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

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Abstract

An influential model for explaining humans' 'feeling of agency' - the Comparator model - heavily leans on the idea of the veracity of sensorimotor prediction: the fit between the predicted sensory input following the execution of a specific motor program and the immediately incoming sensation. As such, the model describes one way in which our mind estimates the degree of control over the environment (henceforth, its effectiveness). Previously, using a behavioral phenomenon we named Reinforcement from Sensorimotor Predictability (or RSP for short), we examined how effectiveness is dynamically updated as it rapidly changes. We found that response times decrease the more predictable their sensory 'effects' become and also, that when strong predictions are violated, response times significantly decrease. As we recently clarified, the latter effect was found to stem in full from a confound -- the costs induced by switching back from attentional probes to the main task. In this republication, we provide a re-analysis of the original experiments, now with the influence of such switch costs filtered out. We confirm that RSP increases with the predictability of sensory 'effects' (and that the slowing down of its violation stemmed from the confound) in a re-analysis of a yet unpublished data set, two new experiments which do not include probes, and a meta-analysis over 1001 participants. We also describe in full the influence of the confound and discuss the relevance of the results for both the mechanism underlying the 'feeling of agency' and mainstream models of reinforcement learning.

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 ones (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' –controlling the environment – is rewarding, even if the outcome of the effective action is valence-neutral (Eitam et al., 2013; Higgins, 2011; Hull, 1943; Wen et al., 2018; White, 1959); potentially, because selecting effective actions is adaptive (Skinner, 1953).

Indeed, infants' movements were found to be reinforced when they effectively manipulated 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 initially 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), motivation from control was explained using the Comparator model (Blakemore, Frith, & Wolpert, 1999).

The model specifies that once a motor plan is selected for execution, 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.

The dynamic evaluation of a motor program's effectiveness

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[1] 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) led some scholars to recently argue that the lack of a sensory prediction error (SPE) is not sufficient for agency and that (some) causal inference is also required (Zaadnoordijk et al., 2019). In the current study our focus is not on the person-level 'judgement of agency' (Gallagher, 2012; Synofzik et al., 2008), but is more akin to what has been termed the 'feeling of agency' which is closer to the inverse of-SPE. Elsewhere we suggest a comprehensive differentiation between the judgement of agency and this SPE-based process (Hemed et al., 2022; Karsh & Eitam, 2015; Karsh & Eitam, 2015), but this is beyond the scope of the current paper.

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 evaluation of an action's effectiveness see Hemed et al., 2022).

One limitation of the comparator model's current state (the theoretical cornerstone for much of the research on agency) - is that in its current form at least, 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 motor program turns effective and vice versa.

It has been empirically shown that 'deviant' or novel, yet contextually-invariant, environmental contingency (e.g., spatially offset from actual movement, e.g. Blakemore et al., 1998) or temporally lagged sensations (Blakemore et al., 1998; Karsh et al., 2016; but see Wen, 2019 and Wen & Imamizu, 2022 for exceptions) 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. Similarly, other experiments consistently showed that when sensory effects appeared in a prior- 'deviant' yet fixed offset (a 'flash' at a fixed distance from the target object, rather than overlapping it; Karsh et al., 2016) RT facilitation appeared. This is also the case for more standard motor adaptation studies in which SPE is clearly shown to modulate behavior through the internal model. Therefore, the 'Comparator' should have some flexibility regarding adapting to novel environments, by adjusting the predicted feedback (after enough learning) with temporal lag, potentially, being a special case (cf. Wen 2019; Wen & Imamizu, 2022).

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 (see Gutzeit et al., 2023).

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 (Press, Thomas & Yon, 2022; 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 clarification and republication of Hemed et al., (2020)

The current study is a republication of a published paper which was recently clarified on our request (Hemed et al., 2020). On May 2022, about three years after the acceptance of the above paper, while analyzing the data for a second manuscript which is based on this task and design (Hemed & Eitam, 2022), we uncovered a confound introduced by the manner we analyzed our data. On June 2022 we reached out to the editorial board on *Journal of Experimental Psychology: General* with a request to correct or retract-and-replace our previous paper. See Table 1 below for a specification of the process.

