (See Eitam & Higgins, 2010, for a review). For example, 'automatic' or unintentional phenomena such as semantic priming have also been shown to be highly sensitive to the task relevance of the primes (Gast & Rothermund, 2011). Even the categorization of so called 'attended' stimuli has been shown to be highly dependent on the stimuli's task relevance (Chen & Wyble, 2015; Eitam, Yeshurun, et al., 2013) ; with subjects' performance of simple and foveally presented yet task irrelevant stimuli at chance level (Wyble et al., 2019). Yet other phenomena such as implicit or statistical learning – loosely defined as the unintentional acquisition of the structure of stimuli (such as repetitions, conditional dependencies and such) were also shown to occur only for task relevance stimuli or dimensions of stimuli (e.g., (Eitam et al., 2009; Eitam, Glicksohn, et al., 2013; Gaschler et al., 2012; Norman et al., 2016), but see Guo et al., 2013).

Task relevance has also been shown to modulate a seemingly more motor related phenomenon termed 'automatic imitation' (Hemed, Mark-Tavger, et al., 2022) which has been recently shown to be highly sensitive to the task relevance of the response inducing stimuli/the responses they induce.

Another study tested the effect of task relevance on what is considered to be a gold-standard index of the sensorimotor 'system' – sensorimotor adaptation (Parvin et al., 2022). This study found that task-irrelevant feedback was highly limited in modulating movement. Specifically, the task-relevance of own-action feedback was manipulated by presenting participants with a reaching task in which participants controlled a cursor moving on the screen, whose location was rotated (e.g., by a 45° clockwise perturbation) as they reached for a target. In one experiment, the cursor was accompanied by two additional *irrelevant* distractor cursors, colored in three distinct colors. Participants were asked to reach towards a target with the central cursor and ignore the two flanking cursors, moving along with the relevant cursor at perturbations of ±45°. A control group performed the same task but only viewed a single (relevant) cursor. When tested on a washout block (i.e., when the rotation-perturbation was removed), the irrelevant-cursors group showed less residual hand angle deviation, meaning that their sensorimotor adaptation process was attenuated by the irrelevant cursors in spite of the prolonged training phase under visuomotor perturbation. In an additional control experiment participants were asked during the perturbation phase to hit the target not with the middle (previously relevant) cursor, but either with the one opposed to the perturbation (thus ignoring it altogether), or the one compatible with it (thus over-compensating for it). The Over-compensation condition displayed increasing adaptation, but there was no adaptation at all on the 'Ignore' group. Thus, it can be argued that the attenuated adaptation observed in the former experiment was not due to the mere presence of the distractor cursors, but due to a process which selectively filters in information coming from the perturbation applied to the relevant cursor, but not perfectly so. A third experiment was aimed at quantifying the relation between the selective filtering and task-relevance of the relevance cursor. Participants viewed two targets placed at ±45° relative to 12 O'clock and when reaching – two matching cursors moved towards the targets. They were instructed that only one cursor-target pair is relevant (e.g., the right-hand one) and they should hit the relevant target with the relevant cursor. The location of each target was jittered independently on each trial by ±10°. The researchers tested the trial-to-trial changes in hand angle direction, showing it was influenced mainly by the previous jitter from the relevant target, and this influence was correlated with the strength of adaptation. The study by Parvin (et al., 2022) shows considerable filtering of irrelevant feedback at least when sensorimotor adaptation is concerned, but it is pending on how the subject actually treats the cursor as relevant.

On the other hand, other evidence suggests that the effect of task relevance may be limited to specific conditions and hence, to a potentially limited set of processes depending on 'executive' functions (contents of working memory) and/or the activation of semantic representations (Eitam & Higgins, 2010).

For example, returning to the goal standard index of sensorimotor processes – sensorimotor adaptation. It has been repeatedly shown that explicitly irrelevant feedback robustly modifies adaption, although uninformative to actual performance. The first set of evidence comes from studies using the clamped feedback method. In this method, participants reach towards a target on a screen using a cursor, while the movement of their hand is obscured. Participants are asked to aim straight to the target but are made well aware of the fact that the feedback is bogus in the sense that independently of their aiming direction, it will move in a predetermined trajectory relative to the target (e.g., 3° to the left of the target). There are multiple reports (e.g., Kim et al., 2018, 2019; Morehead et al., 2017), of how invariant clamp feedback leads to accumulation of very large deviations in movement aiming angle, opposed to the clamp (15-20°). Thus, it can be argued that performance-irrelevant feedback generates behavior should counter the "offset".

Second, some studies focused on how action-effects from one task influence performance on a secondary task, introduced after each trial. This is used as proxy for testing whether irrelevant feedback is processed or has any downstream effects on actions.

