Evaluation of an Action's Effectiveness by the Motor System in a Dynamic Environment: Amended

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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THIS IS A PRE-PRINT AND HAS NOT BEEN PEER-REVIEWED.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 1A AND 2C PROVIDED CONCLUSIVE RESULTS AND ADDITIONAL DATA COLLECTION IS UNDERGOING.

Abstract

An important model for explaining humans' feeling of agency - the 'Comparator model' - draws on ideas used to explain effective motor control. The model describes how our mind evaluates the degree of control over the environment offered by a specific motor program (in short, an action's effectiveness). However, given its current level of specification, the model is at best vague on how (or even whether) the prediction of effectiveness of an action is dynamically updated. To test the issue empirically, our participants performed multiple experimental blocks of a task (reliably shown to measure reinforcement from effectiveness) in which blocks with and without action-effects (or with spatially unpredictable feedback) were interlaced. This design creates repeated objective increases and decreases in effectiveness (quantified as the *n*-trials back probability of receiving sensorimotor-predictable feedback), which participants were unable to report. As previously found, response speed indexed reinforcement from effectiveness. The results suggest that reinforcement from effectiveness is sensitive to both the degree and 'trend' of effectiveness; that is, reinforcement is sensitive to whether it is increasing, decreasing or is unchanged. Given the previous links made between reinforcement from effectiveness and the computation of effectiveness by the motor-system, the results are the first to show an 'online', dynamic and complex sensitivity to a motor-programs' effectiveness that is directly translated to its production. The importance of testing the so called 'sense of agency' in a dynamic environment and the implications of the current findings for a dominant model of the sense of agency are discussed.

Introduction

It is important for goal-directed agents to act on their environment effectively to achieve both simple goals (e.g., locomotion, food attainment and consumption) and complex goals (such as social, or professional interaction). Permanently limited resources (energy, time) make the selection and execution of effective actions even more important. In fact, various scholars suggested that mere ‘effectiveness’ – (predictably) controlling the environment – is rewarding, even if the outcome of the effective action is valence-neutral (Higgins, 2011; Hull, 1943; Wen et al., 2018; White, 1959); potentially, because selecting effective actions is adaptive (Skinner, 1953).

Indeed, infants are motivated to select and execute motor actions that effectively manipulate their environment even when no relevant tangible reward is gained (e.g., food, warmth, attention from care-takers; Angulo-Kinzler, 2001; Hauf & Aschersleben, 2008; Hauf, Elsner, & Aschersleben, 2004; Rovee & Rovee, 1969; Verschoor, Weidema, Biro, & Hommel, 2010; Watanabe & Taga, 2006, 2011).

Recently, speculation regarding the reinforcing effects of such effectiveness in adults received substantial empirical support. Using various paradigms, it was demonstrated that if responses merely lead to a perceptual change in the environment an increase in both the frequency of their selection and the speed of their execution is observed (Eitam et al., 2013; Hemed et al., 2022; Karsh & Eitam, 2015; Karsh et al., 2016, 2020; Penton et al., 2018; Tanaka et al., 2021; Wen & Haggard, 2018). This finding was dubbed ‘motivation from control’.

The 'Comparator' model

In line with the recent literature on what is called 'implicit' or indirect measures of the feeling of agency such as Sensory attenuation (Blakemore, Wolpert, & Frith, 1998) and Intentional binding (Haggard et al., 2002), motivation from control was explained using the Comparator model (Blakemore, Frith, & Wolpert, 1999).

The model specifies that once a voluntary motor plan is selected and executed, a prediction of the sensory effects of the planned movement is generated and then compared with the actual received feedback. If the comparison results in a match (or lack of discrepancy) between the two, the comparator generates a feeling of control or agency that contributes to the judgment of authorship over the effect of the action (Blakemore, Wolpert, & Frith, 2000; Blakemore et al., 1998; Blakemore, Wolpert, & Frith, 2002; Synofzik, Vosgerau, & Newen, 2008)[1]. In contrast, minute spatial (Blakemore et al., 1999) or temporal discrepancies (Blakemore et al., 1999; Blakemore, Wolpert, & Frith, 1999), or weak action-effect contingency (Blakemore et al., 1999; Shergill, Whiet, Joyce, Bays, Wolpert & Frith., 2013) will lead to a mismatch (Sensory prediction error, SPE) and arguably little or no feeling of agency or control over the effect of the action will be generated.

[1] Due to research on effective behavior in infants mentioned above (e.g., Watanabe & Taga, 2006) and robot simulations of ‘agent-babies’ (Kelso & Fuchs, 2016; Zaadnoordijk et al., 2018, 2016) it has been recently argued, that the lack of a sensory prediction error (SPE) is not a sufficient condition for agency and causal inference is required (Zaadnoordijk et al., 2019).
At any rate, in the current study we are not interested in the person-level ‘judgement of agency’ (Gallagher, 2012; Synofzik et al., 2008), but in ‘feeling of agency’, an indirect, implicit measure which stems from the lack of-SPE. Elsewhere we suggest a comprehensive differentiation between the two regarding ‘motivation from control’ (Hemed et al., 2022; Karsh & Eitam, 2015; Karsh & Eitam, 2015), but it is beyond the scope of the current paper.

The dynamic evaluation of a motor program's effectiveness

Slightly rephrased, *the motor-based evaluation of agency involves the evaluation of a motor program's effectiveness in influencing the environment*. Linking this to our previous work cited above, this phrasing supports the conclusion that motor programs thus evaluated as being effective in affecting the environment are more likely to be (re)selected or rapidly executed (For a more elaborate discussion on the judgement of an action's effectiveness see Hemed et al., 2022).

One limitation of the comparator model (the theoretical cornerstone for much of the research on agency) - is that in its current form, it cannot address how (or even whether) the comparator system would handle changes in the degree to which an action affects the environment – when an ineffective action turns effective and vice versa. Specifically, the model includes an internal model that generates a prediction to which the actual stimulus is compared.

It has been empirically shown that 'deviant', yet invariant, environmental contingency (temporally lagged and spatially offset from actual movement, e.g. Blakemore et al., 1998; Karsh et al., 2016) decreases indirect markers of agency even after many (even hundreds of) trials. These data may suggest that the 'Comparator' is unable to adapt to changes in a motor action's effectiveness (here, changes from a strong prior belief affecting the process due to substantial experience). This, although possible, is surprising as it would substantially reduce its functionality. However, there are recent arguments for incorporation of less flexible ("stubborn") processes into frameworks of cognition, as they may end up being actually adaptive to the organism (Dallmann, 2017; Yon et al., 2019).

Somewhat in contrast, a recent study (Kilteni et al., 2019) have shown that an indirect measure of agency – Sensory Attenuation – can be reversed. Specifically, that lagged self-administered haptic stimulation (‘tickle’) is also attenuated with sufficient experience (in contrast with the finding of Sensory Attenuation for only non-lagged self-produced stimuli). In the relevant conditions, participants were exposed to lagged feedback (‘self-tickling’) for hundreds of trials. Following the long training, they showed less-attenuation for immediate stimulation and heightened-attenuation of lagged stimulation. Therefore, the ‘Comparator’ may have some flexibility regarding adapting to novel environments, by adjusting the predicted feedback (after enough learning).

There are reasons beyond intuition to think that the evaluation of a motor program's effectiveness through the 'comparator' may be sensitive to on-line changes in effectiveness. First, the ‘Comparator model’ (as a theory) is one of a class of forward models of motor control. Generally, such models have been suggested to bias selection of actions to those that induce small SPE – “If the prediction of one of the forward models closely matches the actual sensory feedback, then its paired controller will be selected and used to determine subsequent motor commands” (Wolpert & Flanagan, 2001, p. 731). Furthermore, the continuous comparison between predicted and actual sensory outcomes of a response is a general principle in learning of a motor task from SPE (Wolpert, Diedrichsen, & Flanagan, 2011).

Second, published studies, relying on the 'Comparator' as a primary theoretical framework, did not examine the responsiveness of the system to temporal dynamics, but rather treated it as being constant in time (e.g., manipulating the probability of an action leading to some result within separate blocks or between subjects) by averaging over experimental trials (Buehner & Humphreys, 2009; Eitam et al., 2013; Engbert, Wohlschläger, & Haggard, 2008;

Haggard & Clark, 2003; Haggard, Clark, & Kalogeras, 2002b; Hon, Poh, & Soon, 2013; Karsh & Eitam, 2015a; Karsh et al., 2016; Longo & Haggard, 2009; Moore & Haggard, 2008; Moore, Lagnado, Deal, & Haggard, 2009; Moore, Ruge, Wenke, Rothwell, & Haggard, 2010; Moore, Wegner, & Haggard, 2009; Obhi & Hall, 2011; Sato, 2009; Tsakiris, Prabhu, & Haggard, 2006). Such an experimental design is inadequate for testing dynamic updating of the internal model and hence, less informative about whether such updating occurs, and if so, what is its nature.

Third, previous work on the sense of agency tended to equate the constructs with their measurement, often without sufficient justification (for similar cases in cognitive and social contexts see De Houwer & Moors, 2015). The problem increases when so called 'implicit measures' are used as these often lack face validity and are based on correlations with other measures (De Houwer et al., 2009). The dominant example in the current literature on the sense of agency is the intriguing phenomenon of 'Intentional binding' (Haggard et al., 2002). This effect, dubbed an 'implicit measure of agency’, has become a de-facto gold-standard for indirectly measuring agency although its specificity to agency has been repeatedly challenged (Buehner, 2012) and recently finding of intentional binding without intentional action (Suzuki et al., 2019) and its theoretical relationship to the comparator model or other models of the sense of agency has never been clearly stated.

A second prominent indirect measure of the ‘Sense of agency’ that was in fact well linked theoretically to the comparator model - sensory attenuation - provided inconsistent results in recent years. There are reports of some failing to find attenuation (Schwarz et al., 2017), some finding amplification of self-generated effects (Yon et al., 2018; Yon & Press, 2017) and yet others finding action-effects leading to modulation of perception of some features in a stimuli but not others (Dogge et al., 2018). These mixed findings led some to claim that sensory attenuation may represent an independent phenomenon unrelated to the Sense of Agency (Weller et al., 2017).

Beyond running the risk of expanding a theory on the unverified assumption that the above phenomena tap into the same computation, another risk here is that the notion of a motor-based computation of 'agency' will be rejected with the empirical phenomena that it has become equated with, regardless of its theoretical merit.

The current study – a retraction and republication of Hemed et al., (2020)

The current study is a republication of a published paper which was recently retracted on our initiative (Hemed et al., 2020). About three years post-publication, while analyzing the data for a second manuscript which is based on a similar task (Hemed & Eitam, 2022), we uncovered a confound in the analysis of our data,

The confound resulted primarily from the unwanted influence of switching from attentional probes back to our main task . In the current version we correct the record by:

(1) reporting the results of the two published experiments after filtering out the vast majority of the affected trials.

(2) report a 3rd unpublished study that adds support to the weaker results found in the newly filtered Experiment 2.

(3) report the results of the replication of the two published studies which were run without the probes.

(4) report the results of a meta-analysis over all eight experiments that used the design reported in the original, now retracted, paper and a recent one (Hemed & Eitam 2022).

Like the retracted paper the current study aims to overcome the lacuna in the literature which we described above – treating the mind's estimations of the degree of control it has over the environment (arguably, a form of 'sensing agency') as a static phenomenon, rather than studying how it unfolds in time. Here we examine whether and how the effectiveness of a response is evaluated in a dynamic environment using the recently discovered reinforcement from sensorimotor predictability (RSP; Hemed et al., 2022; Noam Karsh et al., 2016; Karsh & Eitam, 2015; Penton et al., 2018; Tanaka et al., 2021; Wen et al., 2018).

To do so, we used a paradigm first introduced by Eitam et al. (2013). In that study, participants viewed a colored circle descending the screen from one of several horizontal starting positions and were asked to respond with a keypress to the matching location. Depending on experimental condition, a correct response led either to a sudden ‘flash’ of the descending circle (consult Figure 1B in the current paper), to no ‘flash’, or other forms of visual feedback. Participants’ responses were faster in the 'flash' condition (by ~40ms, a Cohen’s d of ~0.8 (Bakbani-Elkayam et al., 2019; Eitam et al., 2013; Karsh et al., 2016, 2020; Tanaka et al., 2021).