The confound primarily involved unwittingly mixing the influence of switch costs from attentional probes back to our main task with our effect of interest – the influence of recent history of sensorimotor effects on the modulation of the influence of a sensorimotor effect on RT of the immediately consequent trial (see figure 1B for the above switching costs on trials t+i following switching back from the attentional probe to the main task). In the current paper 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 paper and a more recent one (Hemed & Eitam 2022).

Table 1: A timetable of the clarification and re-publication process

PERIOD	ITEM
12/2017-05/2018	Data collection for Experiment 1a (Experiment 1 on original manuscript)
05/2019	Data collection for Experiment 2a (Experiment 2 on original manuscript)
01/2020-12/2020	Data collection for Experiment 2b (Replicating Experiment 2a as a pilot for remote data collection)
04/2022	Preparation of related manuscript (Hemed & Eitam, 2022)
05/2022	Discovery of confound and internal investigation
06/2022	Reaching out to editorial board
06/2022-08/2023	Data collection for Experiments 1b and 2c
08/2023-09/2023	Preparation and re-submission of amended manuscript

The current study

We use 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), to no 'flash', or to other forms of visual feedback. Participants' responses were faster in the immediate '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

Establishing the length of the running window accumulating the history of effects

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 given that we use a Bayesian stopping rule (Dienes, 2011) after reaching the intended sample size, we continued to run subjects until the end of the term, resulting in 127 subjects. Since the pattern of results is

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[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.

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 [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. Demographics data from one participant were not collected. Participants sex demographics were collected using a radio-button style question included in the demographics questionnaire, under the prompt 'My Sex is –' and were able to choose from the options *Female* and *Male* or skip the question (by calling the research assistant). None skipped the question, 98 identified as female and 28 as male. Participants were not asked to report their race, ethnicity or religion.

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 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.

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

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[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.

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.

Capturing the Dynamics of Effectiveness

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 (based on the results from a little over 1,000 participants) 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.
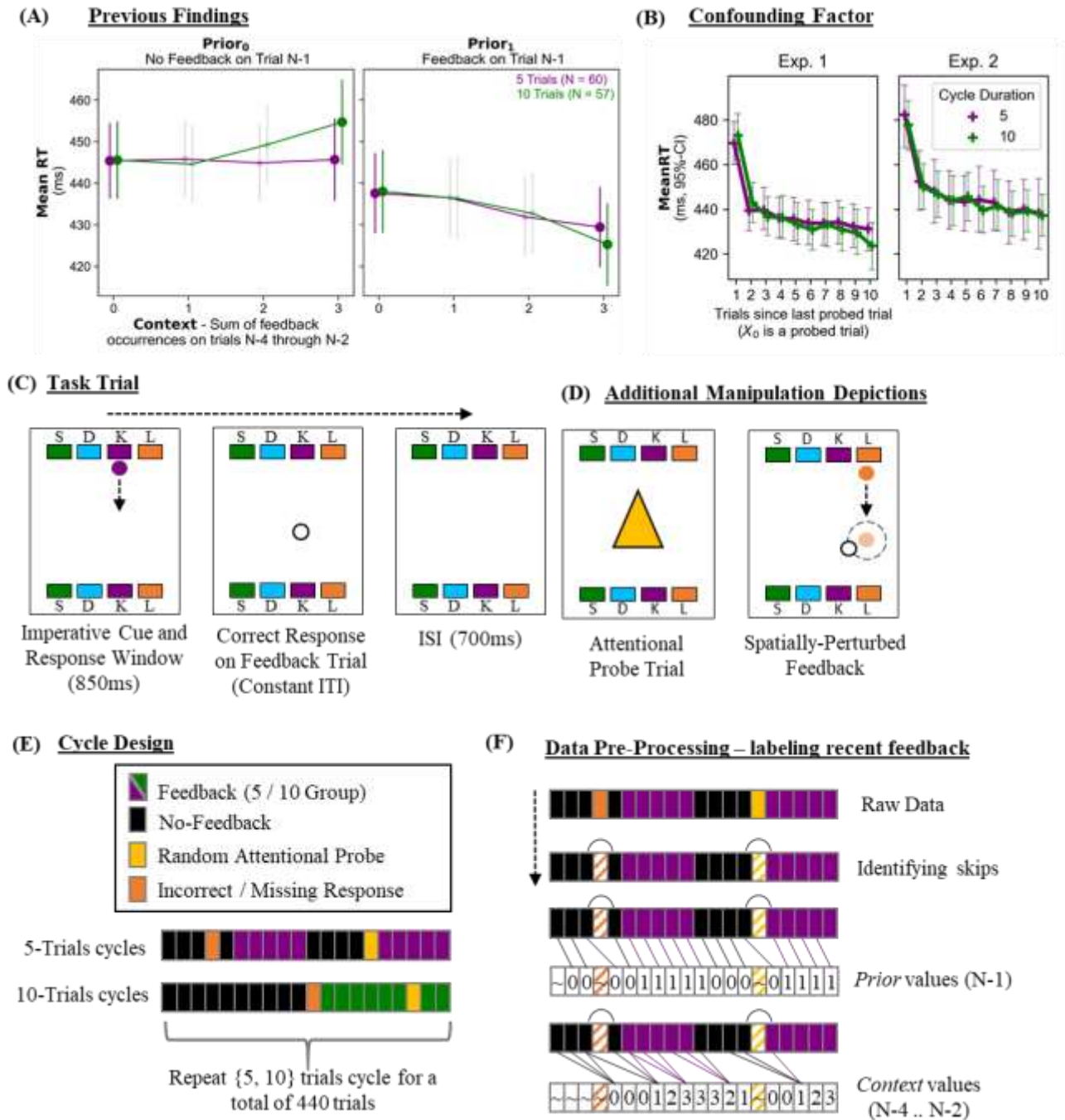
Figure 1: **General design and preprocessing. (A) Results from Experiment 1 on Hemed et al., (2020), using the confounded preprocessing. (B) The confounding factor, a task switching cost from the attentional probes. Note that the probed trials were never analyzed. (C) Participants' correct responses on feedback trials caused an imperative cue to flash and disappear. (D) 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". (E) Pending experimental group, feedback and no-feedback were cycled either every 5 or 10 trials repeatedly. (F) 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.