A recent study (Schaaf et al., 2022a) shows that action-effects are monitored independently from actual performance. Participants were asked to respond on the basis of the color of a cue shown on the screen. Their responses caused the appearance of a larger color patch, which matched the color of the cue on 75% of the trials, but otherwise was randomly colored. The color of the action-effect patch itself was not informative for performing the actual task. Following the appearance of the action-effect, a letter stimulus appeared on the screen and participants were required to respond to it using another set of keys. The authors found that following color-mismatch "perturbation" trials – response time was elevated (independently of performance errors on the primary task). Thus, although the action-effect (the patch color) was seemingly unrelated to performance in the primary or secondary tasks, it was monitored. One possible critique is that the imperative cue always appeared on the surface of the color patch, hence the space occupied by the color patch was attended to, and hence relevant. However, it does not seem likely that participants were unable to ignore the action-effects just due to their location, given previous findings that people can process relevant information and filter out irrelevant information which appear at the same location (Chen et al., 2016; Eitam, Glicksohn, et al., 2013).

Two other sources support the conjecture that irrelevant action-effects are unintentionally processed. First, people make anticipatory saccades towards the *predicted* location of action-effects which are comparable on both cued-choice tasks and on free-choice tasks. While on the former action-effects may serve as performance (hence task-relevant) feedback, on the latter, the action's effect is dissociable from actual performance (Pfeuffer et al., 2022).

Finally, in a recent study (Schaaf et al., 2022b) participants performed a dual-task procedure, similar to the one described below on the first task, they were asked to press a key based on a cue, which caused a stimulus to appear on the screen. On the primary task the instructions were either “add a puzzle piece to the [left/right] side by pressing the [left/right] key” in the relevant condition, or “press the [left/right] key which will produce a task-irrelevant [left/right] puzzle piece” in the irrelevant condition. The action-effect appeared either in a spatially compatible location (e.g., on the right-hand side of the screen when responding using the right-hand key) or in a spatially-incompatible location (e.g., on the left-hand side of the screen when responding using the right-hand key). On the secondary task participants were asked to identify a letter stimulus and to respond using one of two keys. Generally, in terms of performance on the secondary task, it was found that spatially-incompatible or unpredictable-feedback occurrences (vs. compatible or predictable feedback) reduced performance either in response speed or error rate for both conditions. Thus, it seems that monitoring of action-effects occurs both when feedback is relevant (effect-instructions) or irrelevant (response-instructions). Asking participants to count the number of unexpected feedback occurrences (and thus making it relevant) did not change this pattern, corroborating the findings from our study (e.g., Experiment 1 vs. Experiment 2).

Control relevance motivates motor behavior independently of value relevance.

Can these, seemingly conflicting, bodies of evidence be accommodated? As alluded to above, the framework called *Relevance Of Activated Representations* (*ROAR*, in short; (Eitam & Higgins, 2010)) specifies that as we go about our lives perceiving different stimuli, most stimuli are processed, but only some are selected due to their current (or chronic) relevance to sufficiently activate mental representations and influence information processing and action.

This simple framework was conductive to the discovery of the effects that task relevance has on seemingly 'automatic' processes such as “irrelevance induced blindness” – the difficulty in (and sometimes inability of) reporting task irrelevant features of attended stimuli (Eitam et al., 2015; Eitam, Yeshurun, et al., 2013; see also Chen et al., 2016;). Another example, is 'automatic imitation' (Brass et al., 2000) – the unintentional imitation of modelled movements – that was recently shown to be in fact, highly controlled by the task relevance of the to-be-imitated movements (Hemed, Mark-Tavger, et al., 2022).

Yet in hindsight, the key contribution of the framework may have been in applying Higgins' (2011) insight regarding motivated behavior to information processing and by doing so, broadly outlining which forms of information can be motivating.

Specifically, the current study demonstrates *both* (a) that the reward value associated with responding and the task relevance of the information – two forms of 'value relevance' in ROAR – do not motivate motor behavior (i.e., do not modulate RSP) and doing so (b) vindicates another source of relevance specified by Eitam & Higgins (2010) – ‘control relevance’.

Control relevance is described by Eitam & Higgins (2014) as "[implementing]… the mind's necessity to know what can be successfully done/affected/controlled by the organism (or by other agents) in the environment." (P.141) and returning to the apparently conflicting results reviewed above – we propose that they may reflect a mixture of processes with different sensitivity to two types of relevance – value relevance (e.g., rewards, task instructions) and control relevance. We hypothesize that control relevance motivates – but it does so directly only within the motor system and does so 'non-conceptually'—that is through dedicated mechanisms such as the 'comparator model'. Conversely, value relevance can affect motor behavior but only indirectly so, through the 'explicit' or decisional control of movement (e.g., re-aiming; investment of more effort). We leave open the possibility that control relevance can modulate behavior *indirectly* out of the motor-system, for example by modifying the outcome-relevance of information (Parvin et al., 2022). Thus, our empirical prediction is that experiments in which the conditions for information to be control relevant (immediate and predictable sensations following own-actions) will fail to show sensitivity to manipulations of task and outcome-based relevance (i.e., value relevance) because, under these conditions, control-relevance is sufficient to motivate behavior.