Predictions

In an unpublished experiment ran in our lab (Hemed, Karsh, & Eitam, 2018), subjects performed a task very similar to the one used by Eitam et al., 2013, but the occurrence of a ‘flash’ action-effect was determined probabilistically on each trial (with feedback probability ranging 0.15 and 0.9). Beyond estimating the effect of the overall probability (i.e., over all trials) of feedback on response speed, we tested several possible windows for estimating effectiveness (i.e., the number of trials-back for which effects are aggregated; 1,3,5,7 and 10-trials back). We found that modeling the number of feedback occurrences in trials N-1 *to n-3* led to the best prediction of response time on trial *n*, (over and above overall probability of feedback – see between subject conditions below). Therefore, in the current study we predicted that participants’ response speed will track the effectiveness of the actions of the participant, computed as a running average with a window size of roughly 3 trials[2].

Experiment 1a – the re-analysis of Experiment 1 in Hemed et al., 2020

Methods

The experiment was pre-registered using the Open Science Framework site (Hemed, Bakbani-Elkayam, et al., 2018). We planned on running 84 participants, but after finishing collecting the sample, we added subjects until the end of the term, resulting in 127 subjects. Since the pattern of results is identical yet the quality of estimation is naturally better in the larger sample, we collapsed the two samples.

Participants

A total of 127 participants were recruited [77% Women, Ages 18-44, M=24.72, SD = 4.47], via the Psychology department's online registration system. Data from two participants (in the pre-registered sample) was lost due to technical problems and two additional subjects were run instead.

Apparatus

The experiment was programmed in PsychoPy2 Version 1.83 (Peirce et al., 2019) and ran on HP COMPAQ ELITE 8200 MT (for the pre-registered sample) and HP Z240 PCs (for the

[2] Using this time window was data-driven, as given the current limitations of the comparator and lack of relevant previous literature, we had no direct theoretical argument that could a-priori constrain the size of the window. As detailed below, the time window may not be the perfect way of parsing the data, since the most interesting part of the 'action' occurs in the extreme values of the possible levels of effectiveness – when participants begin to 'gain' or 'lose' effectiveness – as our analysis in the current work shows.

additional sample). Stimuli were presented on a BENQ XL2420T screen, set to 120Hz. Responses were collected using a standard PC keyboard.

Design

The task is a modified version of the Effect-Motivation task (Eitam et al., 2013), but whether correct responses led to an effect changed every short groups of trails (cycles which are 5 or 10 trials long), as detailed below. A trial is illustrated in Figure 1B-C. Sitting distance from the monitor was not controlled for but was around 60CM.

Besides task trials (described below), 16% (70/440 trials) of the trials were surprise probe trials intended to discourage subjects from counting trials throughout the experiment anticipating the switch between cycles of Feedback and No-Feedback trials and removing any uncertainty in the prediction of when they will receive or stop receiving feedback[3]. Additionally, the probes enable us to gauge people's sustained attention (for a similar strategy see, Karsh & Eitam, 2015a). On probe trials a yellow triangle (vertex = ~4.2 cm) appeared in the middle of the experimental scene (Figure 1C), to which the subject was asked to respond by pressing the spacebar (i.e., a different key), using their thumbs. To preclude the possibility that the probes' disappearance will be deemed as an action-effect, the probe remained on the screen for 1000ms regardless of response (with a 550ms ITI). Their interlacing within the task trials was identical for all participants, predetermined by sampling 70 random unique trial numbers.

[3] As part of the debriefing questionnaire, participants were probed about their knowledge on the regularity of feedback occurrences. As we intended, none reported the actual regularity of feedback occurrences. Most reported that feedback was either random, depended on response speed, related to the probed trials or followed some other complex regularity which they did not understand. Only 4 participants (~3%) mentioned that there was a fixed number of trials in each block but did not indicate the number.

On task trials (84%, 370/440 trials) a colored circle (1.4cm in diameter) appeared in one of four possible horizontal locations and descended vertically at a rate of ~13.3 cm/sec until traversing the full length of 850 milliseconds response window; participants were asked to respond with their right or left index or middle fingers (spatially and color coded) to the cue's location. On Feedback trials, given a correct response, the colored circle 'flashed' (i.e., turned white for 100ms and then disappeared), see Figure 1C. In No-feedback trials (Figure 1B), no feedback was given - regardless of accuracy of responding ; with the cue descending uninterrupted for the full duration (850ms) and length of the response window. A fixed 700ms-long ITI followed each task trial.

Every fixed number of trials (see below), the streaks of Feedback and No-Feedback trials alternated, dotted with occasional probes. Block ('streak') length, 5 or 10 trials long, was manipulated between participants, with order of blocks (Feedback/No-Feedback block first) counterbalanced between subjects. For all blocks, a correct response on un-probed trials *either* led to no feedback, or to a 'flash' (Figure 1B-C). The ending of one block and the beginning of the next was not signaled to the participant in any way. See Figure 1D below for a graphical depiction of a number of sequential blocks.

It should be highlighted that even though participants received either short (5) or long (10) blocks of Feedback and No-Feedback trials, there was an equal number of Feedback trials in both between-subject conditions – meaning the probability of feedback was the same – exactly 50% of task trials.

Procedure

Upon arrival to the lab, participants gave their informed consent to participate in the study, were seated in a dimly lit room, instructed on the nature of task and probe trials and the appropriate responses and asked to respond as accurately and quickly as possible on all trials. Then, they completed 440 trials (as described above) with no breaks and continued to fill self-report questionnaires – one related to their feeling of agency in the task and one which evaluates the chronic sense of agency (Tapal et al., 2017), followed by a demographics' questionnaire.

<u>Capturing the Dynamics of Effectiveness</u>

Data preparation plan

The crux of our method of our analysis focused on mitigating the confound we introduced in Hemed, 2020.

The confound

In that paper, we unwittingly included task trials which immediately followed attentional probe trials and post-error trials (note that at no point did we analyze probed-trials or erroneous-trials). As these trials de-facto did not follow an action-effect (by design, a response to a probe did not elicit an effect) , they were coded as trials following a no-feedback trial.

This led to the unfortunate outcome that the cost of task-switching and post-error slowing largely inflated the difference between trials immediately following an action-effect, and trials which did not.

The confounded pattern

As described above, the experiment alternated between predicted, unperturbed feedback and no feedback (or spatially-perturbed feedback, see Experiment 2). The cycles alternated every fixed number of trials. To test the responsiveness of response speed to recent changes in feedback, we contrasted two distinct bins of trials which followed a consecutive series of no feedback occurrences (0 action-effects occurrences on trials N-4, N-3 and N-2) and trials which followed a series of consecutive feedback occurrences (action-effects on trials N-4, N-3 and N-2). We repeated this contrast four times on each experiment - independently for each combination of experimental group (5 or 10 trials cycles), and trial N-1 (feedback or no-feedback).

We uncovered a distinct pattern, which reliably replicated (Experiment 2), and practically on each other replication of the task (see meta-analysis section in GD). The two patterns can be described briefly as follows. Once participants transitioned to a feedback cycle and 'accumulated' feedback occurrences, they responded progressively faster, independently of the duration of feedback cycles participants completed (either 5 or 10 trials), with medium-to-strong effect sizes (Cohen's d of ~0.4 and ~0.6, for the 5 and 10 trials respectively) with conclusive Bayes factors. However, the pattern was different between the two groups once participants stopped receiving feedback occurrences (mostly due to feedback cycles ending, but also due to post-error and post-probe trials). For the 5-trials groups, as the sum of recent feedback occurrences grew smaller, there was no change in response speed, and it was set back to baseline until feedback occurrences returned once again (Cohen's d ~0.05, with Bayesian support for the null). For the 10-trials group there was sudden and strong slowing down of response speed once feedback stopped (Cohen's d ~0.5, with conclusive Bayesian factors support), and during the next couple of trials response speed sped up once again until converging to a baseline similar to that of the 5-trials group.

As will become clear from the current report. The confound boosted one pattern (facilitation following feedback accumulation), which, was the one we predicted and pre-registered and created the second unexpected yet stable pattern (the difference between the baseline response speed of the 5-trials group and the inhibition shown in the 10-trials group). Again, although the latter pattern was (and is) not predicted by our model we became interested in it and hence pre-registered and replicated it, first on the previous version of this work (Hemed et al., 2020; Experiment 2), and then several more times. Evidently the confounded pattern was quite stable, as evident from a Meta-Analysis involving all five studies from the current work, and three from a recent one (Hemed & Eitam, 2022). The meta-analysis for both the confounded and amended processing pipelines are available on the online supplementary materials repository of the current work.

The outcome of controlling and eliminating the confound

To bode the results of the current report. The theoretically predicted pattern of facilitation is detected (if somewhat less robustly) when the confound is controlled (Experiments 1-3) or not introduced in the first place (Experiments 1b and 2c) but the (unexpected) pattern of slowing down after not receiving an effect but only given substantial experience with receiving effects disappears. Next we describe in detail the preprocessing procedure which allowed us to analyze the dynamic nature of the task while dealing with the previous confound.

Amended preprocessing (Experiments 1a and 2a-2b)

Experiments 1a and 2a-2b included attentional probes and thus their preprocessing was move elaborate. However, the preprocessing was crucial to avoid the disturbance caused by switching back from the attentional probes from biasing the reported pattern of data, as before.

Preprocessing included two main stages as depicted in Figure 1D-E – (a) binning based on feedback received on previous trials and (b) invalid trials removal .

On the first stage, we applied the following steps to bin trials based on their recent feedback occurrences.

1. For each participant, the sequential series of all task-trials with correct responses was obtained and trials were labeled as either being in a feedback cycle or a no-feedback cycle.
2. Next, we assigned each of these trials the value of *Prior* (feedback on trial N-1)– based on whether on the absence (0) or presence (1) of feedback on the most recent correct-response task trial. Note that for Experiments 2a-2c, "absence" of feedback is spatially-perturbed feedback.
3. Finally, we assigned for each trial a value of *Context* (0, 1, 2, or 3) – the sum of feedback occurrences on trials N-4, N-3 and N-2 when considering only correct-response task trials.

The usage of the two different values (for prior and separately, for context) enables us to capture the dynamic nature of the task, as *Prior* represented immediate effectiveness (on Trial N-1) and *Context* captures the change in effectiveness incorporating less recent 'history'. Note that these steps skip probed trials and incorrect-response (or omissions) on task trials. Even though a task following a probed trial should have a *Prior* value of 0, because there was no-feedback on the previous trial (and this was our previous assumption) – the interruption due to task switching inflates the effect of feedback-absence (i.e., $Prior_0$).

Relatedly, to this end we conducted experiments 1b and 2c, in which no attentional probes were used. The preprocessing for Experiments 1b and 2c was identical, except that only incorrect-response task trials and omissions were skipped, simply because there were no attentional probes.

On the second stage of preprocessing, we marked trials as invalid (i.e., trials that should not be analyzed), based on several criteria:

1. Task trials with no correct response (i.e., response omission or incorrect response), and trials immediately following them (removing post-error slowing).
2. Probed trials (regardless of response accuracy), and the trials immediately following them (removing the effects of task switching).
3. Task trials with a correct response, and extremely slow or fast response times(based on previous studies). While we did not analyze extremely fast (<100MS) or slow (>750MS) correct responses which accounted for ~1-2% of all responses on each of the experiments (as specified below), they were not considered as incorrect-responses (as they, appropriately, included an action-effect based on the type of feedback cycle they were in). Thus, trials immediately following an outlier due to slow or fast responding were not removed.

Note that the several trials per participant that could not have a defined *Prior* and *Context* values (e.g., the first trial on the session) we also not analyzed.