<u>Results</u>

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-

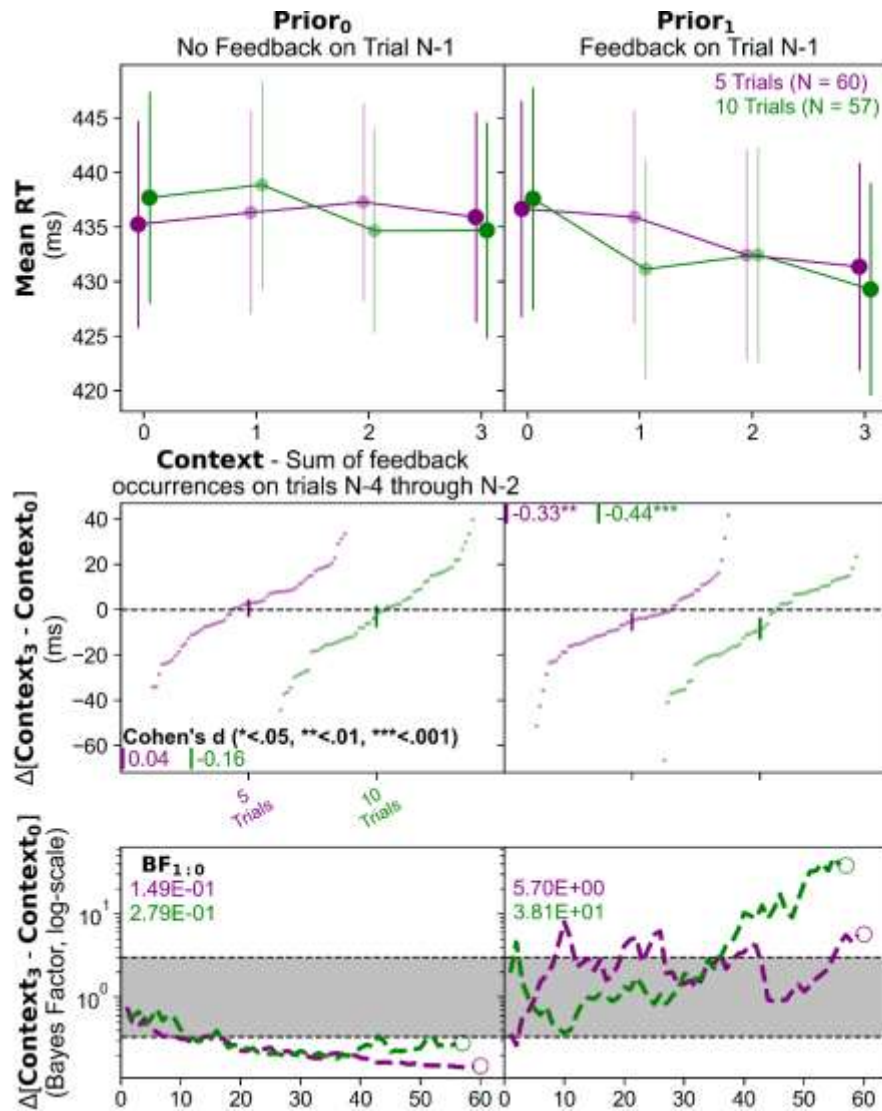error slowing and task-switching costs (see above).