A second empirical prediction stemming from the above hypothesis is that when the above conditions for information to be control-relevant are not met, manipulations of value relevance may influence behavior but will do so through non-motor (i.e., decisional) processes.

Not All Predictions Are Equal

Notably, control relevance is also differentiated from the third source of relevance – truth-relevance. Truth relevance is described as "[implementing]…the mind's need to know what is really "out there." If the mind is expecting something (see Bruner's "perceptual readiness"), once a sufficiently similar signal appears, the corresponding representation will be more strongly activated because of its high truth relevance." (Eitam & Higgins, 2014; Bruner, 1957). We find this interesting because in contrast with influential theories such as 'predictive coding', the notion of motivational relevance of information as described in ROAR differentiates between information that is uniquely truth relevant and that corresponds to a prediction that is 'cognitive' or 'conceptual' to mean driven by semantic knowledge, logic, or both and a prediction that is uniquely control relevant and corresponds to a prediction that stems from an 'internal model' and which, we argue, is solely sensorimotor and hence, modular and insensitive to semantic knowledge or logic.

Limitations

One limitation of the current study is its inability to pinpoint why we observed differences between the 5-trials group and 10-trials group in their responsiveness to changes in action-effectiveness. Previously we hypothesized that this stems from the more reliable signal (i.e., less dynamic; Hemed, Bakbani Elkayam, et al., 2022) which is found in the 10-trials group, however, this study suggests the opposite conclusion if anything – reinforcement builds up quickly and decays quickly, potentially reflecting a very rapid learning rate of this process. One possibility to probe these differences would be to use some varying cycle-duration, while controlling for noise in the distribution of cycle-durations a participant encounter.

Second, one could argue that our manipulations were not robust enough to change the task-relevance of own-action effects. However, it is not clear whether this holds for Experiment 1 (where feedback consisted of images of cumulative monetary rewards) or Experiment 2 where participants were not told to choose any specific action out of the 4 available responses. This explanation seems even more unlikely for Experiment 3, where participants were given training where they were forced to respond using the “wrong” key and so observe that their responses still yielded the same action-effect, regardless of what they chose to respond, much like used on clamped visual feedback in many sensorimotor adaptation tasks (e.g., Kim et al., 2019).

Finally, the study only examined one behavioral manifestation of reinforcement, namely the speed of responding. Other behavioral measures such as frequency of selecting a specific response (Hemed et al., 2022a; Karsh & Eitam, 2015), could provide a more nuanced view of the effect of relevance on own-action-effects, as these methods probe a higher cognitive or inferential process (Hemed et al., 2022a) . Note though that other behavioral measures are more sensitive to decisional aspects then RT thus a future study should possibly combine latent variables analysis (e.g., drift-diffusion modeling) to uncover subtler implications of changes in the stimuli's task relevance on RSP (possibly by others utilizing our publicly available data).

A less worrying result is the slight variance in the results, as not all the contrasts we tested proved significant, although in the predicted direction. This could be either due to the quality of the data as Experiment 3 was conducted online rather than in person or due to the breadth of manipulations used. However, the pattern of our results is stable and does not change due to screening criteria (see Supplementary Materials). This concern is also amended provided that our recent work (Hemed et al., 2022), included a meta-analysis of the data from eight different experiments using the current task (three out of the eight were included in this paper). The aggregated results of that meta-analysis prove that above all inter-experiment differences there is a robust and significant facilitation effect (for the contrast of *Context* values given $Prior_1$), both for the 10-trials and the 5-trials groups, although the facilitation is slightly stronger for the 5-trials group. Regarding the contrast of trials following No-Feedback (*Prior0*), there is most certainly no facilitation, and most likely no inhibition as well. Thus, across a series of tests, the pattern of our results confirms our hypotheses.

Conclusion

In this work we aimed to gauge whether changes in task- and reward relevance of feedback (the information's 'value relevance') influence reinforcement from sensorimotor predictability. We showed using a varied set of manipulations that increasing or decreasing task-relevance has very little effect on reinforcement of actions. Finding that self-generated feedback is impervious to changes in task relevance opens the door for the notion of 'control relevance' and calls for the exploration of how filtering interacts with the seemingly modular system that is our current best guess as the mechanism underlying RSP.

Additional Information

Data availability statement

All code and data related to this manuscript are available on a public GitHub repository (https://github.com/EitanHemed/patches-papers).

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Author Contributions

Both authors developed the study concept. E.H., and B.E. contributed to the experiments' design. E.H. collected the data and performed data analysis. Both authors wrote the paper and approved the submitted version of the manuscript.

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The current work was not previously published or presented.

Declaration of Conflicting Interests

The authors declared that they had no conflicts of interest with respect to their authorship or the publication of this article.

Research Ethics Statement

The study was approved by the Ethics Committee, Department of Psychology, University of Haifa (Approval No. 425/16 and 465/21).

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