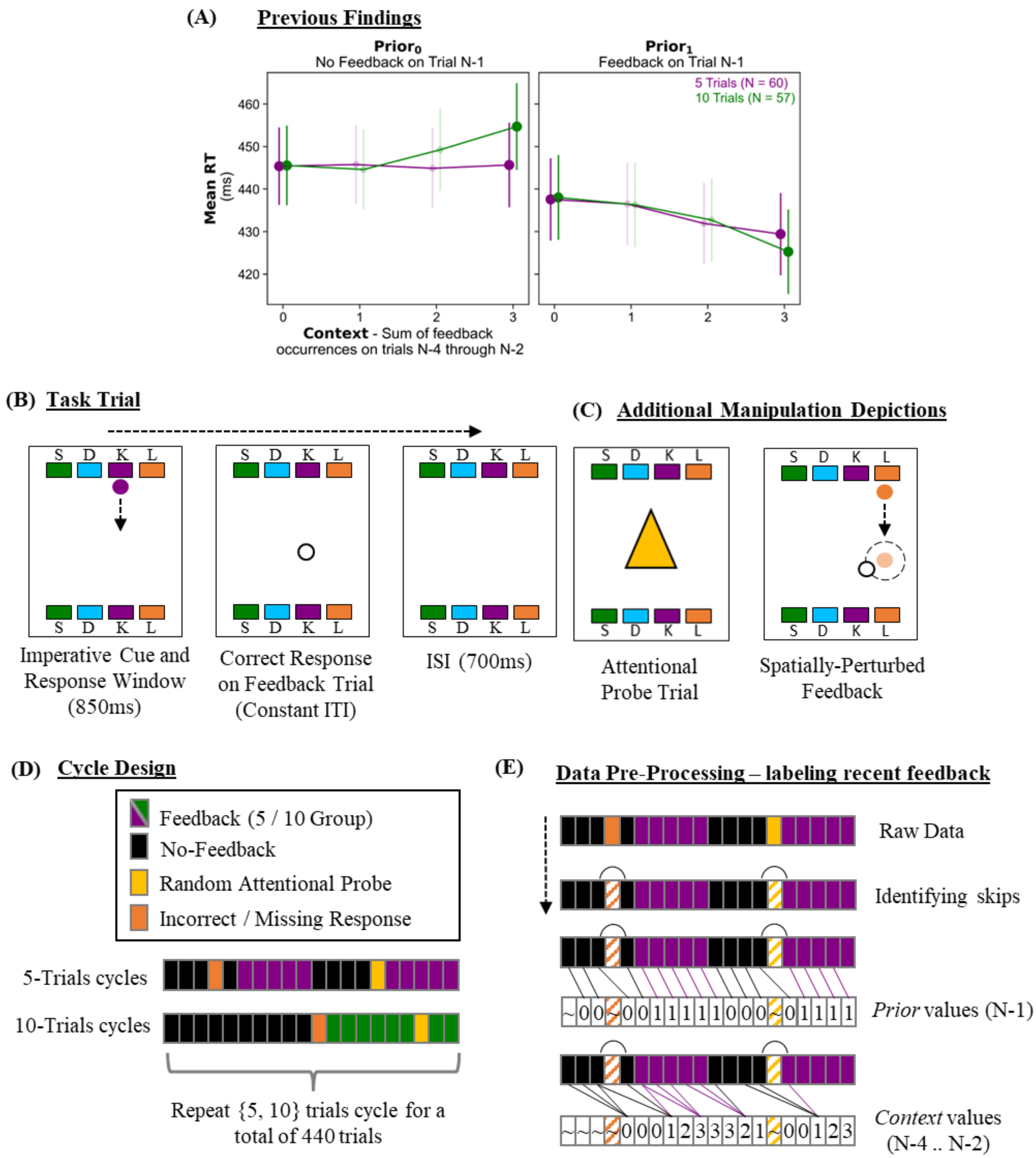


Figure 1: General design and preprocessing.(A) Results from Experiment 1 on Hemed et al., (2020), using the confounded preprocessing. (B) Participants' correct responses on feedback trials caused an imperative cue to flash and disappear. (C) On attentional probe trials, an out-of-set response was required; In the Spatial-Perturbation manipulations (Experiments 2a-2c), the feedback was randomly placed around the cue instead of “No-Feedback”. (D) Pending experimental group, feedback and no-feedback were cycled either every 5 or 10 trials repeatedly. (E) 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).

Data analysis plan

Statistical modeling was the same for all experiments reported here, and is described in detail here. Modelling included two approaches. First, a 2 X 2 X 4 repeated measures ANOVA which tested the joint effects of Cycle Duration (5 or 10 trials), Feedback on prior trial N-1 (Feedback, No-Feedback) and Context of feedback occurrences on trials *n-4* through *n-2* (0, 1, 2, 3) on response speed. The ANOVA was run with Cycle Duration as a between subject factor, The effect of Prior Feedback and Context as Within-Subject Factors .

Following the ANOVA, we used pairwise contrasts to further quantify the pattern of change in response speed –accompanying a frequentist t-test with a Bayesian t-test. In our previous work we converged on a method of analysis of the dynamic changes in action effectiveness, by comparing the difference in response time between the two extreme levels of *Context* (i.e., $Context_3$ vs. $Context_0$), depending on whether participants just received feedback on Trial N-1 ($Prior_1$) or stopped receiving feedback ($Prior_0$). Thus, each experiment was analyzed using four contrasts, two for each of the Cycle-Duration groups.

A brief description of the results reported in Hemed et al 2020

As stated above, in (Hemed et al., 2020), our (confounded) data from Experiment 1 led us to hypothesize a different pattern for the 5-trials and 10-trials groups. Orthogonally, our theory predicts that when control accumulates ($Context_3$ vs, $Context_{0,}$ given $Prior_1$), response speed will facilitate, and so was found for the 5-trials and 10-trials groups.

While our theory does not predict a specific pattern for when feedback is removed (e.g., $Context_3$ vs, $Context_{0,}$ given Prior0), yet we repeatedly found that the RT in the 10-trials group was consistently slower immediately after not receiving feedback but only after 2 and 3 prior effects (context levels 2 and 3) prior to returning to baseline performance on No-Feedback cycles. The 5 trials group in turn did not show this pattern, and immediately went into (baseline) slower response speed when feedback was removed. While this was an interesting and replicable finding, it proved to be driven by and large by the interference from attentional probed trials.

This work comes to amend the confound in our previous publication and on the basis of the amended results of Experiments1a, 2a, and -2b, for (new) experiments 1b and 2c we maintain our theoretically driven prediction that context facilitates the effect of Prior_1 but change our hypothesis regarding the modulation of the influence of the lack of feedback by context. Thus, we predict that no difference will emerge between context_3 and context_0 given Prior_0 (i.e., null) - for both the 5-trials and 10-trials groups.

To facilitate appreciation of the robustness of the pattern we also include a meta-analysis of the standardized effect sizes from each of the pairwise contrasts.

The analysis of Experiments 1a and 2a-2b can be described as exploratory. Although the experiments were pre-registered, the current analysis is brought in order to amend the confound previously introduced. The analysis of Experiments 1b and 2c is confirmatory, given the pre-registration of the novel analysis method.

Results

All statistical analysis was performed using Python's *robusta* package 0.0.4 (Hemed, 2022), plotting was conducted mostly using *Seaborn* (Waskom, 2021)

Data Preparation and Screening

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 (3.95%) or missing (1.39%) responses, task trials with extremely fast (RT < 100, 1.42%) or slow RTs (RT > 750, 0.76%).

Next we removed the data from a total of 10 participants (7.87% out of 127) where their accuracy on task trials was below < 80% (N = 4), accuracy on attentional probe trials was below < 50% (N = 3), or where less than 80% of the trials were valid in terms of either RT, accuracy or both (N = 7). In total, 11.40% 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).

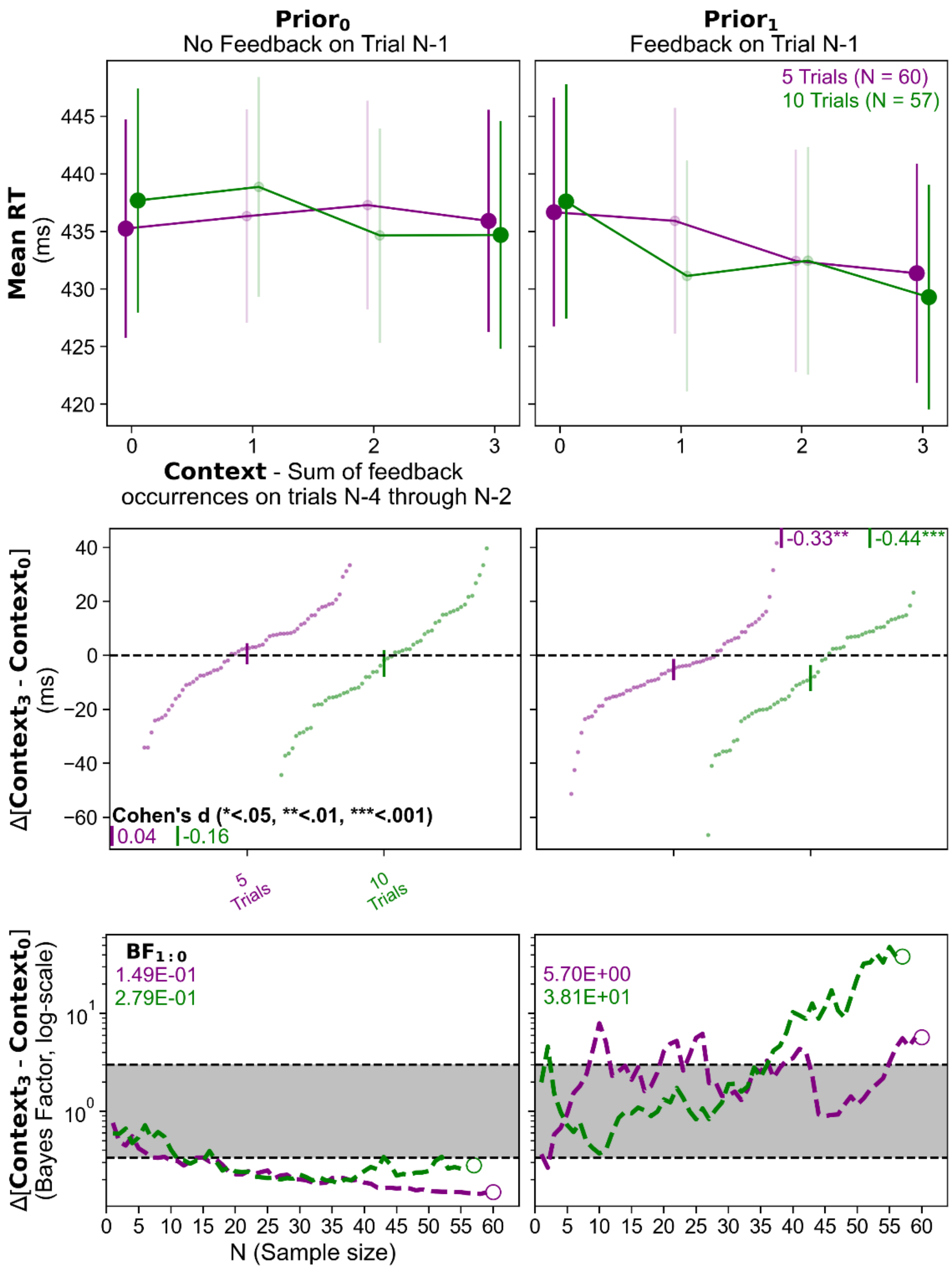


Figure 2, Experiment 1a. The effect of *Prior* (feedback on trial N-1; plot column) is dependent on *Context* – (number of feedback occurrences in trials N-4 through N-2; X-Axis) and Cycle-Duration (Separate Lines). Top – Response Time by Prior, Context and Cycle Duration. Mid - contrasts for RT On *Context*$_3$ – *Context*$_0$, 95%-CI around mean difference. Scatter indicates individual means. Annotation indicates Cohen-d values and significance (* < .05, ** <.01, *<.001). Bottom – Bayes factors obtained using sequential Bayesian t-tests. Shaded area indicates inconclusive result (1/3 < BF < 3). The large point indicates the terminal Bayes factor on the sequential analysis.**

Analysis

The Bayes factor for each of the reported ANOVA terms specifies a model in which it is the sole factor, vs. the null model.

The effect of Feedback on trial N-1 on response time was significant [$F(1, 115) = 10.11$, $p = 0.002$, Partial Eta-Sq. = 0.08], which is a replication of the basic facilitation effect found by our paradigm (Hemed et al., 2022; Karsh et al., 2016, 2020; Tanaka et al., 2021). Also *Context* - the number of Recent Feedback Occurrences was also significant [$F(3, 324) = 3.68$, $p = 0.014$, Partial Eta-Sq. = 0.03] , but Cycle-Duration [$F(1, 115) = 0.01$, $p = 0.927$, Partial Eta-Sq. < 0.01] had no significant influence on response time. *Prior* and *Context* interacted [$F(3, 341) = 2.64$, $p = 0.050$, Partial Eta-Sq. = 0.02], but there was no interaction between *Prior* and *Cycle-Duration* [$F(1, 115) = 0.88$, $p = 0.349$, Partial Eta-Sq. = 0.01] or *Context* and Cycle-Duration [$F(3, 324) = 0.74$, $p = 0.523$, Partial Eta-Sq. = 0.01]. Additionally, there was no significant effect of the 3-way interaction [$F(3, 341) = 1.80$, $p = 0.147$, Partial Eta-Sq. = 0.01].