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 = .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 = .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 fully 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 immediately after a previously predictable sensorimotor effect becomes unpredictable.

Experiment 1b – A replication of Experiment 1a but without attentional probes

Experiment 1b was conducted to investigate how the removal of the attentional probes we used on previous experiments and created a confound within our design will affect our results as well as test our statistical (filtering) solution of that confound. As it is possible that the cost of switching from attending to the attentional probes still influenced our results even though we 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).

<u>Methods</u>

In Experiments 1b we again used the absence of feedback to degrade effectiveness as in 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 119 participants. The participants were aged 18.0-45.0 (M = 26.39, SD = 6.24). Participants sex demographics were collected using a radio-button style question included in the questionnaire, under the prompt 'I'm a-' and were able to choose from *Female, Male, Other* and *Rather not say*. 87 identified as female and 32 as male.

Design

The visual workspace was bounded between two rows of rectangles (see Figure 1B) and occupied ⅔'s of the height of a 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 time-related parameters of the experiment were identical to Experiment 1a. Following a slide showing reminders on the task objectives, participants continued to perform an experimental block of 440 trials, as in 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 (7.33%) or missing (4.85%) responses, task trials with extremely fast (RT < 100, 0.28%) or slow RTs (RT > 750, 0.97%). Next, we removed the data from a total of 17 participants (14.29% out of 119) because their accuracy on task trials was below < 80% (N = 15), or because less than 80% of the trials were valid in terms of either RT, accuracy or both (N = 17). In total, 21.26% of the task trials were thus removed, by filtering whole participants' data or individual trials. Note that out of the clean data, we select only trials which immediately follow a correct-response task trial.

Analysis

The effect of Feedback on trial N-1 (*Prior*) on the response time of trial N was significant [$F(1, 100) = 26.14$, $p = 0.001$, Partial Eta-Sq. $= 0.21$]as well as the effect of Context [$F(3, 292) = 4.98$, $p = 0.002$, Partial Eta-Sq. $= 0.05$]. In contrast, the effect of *Cycle Duration* was not significant [$F(1, 100) = 0.07$, $p = 0.786$, Partial Eta-Sq. < 0.01]. As per interactions, the interaction of *Prior* X *Context* was statistically significant [$F(3, 277) = 8.09$, $p = 0.001$, Partial Eta-Sq. $= 0.07$], but not that of *Prior X Cycle Duration* [$F(1, 100) = 2.03$, $p = 0.157$, Partial Eta-Sq. $= 0.02$] *or Context* X *Cycle-Duration* [$F(3, 292) = 0.17$, $p = 0.912$, Partial Eta-Sq. < 0.01], nor was the 3-way interaction [$F(3, 277) = 1.78$, $p = 0.155$, Partial Eta-Sq. $= 0.02$].
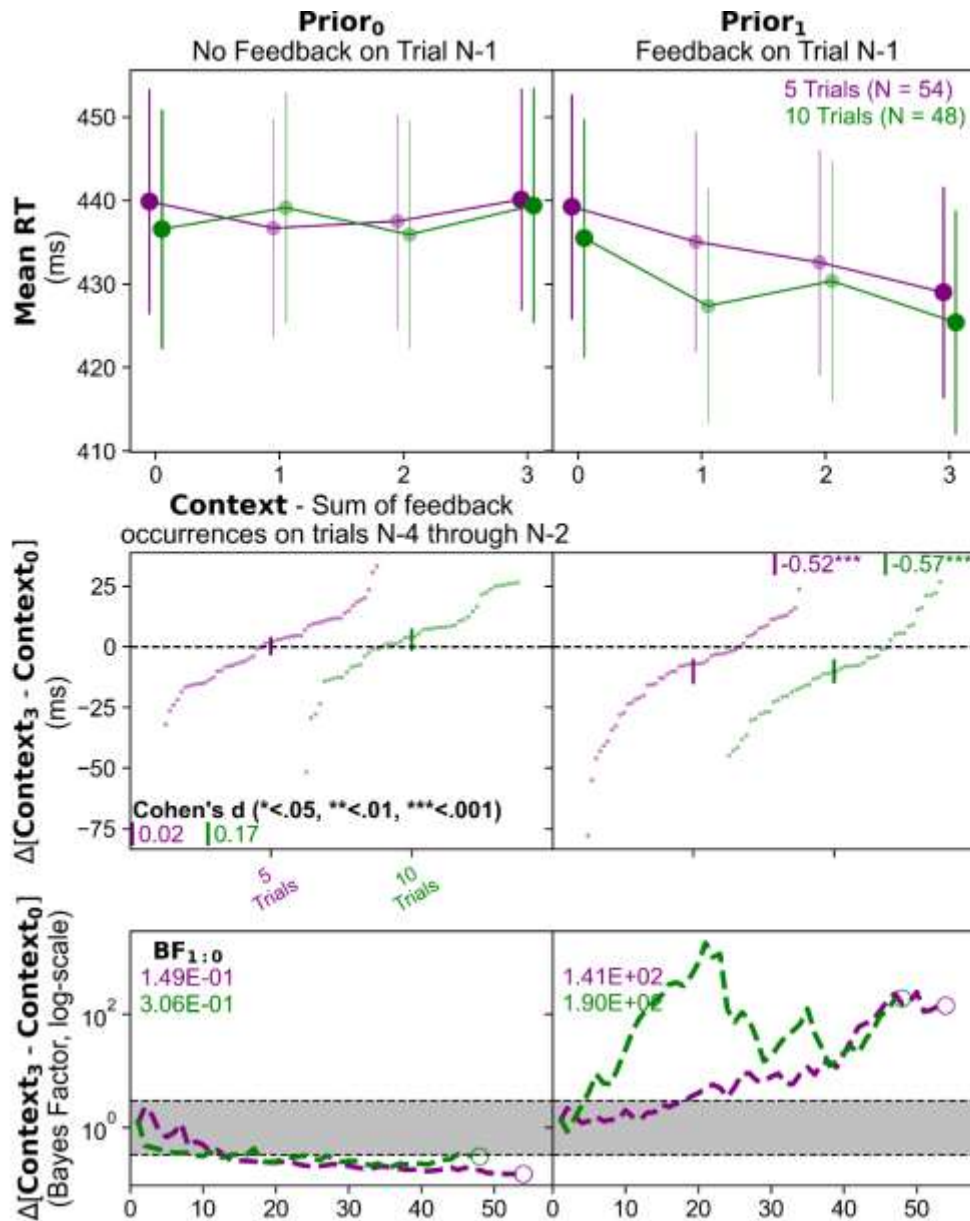
Figure 3. Experiment 1b: A replication of Experiment 1a but without attentional probes. **Top – Response Time by Prior (columns), Context (x-axis) and Cycle Duration (line color). Mid - contrasts and standardized effect size for RT On *Context 3 – Context0* (* < .05, ** <.01, ***<.001). Bottom – Bayes factors obtained using sequential Bayesian t-tests.**

First, we find 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 [-10.24 MS (19.54); t(53) = -3.82, p < 0.001, Cohen's d = -

0.52, (-0.80, -0.23), BF1:0 = 140.9214]and 10-trials groups [-10.09 MS (17.55); t(47) = -3.94, p < 0.001, Cohen's d = -0.57, (-0.87, -0.26), BF1:0 = 190.2249].