To explore the pattern described above, two specific contrasts were selected because of their centrality in evaluating the degree to which the evaluation of effectiveness is sensitive to sudden changes (see Hemed et al., 2020 as well as pre-registration here). We contrasted how the 'extreme' *Context* values influence response time, by examining the contrast of [$Context_3 - Context_0$] independently when the trial followed an action-effect or not ($Prior_0$ and $Prior_1$),separately for the 5- and 10-Trials conditions. For the Bayesian analysis we selected an uninformed ('default') prior (Cauchy $\chi_0 = 0$, $\gamma = 0.707$).

When holding feedback on trial N-1 at 1 (i.e. when considering only trials for which feedback was given on the response just before, an uninterrupted streak of 3 previous responses that were followed by feedback ($Context_3$) facilitated responding compared to a uninterrupted streak of responses that were not followed by feedback ($Context_0$) significantly facilitated response speed for both the 5-Trials Cycle Duration group [-5.30 MS (15.83); $t(59) = -2.57$, $p = 0.006$, Cohen's $d = -0.33$, (-0.59, -0.07), BF1:0 5.6963] and the 10-Trials Cycle Duration condition [-8.31 MS (18.62); $t(56) = -3.34$, $p < 0.001$, Cohen's $d = -0.44$, (-0.71, -0.17), BF1:0 38.1150].

Conversely, when holding Feedback on trial N-1 at 0 we found that, 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 5-Trials Cycle Duration group [0.66 MS (15.57); $t(59) = 0.33$, $p = 0.744$, Cohen's $d = 0.04$, (-0.21, 0.30), BF1:0 0.1487], or the 10-Trials Cycle Duration condition [-2.99 MS (18.99); $t(56) = -1.18$, $p = 0.243$, Cohen's $d = -0.16$, (-0.42, 0.11), BF1:0 0.2793].

Thus, as can be seen in figure 2 –**the effect of recent experience with 'being effective' (i.e., with responses leading to effect or not) is contingent on a response continuing to have the same effect.** When it does, the more experience there is with that effect the more (generally) does the same effect facilitates response speed but when it does not have the **same** effect (as Experiments 2a-2c will show) recent experience fails to modulate RT. Note that this pattern is highly consistent with the Reinforcement from Sensorimotor Predictability (RSP) interpretation of RT facilitation as it shows that **predictability is both a sufficient and necessary condition for RT facilitation**, in this paradigm. Note that if it was merely "accumulation" of reinforcement on a mental locus (e.g., on a motor program) then one would expect a linear slope of Rt facilitation by context (potentially accentuated by recency) rather than the complete disappearance of reinforcement when a previously predictable sensorimotor effect becomes unpredictable.

Experiments 1b – Replication of Experiment 1b, excluding attentional probes

Experiment 1b was conducted to investigate whether the attentional probes we have used on previous experiments created a confound within our design. It is possible that the task switching cost from attending to the attentional probes interfered with our participants' performance, although we still discarded trials which included a probed trial on trial N-1. The experiment was pre-registered on the open science framework (https://osf.io/zy4br).

Methods

In Experiments 1b we again used the feedback absence effectiveness degradation as on Experiment 1a. The experiment was conducted online rather than in person, due to COVID-19 restrictions. There were no attentional-probe trials, thus participants performed 440 task-trials, rather than 370 as before.

Participants

We recruited 109 participants. The participants were aged 18.0-45.0 (M = 26.33, SD = 6.23), 74.31% identified as female.

Design

The visual workspace bounded between the two rows of rectangles (see Figure 1B) occupied ⅔'s height of the participant's screen, and ¼ of its width. participants performed a 20 trials training block. During practice trials an on-screen text notification was shown throughout each trial, stating which key is correct on the current trial (e.g., "Press L"). During the practice phase the cue did not descend on the screen but appeared in the middle of each 'column' and remained there for 2S. Otherwise than that all temporal parameters of the experiment were identical to

Experiment 1. Following a slide showing reminders on the task objectives, participants continued to perform an experimental block of 440 trials, as on Experiment 1a. During the experiment each of the four cue locations was repeated 115 times, all in a random order. Throughout the experiment an on-screen counter displayed the percentage of trials completed so far (updated each trial).

Results

Data preparation and screening

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 (7.27%) or missing (5.23%) responses, task trials with extremely fast (RT < 100, 0.23%) or slow RTs (RT > 750, 1.03%). Next we removed the data from a total of 16 participants (14.68% out of 109) where their accuracy on task trials was below < 80% (N = 15), or where less than 80% of the trials were valid in terms of either RT, accuracy or both (N = 16). In total, 21.66% of the task trials were removed, by filtering whole participants' data or individual trials.

Analysis

The effect of Feedback on trial N-1 (*Prior*) on response time was significant [F(1, 91) = 24.13, p = 0.001, Partial Eta-Sq. = 0.21], as well as the effect of Context [F(3, 262) = 4.94, p = 0.003, Partial Eta-Sq. = 0.05]. In contrast, the effect of *Cycle Duration* was not significant [F(1, 91) = 0.45, p = 0.503, Partial Eta-Sq. = 0.01]. As per interactions, the interaction of *Prior* X *Context* [F(3, 253) = 9.94, p = 0.001, Partial Eta-Sq. = 0.10], but not that of *Prior X Cycle Duration* [F(1, 91) = 1.67, p = 0.200, Partial Eta-Sq. = 0.02] *or Context* X *Cycle-Duration* [F(3, 262) = 0.14, p = 0.931, Partial Eta-Sq. < 0.01], as well as the 3-way interaction [F(3, 253) = 1.91, p = 0.133, Partial Eta-Sq. = 0.02].

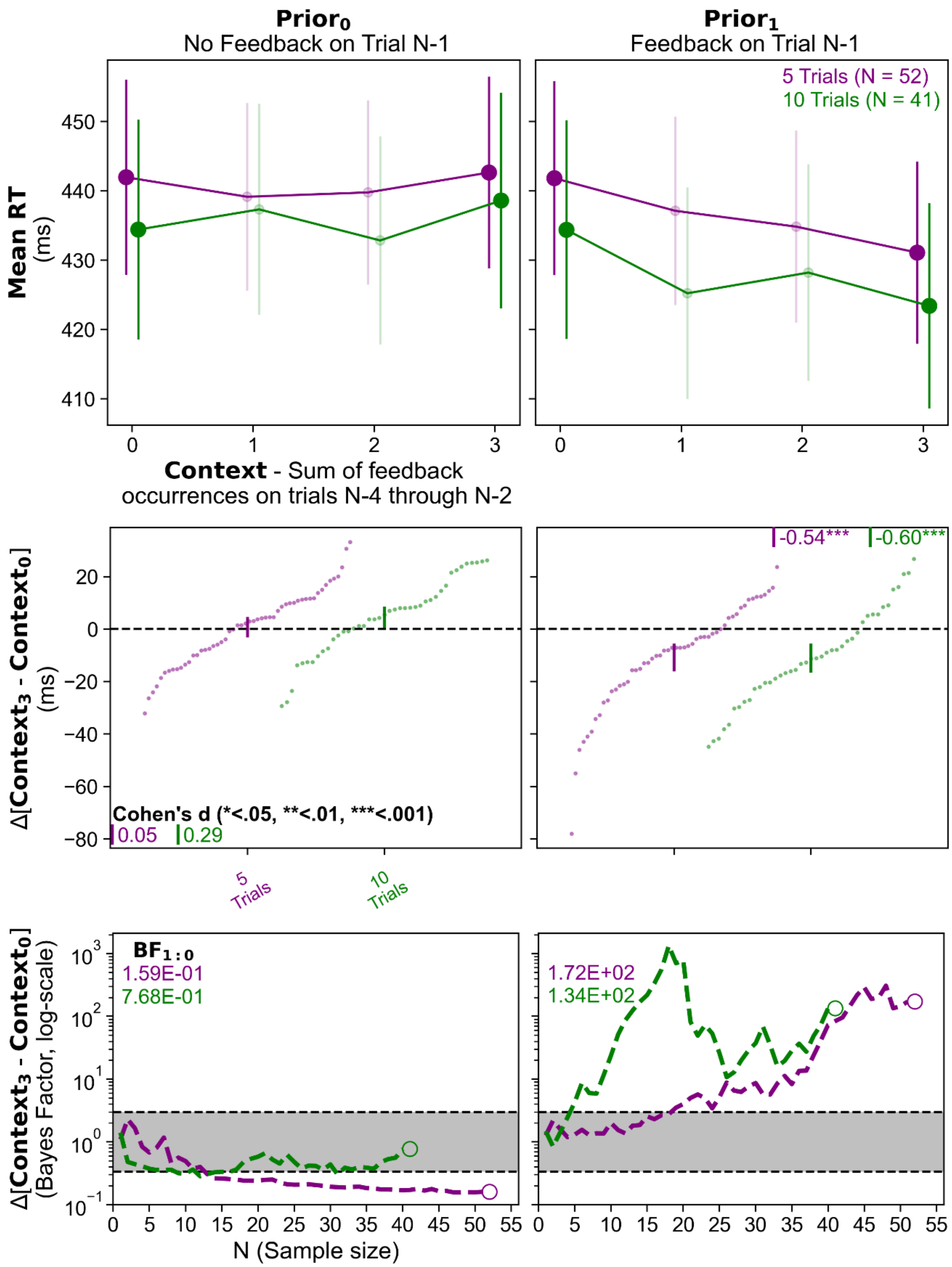


Figure 3, Experiments 1b. Top – Response Time by Prior (columns), Context (x-axis) and Cycle Duration (line color). Mid - contrasts and standardized effect size for RT On *Context* $_3$ – *Context$_0$* (* < .05, ** <.01, *<.001). Bottom – Bayes factors obtained using sequential Bayesian t-tests.**

First, we found that given feedback on trial N-1 ($Prior_1$), a streak of feedback trials ($Context_3$) compared with a streak of no feedback trials ($Context_0$) significantly facilitated response speed for both the 5-trials [-10.74 MS (19.71); $t(51) = -3.89$, $p < 0.001$, Cohen's $d = -0.54$, (-0.83, -0.25), BF1:0 171.6293] and 10-trials groups [-10.97 MS (18.01); $t(40) = -3.85$, $p < 0.001$, Cohen's $d = -0.60$, (-0.93, -0.27), BF1:0 133.8312].

Next we tested the effect of context on RT in trial N when no feedback was given on trial N-1. We found that given no feedback on trial N-1 ($Prior_0$), a streak of Feedback trials ($Context_3$) compared with a streak of no feedback trials ($Context_0$) did not significantly change response speed for the 5-Trials Cycle Duration group [0.68 MS (14.38); $t(51) = 0.34$, $p = 0.737$, Cohen's $d = 0.05$, (-0.23, 0.32), BF1:0 0.1595]or the 10 trials Cycle-Duration group [4.19 MS (14.50); $t(40) = 1.83$, $p = 0.075$, Cohen's $d = 0.29$, (-0.03, 0.60), BF1:0 0.7676], although for the latter the result was not as conclusive.

Experiment 2a – the re-analysis of Experiment 2 in Hemed et al., 2020

The results of Experiments 1a-1b show that appearance of feedback on trial N-1 *(Prior*) facilitated response time, but the magnitude of the effect was modulated by the sum of feedback occurrences on set of trials (*Context)*.

A potential concern that cannot be ruled out by Experiments 1a-1b is that the lack of own action-effects, especially when participants shifted from a feedback streak to a no-feedback streak) could have been understood by the participants as feedback on a performance error, which led to post error slowing. This is not very likely due to a couple of reasons. First, here, we did not analyze trials which immediately followed incorrect responses or response omissions, reducing the threat of post-error slowing. Second, even if post-error slowing had an effect lasting more than 1-2 trials, it cannot easily explain the speeding up following feedback accumulation. Next, this is less likely due to the low levels of performance errors on task trials.

Yet, the lack of feedback *could* have been interpreted by participants at some implicit level as an error (cf. Logan & Crump, 2010). To address this possibility experimentally we used another effectiveness-degradation manipulation. Experiments 2a-2c were designed in order to directly test whether the pattern of results obtained in Experiments 1a-1b would hold when controlling for the potential ‘performance informativeness’ or any other (potentially rewarding) informational value the own-action effects may carry (Charpentier, Bromberg-Martin, & Sharot, 2018; cf. Karsh & Eitam, 2015).