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 [0.24 MS (14.31); t(53) = 0.12, p = 0.903, Cohen's d = 0.02, (-0.25, 0.28), BF1:0 = 0.1495] or the 10 trials Cycle-Duration group [2.83 MS (16.24); t(47) = 1.19, p = 0.238, Cohen's d = 0.17, (-0.11, 0.46), BF1:0 = 0.3059].

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, and that 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 dynamics 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. Participants sex demographics were collected using a radio-button style question included in the demographics questionnaire, under the prompt 'My Sex is –' and were able to choose from the options *Female* and *Male* or skip the question (by calling the research assistant). None skipped the question, 47 identified as female and 32 as male.

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[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.

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.

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, 0.81%). 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]. As per interactions, the key 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].
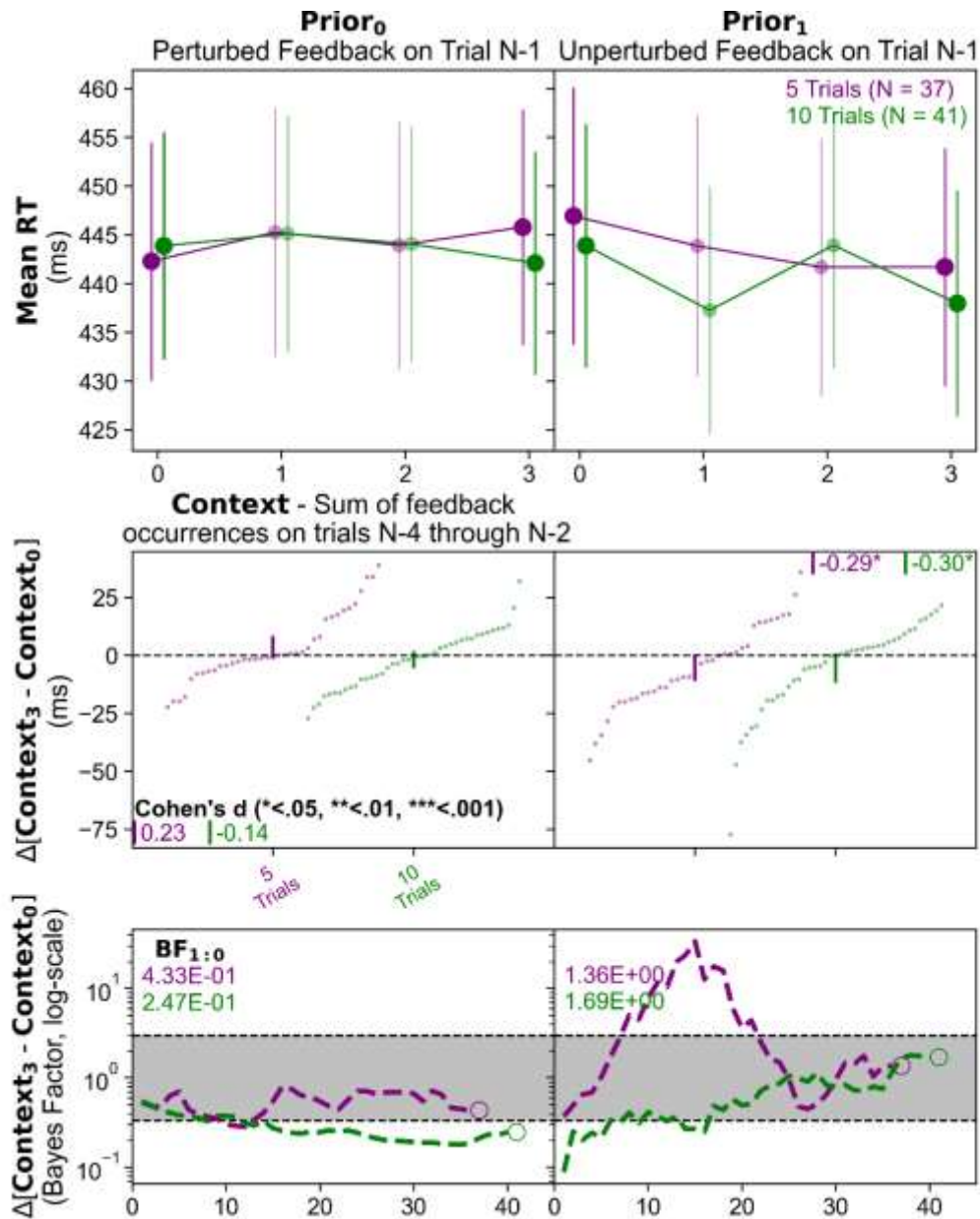
Figure 4, Experiment 2a. Top – Response Time by Prior (columns), Context (x-axis) and Cycle Duration (line color). Mid - contrasts and standardized effect size for RT On *Context 3 – Context0* (* < .05, ** <.01, ***<.001). Bottom – Bayes factors obtained using sequential Bayesian t-tests.