To do so we employed the same task used above on naïve participants, but rather than giving them no feedback for correct responses on ‘ineffective’ blocks, their actions led to an immediate flash in a random location surrounding the cue. As such, given an appropriate response, the information regarding correct performance is fully available, in both spatially predictable-feedback cycles and spatially unpredictable-feedback cycles. But, if as RSP suggests, the evaluation of effectiveness depends solely on a comparator like mechanism – the dynamic discovered above should still occur only in the spatially predictable feedback cycles. This is because the evaluation of effectiveness by the motor system depends on a sensory-motor prediction which would be very noisy in the spatially unpredictable condition (Karsh et al., 2016). In other words, in Experiments 2a-2c the manipulation of effectiveness involves only a sensory prediction error (SPE) due to the random location of the action-effect but not whether the response goal was attained (a target error; see Kim, Parvin, & Ivry, 2019).[4]

Methods

Participants

A total of 80 naïve participants were recruited [60% Women, Ages 20-37, M=25.6, SD = 3.8], via the Psychology department's online registration system. Demographics data from one participant were not obtained.

Apparatus

The experiment was programmed in Python using PsychoPy 1.86 (Peirce et al., 2019). The equipment used was the same as in Experiment 1a.

4 Experiment 2a chronologically followed Experiment 1a, and its original pre-registered hypotheses were dependent on the knowledge we had following Experiment 1a, see Open Science Framework site (Hemed, et al., 2018). Thus, the analysis brought here can be listed as exploratory, as it differs from the original one following the discovery of the confound described above.

Design

Experiment 2a is an exact replication of Experiment 1a, save for the following changes:

1. Sitting distance was fixed - 57cm away from the computer screen.

2. On "ineffective" cycles, instead of no feedback (as in Experiment 1a), participants received an own action-effect as in effective cycles, with the sole difference that the effect did not fully overlap with the location cue as it did in the effective cycles (Figure 1B). Rather, the effect appeared at a random (hence, unpredictable) location relative to the location of the cue. To do so, the center-to-center distance between the cue and the effect was randomly selected on each trial (sampled from a continuous uniform distribution ranging between 0.7 through 2.8, representing distance in degrees of visual angle). The action-effect was displaced at a random angle relative to the cue (sampled from a continuous uniform distribution ranging between 0 through 359, representing angle of displacement). In other words, the spatially-unpredictable action-effect appeared on the circumference of an (invisible) circle surrounding the cue with radius r and with a central angle θ, where $0.7 \leq r \leq 2.8$cm and $0 \leq \theta \leq 359$.

Results

Data preparation and screening

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 (7.27%) or missing (0.30%) responses, task trials with extremely fast (RT < 100, 0.09%) or slow RTs (RT > 750, 0.55%). Next we removed the data from a total of 2 participants (2.50% out of 80) where their accuracy on task trials was below < 80% (N = 1), accuracy on attentional probe trials was below < 50% (N = 0), or where less than 80% of the trials were valid in terms of either RT, accuracy or both (N = 1). In total, 9.99% of the task trials were removed, by filtering whole participants' data or individual trials.

Analysis

The effect of Spatially Predictable Feedback on trial N-1 (*Prior*) on response time was not significant [F(1, 76) = 3.64, p = 0.060, Partial Eta-Sq. = 0.05]. As were the effects of *Context* [F(3, 216) = 1.05, p = 0.368, Partial Eta-Sq. = 0.01] and *Cycle Duration* [F(1, 76) = 0.04, p = 0.845, Partial Eta-Sq. < 0.01] were insignificant. As per interactions, the interaction of *Prior* X *Context* [F(3, 216) = 3.03, p = 0.033, Partial Eta-Sq. = 0.04] was significant, but not that of *Prior X Cycle Duration* [F(1, 76) = 1.29, p = 0.260, Partial Eta-Sq. = 0.02] *or Context* X *Cycle-Duration* [F(3, 216) = 1.45, p = 0.230, Partial Eta-Sq. = 0.02], as well as the 3-way interaction [F(3, 216) = 1.15, p = 0.327, Partial Eta-Sq. = 0.01]].

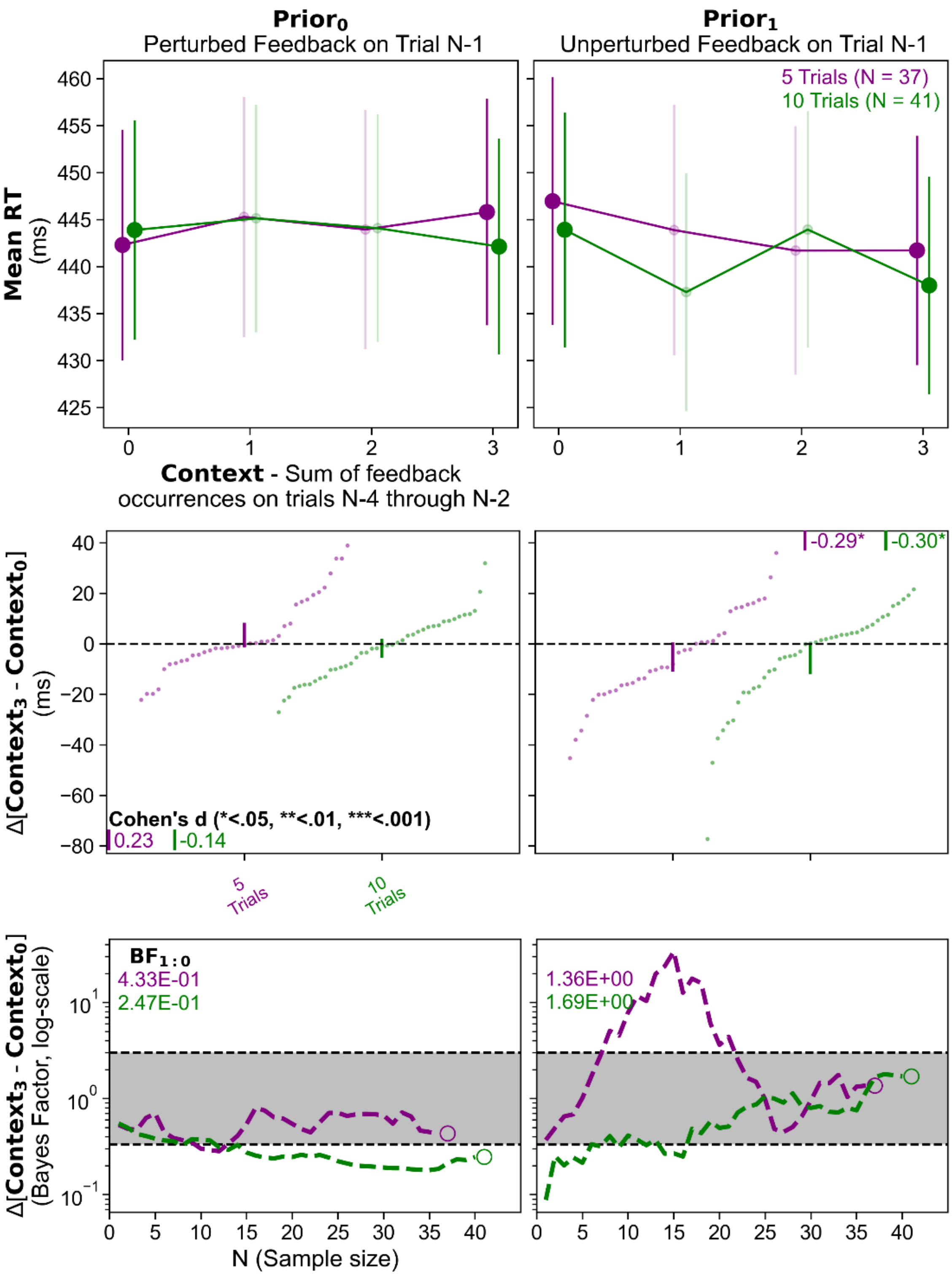


Figure 4, Experiment 2a. Top – Response Time by Prior (columns), Context (x-axis) and Cycle Duration (line color). Mid - contrasts and standardized effect size for RT On *Context* $_3$ – *Context$_0$* (* < .05, ** <.01, *<.001). Bottom – Bayes factors obtained using sequential Bayesian t-tests.**

First, we looked at the effect of accumulating feedback, we contrasted trials for which feedback was given on the response just before (*Prior*$_1$), following an uninterrupted streak of three previous responses that were followed by feedback (Context$_3$) facilitated responding compared to a uninterrupted streak of responses that were not followed by feedback (Context$_0$). **We found that accumulating feedback significantly facilitated response speed** for both the 5-Trials [-5.24 MS (17.86); t(36) = -1.76, p = 0.043, Cohen's d = -0.29, (-0.62, 0.04), BF1:0 1.3592] and 10-trials groups [-5.92 MS (19.60); t(40) = -1.91, p = 0.032, Cohen's d = -0.30, (-0.61, 0.02), BF1:0 1.6949, although Bayes factor support is not conclusive.

Conversely, when holding Feedback on trial N-1 at 0 (*Prior*$_0$), we found that, 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 5-Trials Cycle Duration group [3.50 MS (15.02); t(36) = 1.40, p = 0.170, Cohen's d = 0.23, (-0.10, 0.56), BF1:0 0.4328] or the 10 trials Cycle-Duration group [-1.76 MS (12.31); t(40) = -0.90, p = 0.372, Cohen's d = -0.14, (-0.45, 0.17), BF1:0 0.2469], with substantial support to the null hypothesis only for the latter,

Discussion

Experiment 2a largely corroborated the pattern observed in Experiments 1a-1b, using the contrasts analysis, but with the caveat of the ANOVA results being less clear cut (e.g., a less pronounced effect of *Prior*). Note that the although the Bayes factors are inconclusive which warrants sampling of more participants, this is a re-analysis of Experiment 2 from Hemed et al. (2020) and we relied on the additional (new) data from Experiments 2b-2c to provide further evidence.

All in all, the theoretical contribution of Experiment 2a is highly important as it confirm that reinforcement was driven by sensorimotor predictability, rather than by lack of a performance error signal (see above).

Experiment 2b

Experiments 2b was conducted online due to the COVID-19 pandemic. The experiment's main goals were to validate our paradigm using online experiment platform as well as to supply another replication of Experiment 2a, which involves a subtler, yet theoretically important, manipulation. This experiment was conducted prior to the discovery of the confound specified above, and was not pre-registered.

Methods

The experiment was prepared using PsychoPy3 and conducted online rather than in person, due to COVID-19 restrictions. Due to the novelty of the task in online settings, we changed several parameters in the experiment and a research assistant watched over participants via a video connection, to ensure they are fully engaged in the task.

To gather more data per participant, participants performed 660 trials in a single block, 16% (105) of them being attentional probes. To test whether participants require more time in online settings, we also increased the response window duration to 1250MS, both for probed and task trials.

Participants were recruited via the psychology department's SONA system, scheduled a Zoom meeting with the experimenter, received the participation link during the conversation and performed the experiment under the supervision of the experimenter. Then they filled out the demographics, debriefing and Sense of Agency questionnaires.

Participants

We recruited 104 participants (excluding three participants whose experimental session crashed). The participants were aged 19.0-42.0 ($M = 24.78$, $SD = 4.10$), 70.19% identified as female.

Results

Data preparation and screening

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 (7.64%) or missing (0.20%) responses, task trials with extremely fast ($RT < 100$, 0.15%) or slow RTs ($RT > 1150$, 0.84%). Next we removed the data from a total of 7 participants (6.73% out of 104) where their accuracy on task trials was below < 80% ($N = 3$), 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 ($N = 7$). In total, 13.38% of the task trials were removed, by filtering whole participants' data or individual trials.

Analysis

The effect of Spatially Predictable Feedback on trial N-1 (*Prior*) on response time was not significant [$F(1, 95) = 1.26$, $p = 0.264$, Partial Eta-Sq. = 0.01], as was the effect of Cycle

Duration [F(1, 95) = 0.29, p = 0.590, Partial Eta-Sq. < 0.01] and only a marginally significant effect of *Context* [F(3, 270) = 2.59, p = 0.056, Partial Eta-Sq. = 0.03]. As per interactions, we found an interaction between *Prior* and *Context* [F(3, 278) = 8.71, p = 0.001, Partial Eta-Sq. = 0.08], but not within *Prior* X *Cycle Duration* [F(1, 95) = 0.13, p = 0.716, Partial Eta-Sq. < 0.01], or *Context* X *Cycle-Duration* [F(3, 270) = 0.99, p = 0.397, Partial Eta-Sq. = 0.01], as well as the 3-way interaction [F(3, 278) = 0.57, p = 0.632, Partial Eta-Sq. = 0.01].

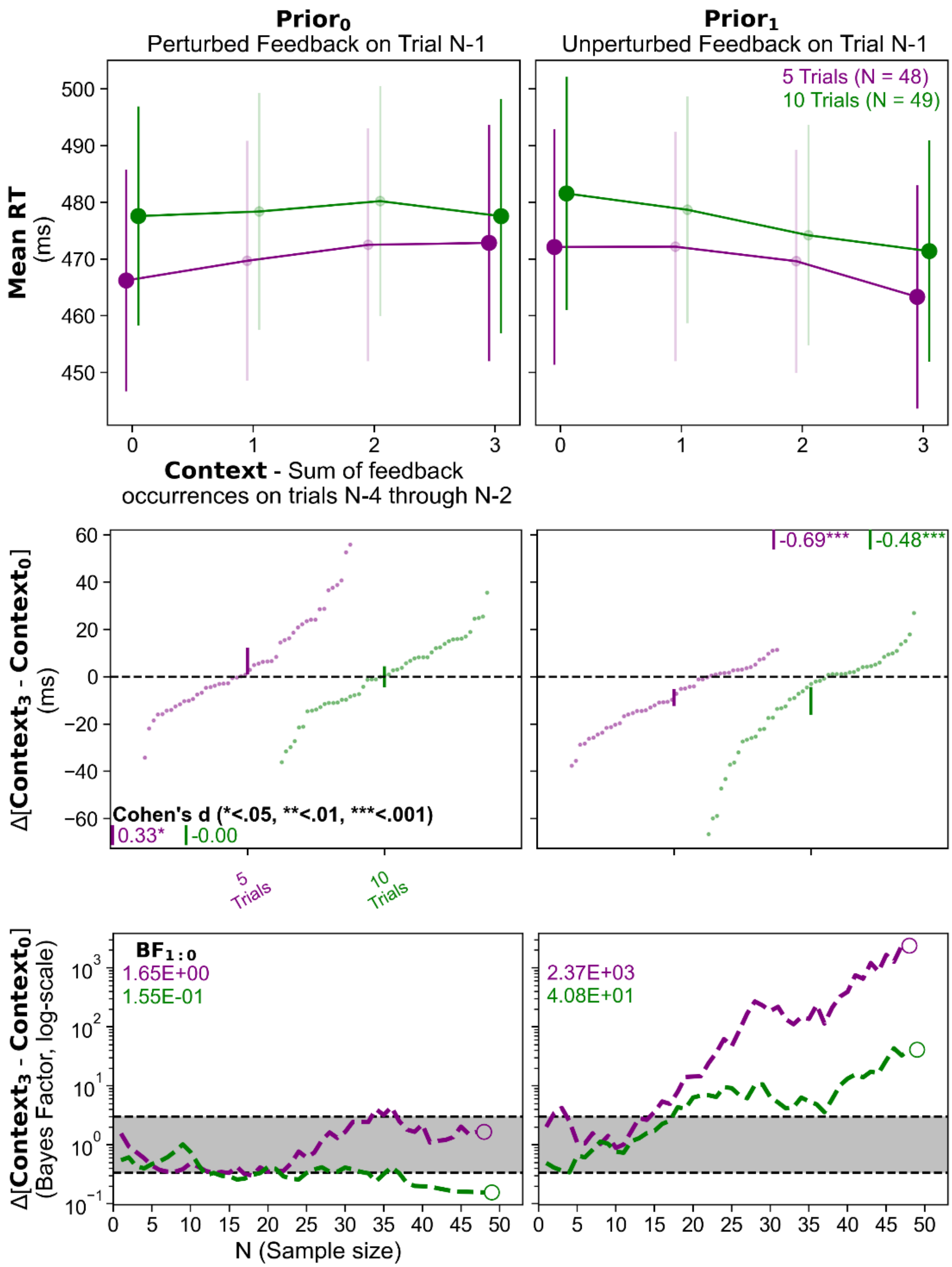


Figure , Experiment 2b. Top – Response Time by Prior (columns), Context (x-axis) and Cycle Duration (line color). Mid - contrasts and standardized effect size for RT On *Context* $_3$ – *Context$_0$* (* < .05, ** <.01, *<.001). Bottom – Bayes factors obtained using sequential Bayesian t-tests.**

Next, we looked at the two different contrasts. First, we found that if Spatially-Predictable feedback was given on trial N-1 (*$Prior_1$*), a streak of Spatially-predictable Feedback trials ($Context_3$) compared with a streak of Spatially-Unpredictable Feedback trials ($Context_0$) significantly facilitated response speed for both the 5-trials [-8.79 MS (12.58); $t(47) = -4.79$, $p < 0.001$, Cohen's $d = -0.69$, (-1.00, -0.37), BF1:0 2367.0834] and 10-trials groups [-10.17 MS (20.87); $t(48) = -3.38$, $p < 0.001$, Cohen's $d = -0.48$, (-0.78, -0.18), BF1:0 40.7823], with substantial support for both results.

Finally, we found that given Spatially-Unpredictable feedback on trial N-1 (*$Prior_0$*), a streak of Spatially-Predictable Feedback trials ($Context_3$) compared with a streak of Spatially-Unpredictable Feedback trials ($Context_0$) significantly slowed down response speed for the 5-Trials Cycle Duration group [6.63 MS (19.91); $t(47) = 2.28$, $p = 0.027$, Cohen's $d = 0.33$, (0.04, 0.62), BF1:0 1.6497] but not for the 10 trials Cycle-Duration group [-0.03 MS (15.82); $t(48) = -0.01$, $p = 0.991$, Cohen's $d = -0.00$, (-0.28, 0.28), BF1:0 0.1553], with substantial support for the null hypothesis only for the latter.

Discussion

Experiment 2b once again replicated the pattern found on Experiment 1a,-1b using the critical contrasts, although once again the ANOVA model provided less clear-cut results. Compared with Experiment 2a which utilized the same spatial-perturbation manipulation, the effect size was larger and support from Bayes factors was greater. In terms of the ANOVA, we did not find a main effect for *Prior*, potentially because the influence of the *Context* factor was significant as well as its interaction with the value of *Prior*.

Experiments 2c – Replication of Experiments 2a-2b, excluding attentional probes

Experiments 2c was a replication of Experiments 2a-2b. But similarly, to Experiments 1b, it did not include attentional probes. Given that at the time of pre-registration we were aware of the confound, the pre-registered hypotheses are confirmatory (https://osf.io/gcev7)., .

Methods

Participants

We recruited 104 participants. The participants were aged 19.0-45.0 (M = 26.04, SD = 5.48), 72.82% identified as female.

Stimuli and design

The spatial perturbation distance was 1.75-2.25 times the cue diameter. Distance and angle were selected randomly on each trial from a uniform distribution. Except for that, all details were identical to Experiments 1b.

Results

Data preparation and screening

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 (8.75%) or missing (3.23%) responses, task trials with extremely fast (RT < 100, 0.4%) or slow RTs (RT > 750, 0.88%). Next we removed the data from a total of 14 participants (13.46% out of 104) where their accuracy on task trials was below < 80% (N = 13), or where less than 80% of the trials were valid in terms of either RT, accuracy or both (N = 14). In total, 20.61% of the task trials were removed, by filtering whole participants' data or individual trials.

Analysis

The effect of Spatially Predictable Feedback on trial N-1 (*Prior*) on response time was significant [$F(1, 88) = 6.29$, $p = 0.014$, Partial Eta-Sq. = 0.07], but the other factor terms were insignificant - Cycle Duration [$F(1, 88) = 1.92$, $p = 0.169$, Partial Eta-Sq. = 0.02] and *Context* [$F(3, 256) = 2.33$, $p = 0.077$, Partial Eta-Sq. = 0.03]. All interactions were not statistically significant *Prior* X *Context* [$F(3, 249) = 0.20$, $p = 0.889$, Partial Eta-Sq. < 0.01], *Prior X Cycle* [$F(1, 88) = 1.33$, $p = 0.251$, Partial Eta-Sq. = 0.01], *Context* X *Cycle-Duration* [$F(3, 256) = 1.54$, $p = 0.206$, Partial Eta-Sq. = 0.02], as well as the 3-way interaction [$F(3, 249) = 0.09$, $p = 0.960$, Partial Eta-Sq. < 0.01].

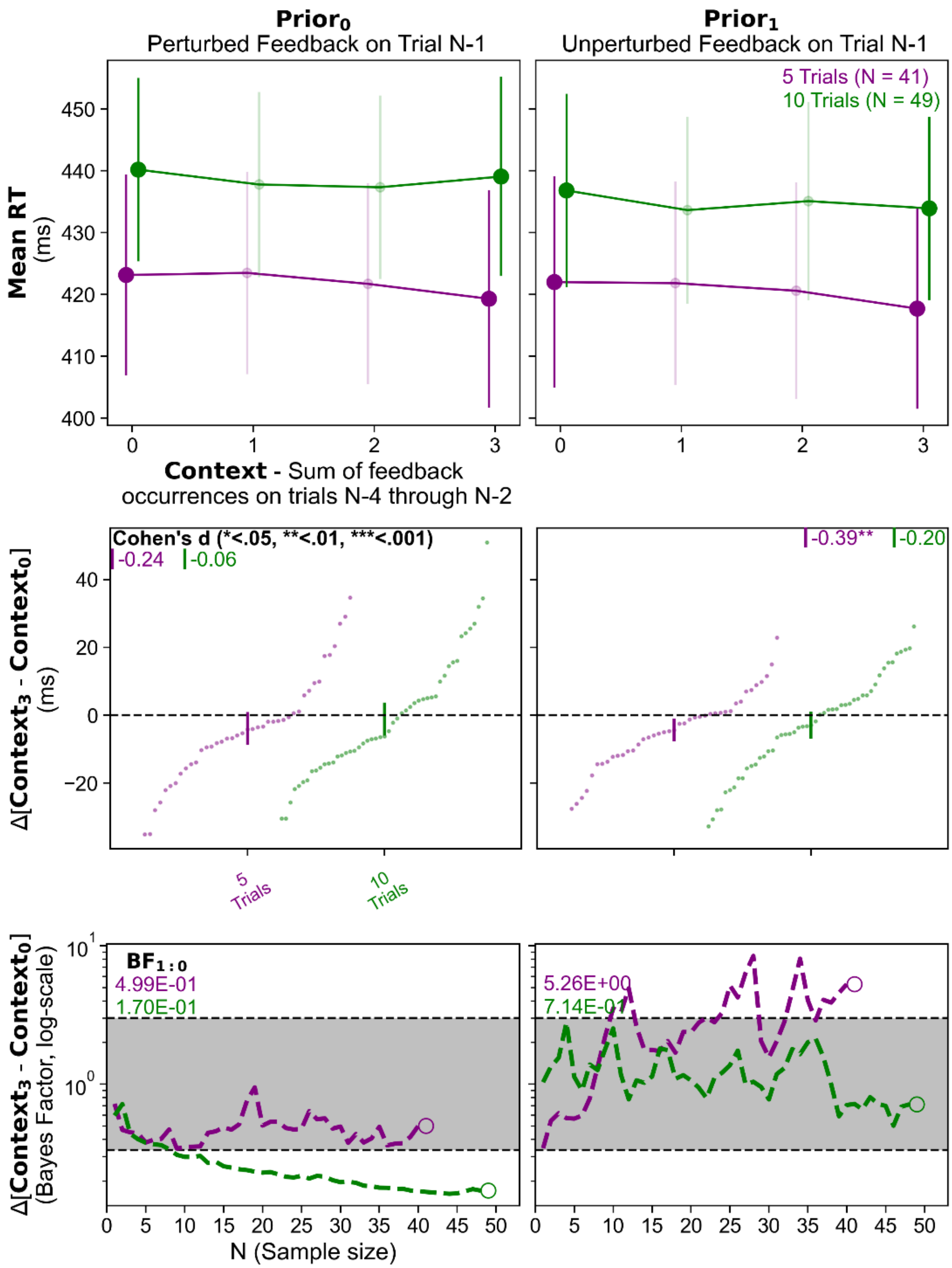


Figure 6, Experiment 2c. Top – Response Time by Prior (columns), Context (x-axis) and Cycle Duration (line color). Mid - contrasts and standardized effect size for RT On *Context* $_3$ – *Context$_0$* (* < .05, ** <.01, *<.001). Bottom – Bayes factors obtained using sequential Bayesian t-tests.**

First, we found that given Spatially-Predictable feedback on trial N-1, a streak of Spatially-predictable Feedback trials ($Context_3$) compared with a streak of Spatially-Unpredictable Feedback trials ($Context_0$) significantly facilitated response speed for the 5-trials group [-4.30 MS (10.84); $t(40) = -2.51$, $p = 0.008$, Cohen's $d = -0.39$, (-0.71, -0.07), BF1:0 5.2598], but not for the 10-trials group [-2.89 MS (14.23); $t(48) = -1.41$, $p = 0.083$, Cohen's $d = -0.20$, (-0.48, 0.08), BF1:0 0.7141], although support for the null was not decisive in the latter.