First, we looked at the effect of accumulating feedback, we contrasted trials for which

feedback was given on the response just before (*Prior1*), following an uninterrupted streak of three

previous responses that were followed by feedback (Context3) facilitated responding compared to

an 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 the Bayes factor is not conclusive.

Conversely, when holding Feedback on trial N-1 at 0 (*Prior*$_0$), we find 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 for 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's 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 hence we rely on the additional (new) data from Experiments 2b-2c for further evidence.

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

Experiment 2b – A replication of Experiment 2a

Experiment 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 supply another test for the results 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). Participants sex demographics were collected using a radio-button style question included in the demographics questionnaire, under the prompt 'My Sex is –' and were able to choose from the options *Female* and *Male*, 73 identified as female and 31 as male.

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, 1.03%). 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, the

key interaction between *Prior* and *Context* [$F(3, 278) = 8.71$, $p = 0.001$ was significant, Partial Eta-Sq. = 0.08], but not the *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].
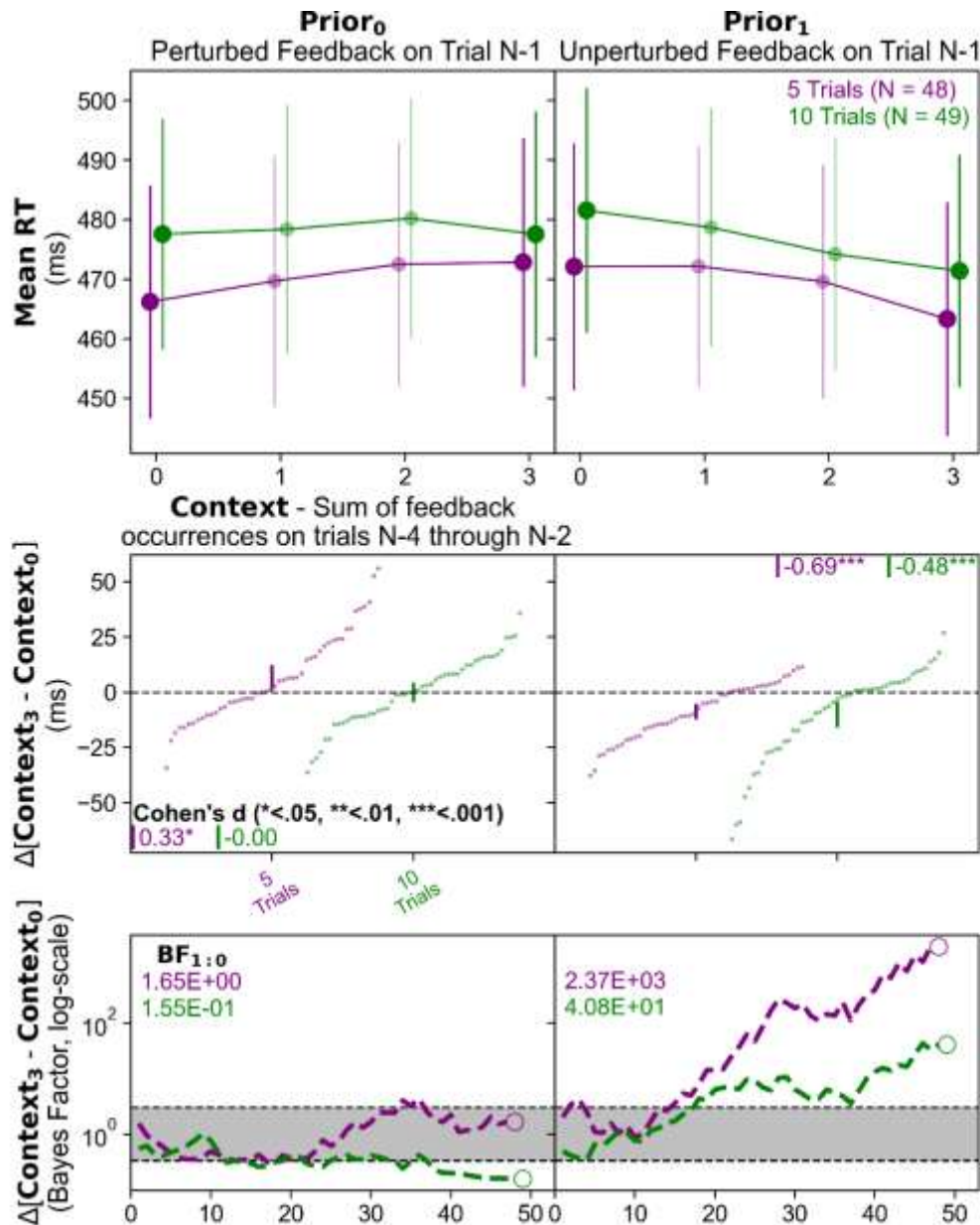
Figure 5. Experiment 2b: A replication of Experiment 2a. Top – Response Time by Prior (columns), Context (x-axis) and Cycle Duration (line color). Mid - contrasts and standardized effect size for RT On *Context $_3$ – Context$_0$* (* < .05, ** <.01, ***<.001). Bottom – Bayes factors obtained using sequential Bayesian t-tests.