Next we tested the effect of context on rt in trial N when Spatially-Unpredictable feedback was given on trial N-1. We found that given Spatially-Unpredictable feedback on trial N-1 (Prior0), a streak of Spatially-Predictable Feedback trials (Context3) compared with a streak of Spatially-Unpredictable Feedback trials (Context0) did not significantly slow down response speed for the 5-Trials Cycle Duration group [-3.85 MS (15.82); $t(40) = -1.54$, $p = 0.132$, Cohen's $d = -0.24$, (-0.55, 0.07), BF1:0 0.4992] or the 10 trials Cycle-Duration group [-1.12 MS (17.46); $t(48) = -0.44$, $p = 0.660$, Cohen's $d = -0.06$, (-0.34, 0.22), BF1:0 0.1705], although support for the null hypothesis was conclusive only to the latter.

Discussion

Experiment 2c was intended to replicate the results of Experiments 2a-2b, while not including attentional probes. Thus, it can be used to rule out both the threat of our results emerging fully from task-switching onset by attentional probes. It also lessens the threat of reinforcement only occurring due to lack of error signal compared with Experiments 1a-1b where ‘ineffective’ cycles included no-feedback rather than spatially perturbed one. Generally, it provided support for our hypothesis that recent spatially-predictable feedback facilitates response speed, pending the occurrence of less recent feedback.

However, the evidence was rather weak in favor of the pattern described above –the effect of recent experience with 'being effective' is contingent on a response continuing to have the same effect. It also did provide us with a somewhat unique pattern, where for the first time we observed a much weaker (and insignificant) facilitation effect in the 10-trials group, was nominally slower in general.

Due to the variance in the strength of evidence, we chose to pool the results from multiple experiments and conduct a meta-analysis of the effect sizes from the two critical contrasts. It is described in the next section.

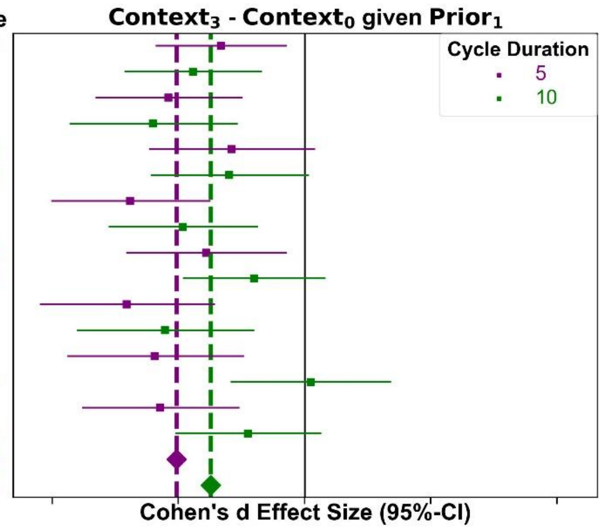
Meta-Analysis

Before discussing our findings, we provide a meta-analysis of the effect of the critical contrasts we included in each of our experiments to test the dynamic influence of feedback on reinforcement. The meta-analysis consists of 8 different experiments, five experiments from the current work, and three from another work using the same task (see Hemed & Eitam, 2022). In total, results from 719 participants were included in the analysis. The significance of the pooled effect sizes is calculated using the inverse variance-weighted average method (Lee et al., 2016). A table and a forest plot are used to summarize the findings below, see Figure 9. For a meta-analysis of the pattern of contracts when using the confounded processing pipeline described above, see supplementary materials repository.

Examining the effect of change in action-effectiveness based on the sum of feedback occurrences on the most recent trials ($Context_0$ vs $Context_3$) given feedback on Trial N-1 revealed evidence for small to medium effects of response speed facilitation (i.e., faster response time). For the 5-trials groups there was a medium effect (Cohen d = -0.51, p = .005), while for the 10-trials group the effect was (unpredictably) slightly smaller (Cohen's d = -0.37, p = .018). The variance in effect size was considerably larger for the 10-trials group, as can be seen from the forest plot. All in all, it seems that effect of recent effectiveness on response speed is considerably affected by the less recent history of effectiveness (see Figure 9, top panel).

Conversely, the effect of recent *lack of effectiveness* on RT is not modified by less recent effectiveness. As revealed by the tiny and insignificant effect sizes of contrasting the sum of feedback occurrences on the most recent trials ($Context_0$ vs $Context_3$) given No-feedback on Trial N-1 ($Prior_0$); Cohen's d was 0.1 (p = 0.32) and <0.01 (p = 0.99), for the 5-trials and 10-trials groups respectively (see Figure 9, bottom pane

Paper	Exp. #	N	Weight (%)	ES	95%-CI	p-value
Current	1A	60	11.94	-0.33	[-0.59 -0.07]	0.006
		57	13.24	-0.44	[-0.71 -0.17]	<.001
	1B	52	07.70	-0.54	[-0.83 -0.25]	<.001
		41	14.16	-0.60	[-0.93 -0.27]	<.001
	2A	37	09.39	-0.29	[-0.62 0.04]	0.043
		41	11.95	-0.30	[-0.61 0.02]	0.032
	2B	48	18.92	-0.69	[-1.00 -0.37]	<.001
		49	10.54	-0.48	[-0.78 -0.18]	<.001
	2C	41	25.45	-0.39	[-0.71 -0.07]	0.008
		49	22.66	-0.20	[-0.48 0.08]	0.083
Hemed & Eitam (2022)	1	40	10.93	-0.71	[-1.05 -0.36]	<.001
		36	13.10	-0.55	[-0.90 -0.20]	0.001
	2	37	05.45	-0.59	[-0.94 -0.24]	<.001
		38	03.56	0.02	[-0.29 0.34]	0.559
	3	46	10.21	-0.57	[-0.88 -0.26]	<.001
		47	10.79	-0.22	[-0.51 0.07]	0.065
Meta Analysis		**361**	**100.00**	**-0.51**		**0.005**
		358	**100.00**	**-0.37**		**0.018**



Paper	Exp. #	N	Weight (%)	ES	95%-CI	p-value
Current	1A	60	14.16	0.04	[-0.21 0.30]	0.744
		57	10.64	-0.16	[-0.42 0.11]	0.243
	1B	52	16.59	0.05	[-0.23 0.32]	0.737
		41	18.23	0.29	[-0.03 0.60]	0.075
	2A	37	15.20	0.23	[-0.10 0.56]	0.170
		41	25.31	-0.14	[-0.45 0.17]	0.372
	2B	48	08.66	0.33	[0.04 0.62]	0.027
		49	15.33	<|.01|	[-0.28 0.28]	0.991
	2C	41	13.71	-0.24	[-0.55 0.07]	0.132
		49	12.58	-0.06	[-0.34 0.22]	0.660
Hemed & Eitam (2022)	1	40	12.05	0.23	[-0.09 0.54]	0.157
		36	06.26	0.07	[-0.26 0.40]	0.673
	2	37	04.54	0.70	[0.33 1.05]	<.001
		38	03.32	0.10	[-0.22 0.42]	0.525
	3	46	15.09	0.03	[-0.26 0.32]	0.834
		47	08.33	-0.08	[-0.36 0.21]	0.593
Meta Analysis		**361**	**100.00**	**0.11**		**0.317**
		358	**100.00**	**<|.01|**		**0.988**

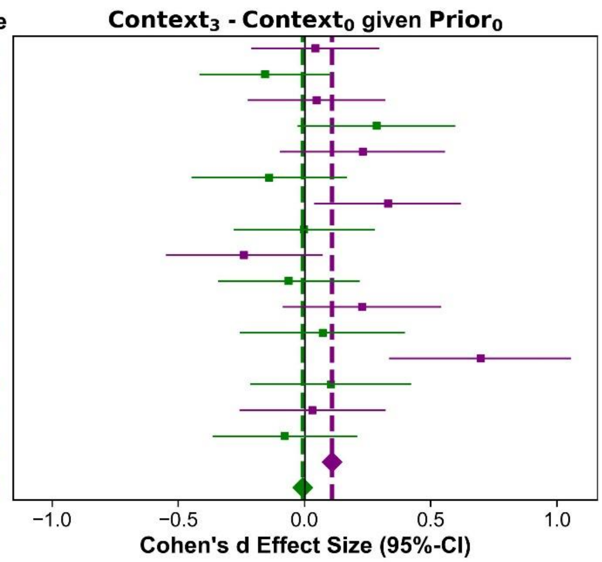


Figure 7: Meta-analysis of the effect of change in feedback on response time. Leftward (negative) values denote decrease in response time. Rightward (positive) values denote increases in response time. Vertical dashed lines denote average effect size across experiments. Top – feedback on trial N-1. Bottom – no feedback on trial N-1.

General Discussion

In the past two decades, the ‘Comparator model’, described above, inspired much research on what was later termed the 'feeling' (‘implicit'; motor based) and the 'judgment' ('explicit'; cognitive) of agency (e.g., Gallagher, 2012; Synofzik et al., 2008).

Using an effect previously dubbed 'motivation from control' - that has been theoretically and empirically linked to a comparator-based computation of a motor program's effectiveness - we explored whether this computation is sensitive to subtle changes in the trend of effectiveness. The results of this study and particularly the contrasts tested in the meta-analysis section show that the ‘motivation from control’ effect is sensitive to both gradual change in the effectiveness of an action but to a larger degree (in the current design) to abrupt changes in effectiveness. intricate –the similar pattern of results between the five experiments lend support to its robustness as well as to our theoretical claims which we elaborate on below.

We interpret the results from the current study as shedding new light on the way a response’s effectiveness is evaluated by the motor system. Specifically, we conclude that the comparator model needs to accommodate that continuous updating of a response’s effectiveness as documented in this study. To a lesser degree, we previously believed that such an updated model should also consider the precision of its sensorimotor predictions, however it does not seem to be the case (See more below).

Our findings are consistent with the Comparator’s reliance on confirmation of sensorimotor *prediction*, minus, as we have stated – the need to introduce to the model a mechanism which can reinforce a response proportionally to the precision of the relevant forward model. The addition of the said motor response-selection mechanism which is what we see as our contribution to the model ("prediction strength"; (Eitam et al., 2013; Hemed et al., 2022; Karsh & Eitam, 2015; Karsh et al., 2016; Karsh & Eitam, 2015). For those interested in the adequacy of optimal control theories for explaining motor control - another interesting aspect is that such modulation, although reinforcing, is orthogonal (i.e., independent of) to any (learned) value function (Friston, 2011).

Differential sensitivity to increases and decreases in effectiveness?

The current results are at odds with findings of 'losing control' being more impactful on behavior compared with 'gaining control', (Wen & Haggard, 2018). Wen and Haggard suggested, among other things, that attention and perception are sensitive to subtle changes in one's degree of control (over moving objects) under dynamic conditions. Specifically, they explored the interaction between control and perception using a series of experiments in which participants were asked to detect a target among distractors, with the discriminative feature between the two becoming apparent when the participant moved the computer mouse. The movements of the target correlated to different degrees with the movements of the participant's mouse (per experimental condition). Their key finding was that relatively more controlled targets (corresponding with the mouse's movements on >50% of the time) were recognized more quickly compared to less controlled ones. However, once a target was relatively controlled, the loss of control facilitated visual recognition while for the corresponding condition (a target which is less controlled than the distractors), a compatible gain in control did not facilitate recognition to the same degree. That study was groundbreaking by documenting the influence of gradual changes in controllability using an ecologically valid task, yet it focused on 'still' images of what were perceived to be key points. Furthermore, given the novelty of the task and hence the paucity of previous work using it, its sensitivity to comparator-relevant parameter and hence the relevance of these findings to the comparator model – is yet unclear.

Here we opted for using a phenomenon ('motivation from control') that was systematically linked to the Comparator model through empirical work and employed a systematic manipulation of various effectiveness levels across time (e.g., a gradual change of the target controllability from 0 to 30, 70 and 100%); this approach enables us to link back our findings to how the computation of a motor-program's judged effectiveness occurs online. Experiments 2a-2c directly demonstrated that spatial prediction is key for effectiveness, as well as effectiveness history, to influence response time. Given that sensorimotor prediction is a key element of the comparator model, we argue that the model explains our findings (by inference to the best explanation or abduction) but should also be slightly modified to fully accommodate them.

Such modifications may include adding a limited-capacity buffer which holds own-action effects several-responses back and an explication of how *Context* and *Prior* (see Results section) approximate a motor plan's effectiveness and come to influence motor-plan selection. Given that previous findings show that the RT measure is insensitive to cognitions (e.g., expectations; (Hemed et al., 2022; Karsh & Eitam, 2015; Karsh et al., 2016, 2020) - how are context and sudden change in of effectiveness represented? Answering these questions would be benefited by other methods such computational modeling of data from our task[5].

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[5] For example, the behavioral data can be used to build a multi-process model (Kim et al., 2019; Smith et al., 2006), assuming the *n-1* (immediate) feedback occurrence is interpreted in light of the recent feedback occurrences (what we referred to as context) and lead to the observed modulations in action-selection (i.e., response time). A third

A role for precision?

It is interesting that here (differently from our previous and misguided analysis), we did not find that the 10-trials group had any inhibition when switching to a No-Feedback cycle (i.e., Context[3] VS. Context0, given Prior0). This is interesting as previously we surmised that such difference could have come mainly from difference in frequency of alternating Feedback and No-Feedback cycles (i.e., whether the appearance of feedback was modified every 5 or 10 trials). It makes perfect sense that there may be higher credibility to the changes in effectiveness of actions in the longer Cycle Duration condition, due to the higher precision. If the different Cycle-Duration conditions are cast as two probability distributions of receiving own-action feedback; both with the same central tendency but differing in their scaling parameter (standard deviation) then, Bayesian precision-weighted frameworks (Ernst & Banks, 2002; Friston, 2011; Yon & Frith, 2021) would indeed predict that a lower variance signal (10-trials Cycle Duration; due to less frequent changes in action-effect contingency) would be weighted more heavily as a cue for effectiveness, for example as implemented in Optimal Control theory as a (high gain) Kalman filter (Friston, 2011). Compared to the more variable one (5-trials Cycle Duration) which, may have been expected to accentuate the effect of effectiveness change on RT's. However, given our new analysis it seems that the difference we previously thought existed was due to interference from task-switching between attentional probes and the main task. One could hypothesize that the data decimation we used on Experiments 1a and 2a-2b obliterated this difference, it was still

component which must be considered is Cycle Duration, since the pattern found by the contrasts is accentuated by length of effect streaks (see additional discussion below). We are currently working on implementing such a model. A second possibility is to model the responses as several competing-accumulator units (e.g, Usher & McClelland, 2001; Brown & Heathcote, 2008) . However, as the current task does not generate many incorrect responses (which are crucial for fitting the model to our data), the model would not be very constrained.

not found on Experiments 1b and 2c where we did not have probed trials (and hence also no post-probe trials).

While it is intuitive to think that we would observe greater changes in response speed for the 10-trials group (where precision is higher), the current data does not support the precision hypothesis as the effect sizes we obtained (see Meta-Analysis) for facilitation are larger for the 5-trials group compared with the 10-trials group (~.5 and ~.38, respectively). While it s it is possible then that given ever longer streaks (of 15-20 trials), the precision would be much higher, and we would observe different response patterns between the different groups, this is only a hypothesis[6].

What could lend support to the precision hypothesis is that the 10-trials group was extremely sensitive to post-probe trials, as evident from the difference between the current analysis (which excluded post-probe trials) and the previous one (which did not; cf. Hemed et al., 2020). Either way, this avenue warrants dedicated research on the modulation of task-switching costs by different precision levels of effectiveness and control.

Dynamics of Effectiveness as RPE and SPE?

We argued above that feedback indicating control over the environment is a reinforcer for said effective actions. The nature of our task can be also seen introducing positive and negative reward prediction errors iteratively, as participants receive unexpectedly feedback or no feedback for their actions (without arguing that our manipulation modifies the ‘Sense of Agency’).

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[6] If correct, this explanation would suggest an additional, long term, mechanism which accompanies the short term (e.g., 3-back) buffer we suggested to incorporate in the Comparator model variants.

Schultz and colleges' (Schultz et al., 1997) work on phasic activity of DA neurons in the Striatum following positive and negative reward prediction error focused mostly on classical conditioning. The work showed that DA activity is modulated on a trial-by-trial basis and hence highly reactive (Schultz, 2016; Schultz et al., 1997). This work led to exploring how similar DA activity affects action selection (i.e., instrumental conditioning; Howard, Li, Geddes, & Jin, 2017; Morris, Nevet, Arkadir, Vaadia, & Bergman, 2006; Redgrave, Gurney, & Reynolds, 2008; Stopper, Maric, Montes, Wiedman, & Floresco, 2014).

Glimcher (2011) proposed that recent (relative to distant) trials have an exponentially larger influence over reward prediction and by definition – the prediction error[7]. In an animal study (Bayer & Glimcher, 2005), to induce reward prediction errors (RPE's) – the action which led to a maximal reward was periodically changed. DA activity indeed matched a recency weighted prediction, based on positive and negative RPE in past trials. Similar results were also obtained by Parker and colleagues (Parker et al., 2016). In the current study, the pattern of changes in response speed due to RPE - the interaction between *Prior* and recent *Context*, is resembling of the exponential decay of the weight of past experiences . However, DA activity and RPE is an interesting framework for our findings in a future work[8].

Additionally, if one rejects the notion that own-action effects are rewarding, they might argue that our task simply introduces a series of sensory prediction errors (SPE's). In contrast to RPE, the monitoring of SPE following motor actions was attributed to the Cerebellum

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[7] It should be indicated that the idea of exponentially weighted average is not a new one and was introduced by earlier reinforcement learning models both classical and instrumental, with the most prominent being the Rescorla-Wagner model (Rescorla & Wagner, 1972).

[8] We would like to thank Prof. Bernhard Hommel, a reviewer of a previous version of this manuscript, for indicating the interesting non-linearity in our data.

(Blakemore et al., 1998; Ishikawa, Tomatsu, Izawa, & Kakei, 2016; Schlerf, Ivry, & Diedrichsen, 2012; Sokolov, Miall, & Ivry, 2017; Tseng, Diedrichsen, Krakauer, Shadmehr, & Bastian, 2007). Interesting though – the Cerebellum itself was also argued to be the neural substrate implementing a computation akin to the Comparator model (Blakemore, Frith, & Wolpert, 2001; Blakemore et al., 1999; Waszak, Cardoso-Leite, & Hughes, 2012; Wenke & Haggard, 2009; Wolpert & Miall, 1996). Furthermore, it was argued recently that SPE could in fact be represented in the same DA system described above, as a general signal rather than limited to RPE (Gardner et al., 2018). At any rate, more research using physiological measures will be able to shed light on the degree to which the processes and mechanisms described above are relevant for explaining our findings.

Limitations and final remarks

The current study may have several caveats. One caveat is the possibility that our choice of design has artificially induced a seeming 3-trial long integration window found to be optimal for explaining RT. However, since we are interested in the *change* in responding as function of the continuous evaluation of effectiveness (which can be inferred from the additional contrasts showing the transition from ‘effective’ to ‘ineffective’ blocks and vice versa), this does not seem to be a major concern. In addition, multiple windows were tested and found to be less adequate (see reference for previous version of manuscript under Supplementary Material); previous findings (Hemed, Karsh, & Eitam, 2018) also suggest the used window size is the most relevant. Previous animal studies have shown that integration of information regarding rewarding responses occurs for several recent trials (Parker et al., 2016), solidifying our conclusion that effectiveness of actions may indeed be evaluated in a rather short time window.

A second caveat is that in contrast with our claims that the change in response speed stem from changes in effectiveness as judged by the motor system (the 'Comparator'), these are an outcome of a more 'conceptual' change, related to a *judgement* of agency. We concluded that this is less plausible because participants were not even aware of what determined whether feedback appeared given correct response (see methods section) but potentially, more sensitive measures may prove us wrong. Elsewhere, we (Hemed et al., 2022; Karsh & Eitam, 2015) argued and empirically demonstrated that action execution, indexed by the RT measure as an identical manner to that used in the current study, is insensitive to 'higher level' or ‘cognitive' expectations such as explicit causal judgement. Conversely, action selection (e.g., which finger to use) was found to be sensitive to accessible task expectancies, such as ‘person-level’ judgement of agency, compared with a motor system-based one (Hemed et al., 2022; N Karsh & Eitam, 2015; Noam Karsh et al., 2016; Noam Karsh & Eitam, 2015). On this note, a more extreme approach is represented by others, arguing that forward models akin to the Comparator model cannot be used to explain out-of-body action-effects (such as the ones we have used), only body-related action-effects such as being tickled (Dogge et al., 2019).

Optionally, one could speculate that if explicit processes in the continuous evaluation of ‘effectiveness’ are indeed involved in the changes in response speed, then asking participants to ignore the visual action-effects would reduce these changes in response speed. In a recent study (Avraham et al., 2020), participants performed a sensorimotor adaptation task in which the movement of their cursor towards a target was rotated in relation to their movement, but the degree of rotation changed every trial, in either a pseudo-random manner (with low trial-to-trial autocorrelation) or followed a random-walk process (i.e., –high trial-to-trial autocorrelation). On additional trials intended to measure the degree of rotation, cursor trajectory was invariant (i.e., it did not reflect the participants' movements, but followed a set path) – it was shown that in the Random-walk condition, participants tended to correct for the perturbation introduced in the previous trial, compared with the Random-rotation group. If participants were asked to ignore the cursor's feedback and simply reach for the target, this difference disappeared, pointing that the trial-to-trial learning resulted from explicit process. However, even though we did not ask our participants to either pay attention or ignore the feedback, when debriefed they either reported that feedback appeared randomly and when probed for the goal of the experiment – most did not refer to the feedback at all. Future studies should address this point and provide more conclusive evidence.

Conclusion

To recap, the 'Comparator model', while originating in a formal computational model (Wolpert, Ghahramani, & Jordan, 1995) has been often used in the psychological literature on the human Sense of Agency heuristically and without much elaboration; yet it has had an excellent generative effect on the empirical study of the sense and effects of agency. Recently though, both theoretical and empirical work has cast doubt over some of the empirical content supporting the comparator model of agency. On one hand, the current work diverges from recent work in supporting the worth of the model but on the other hand also converges with it in highlighting the need to flesh out a modified version of the model.

We also find that the current study points at the potential benefit in following empirical work more closely to guide such modifications (i.e., to maintain contact between the model and its empirical content). Specifically, the current study strongly suggests that a modified version should be (1) able to accommodate the sensitivity to local changes in effectiveness of a motor program as well as (2) rapidly translate the dynamic evaluation of effectiveness to modifications in selecting motor parameters ('execution'). Such elaboration of the model may also help differentiate between computations of motor effectiveness and judgments of (general) causal relations and how these relate to empirical indexes of agency, such as intentional binding and sensory attenuation.

Coda

On a meta-scientific level, we are thankful for the opportunity to correct the -confound we discovered in our previous paper (now-retracted; Hemed et al., 2020). Our experience with self-retraction has been a humbling one, but this is self-correcting science in Vivo. We hope that our ultimately-positive case, will motivate in future researchers to scrutinize their findings, and come forward when finding anything amiss. The normalization of self-retraction will promote transparency and hopefully more accurate and efficient scientific progress, by terminating innocently-misguided research avenues at an early stage.

Significance Statement

Human beings live in dynamic environments and constantly need to evaluate the effectiveness of their motor actions to survive. It is not clear whether and how information about the effectiveness of an action is integrated over time and used to increase and decrease the motivation to perform this action as it becomes more effective or less effective, respectively. We manipulated the control participants had over their environment and measured their response speed to imperative cues. Our results suggest that the human mind is highly sensitive to detect subtle changes in the effectiveness of its motor-plans. We also found that the integration of recent experience interacts in a complex way but consistent fashion with more distant experience, a finding reminiscent of the stronger weighting of recent evidence found in animal studies on reward prediction. Interestingly, this finding may also be accommodated by Bayesian optimal integration, which is a key feature of normative models of motor control.

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).

Acknowledgments

This research was supported by The Israel Science Foundation (ISF) grant number 339/16 and The Bi-national Science Foundation (BSF) grant number 2016/299 to B.E.

We would like to thank Rika (Rivka) Aviv and Amier Kardosh for collecting the data of Experiments 1a and 2a-2b-3. We would like to thank Rotem Kornblum for collecting the data of Experiments 1b and 2c.

Author Contributions

E.H., and B.E., developed the study concept. E.H, L.Y. and B.E. contributed to the experiments' design. L.Y., S.B-E and E.H., collected the data. E.H., A.T., S.B-E and B.E. performed data analysis. All authors wrote the paper and approved the submitted version of the manuscript.

Prior Dissemination Statement

The current work consists of a study which was previously published, self-retracted, reanalyzed and rewritten. See introduction and previous publication (Hemed et al., 2020).

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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