Next, we looked at the two different contrasts. First, we found that if Spatially-Predictable feedback was given on trial N-1 (Prior1), a streak of Spatially-predictable Feedback trials (Context3) compared with a streak of Spatially-Unpredictable Feedback trials (Context0) 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 Experiments 1aand 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 introduced more variability as did its interaction with the value of *Prior*.

Experiment 2c – A replication of Experiments 2a & 2b but without attentional probes

Experiment 2c was a direct 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 282 participants. The participants were aged 18.0-45.0 (M = 26.17, SD = 5.82). Participants' gender demographics were collected using a radio-button style question included in the questionnaire, under the prompt 'I'm a –' and were able to choose from *Female*, *Male*, *Other* and *Rather not say*. 186 identified as female, 91 as male,2 chose Rather not say, and 3 skipped the question altogether.

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 Experiment 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 (8.18%) or missing (2.35%) responses, task trials with

extremely fast (RT < 100, 0.18%) or slow RTs (RT > 750, 0.65%). Next, we removed the data

from a total of 29 participants (10.28% out of 282) where their accuracy on task trials was below

< 80% (N = 27), or where less than 80% of the trials were valid in terms of either RT, accuracy

or both (N = 29). In total, 17.57% of the task trials were removed, by filtering whole participants'

data or individual trials.

Analysis

The effect of Spatially Predictable Feedback on trial N-1 (*Prior*) and *Context* on

response time was significant [$F_{(1, 251)}$ = 30.70, p = 0.001, Partial Eta-Sq. = 0.11] and [$F_{(3, 741)}$

= 4.76, p = 0.003, Partial Eta-Sq. = 0.02]respectively, the effect of Cycle Duration was

insignificant [$F_{(1, 251)}$ = 0.53, p = 0.467, Partial Eta-Sq. < 0.01]. No interaction was statistically

significant - *Prior* X *Context* [$F_{(3, 741)}$ = 1.27, p = 0.285, Partial Eta-Sq. = 0.01]*, Prior X Cycle-*

Duration [$F_{(1, 251)}$ = 0.09, p = 0.762, Partial Eta-Sq. < 0.01], *Context* X *Cycle-Duration* [$F_{(3, 741)}$ = 0.76, p = 0.513, Partial Eta-Sq. < 0.01], as well as the 3-way interaction [$F_{(3, 741)}$ = 0.61, p = 0.603, Partial Eta-Sq. < 0.01].
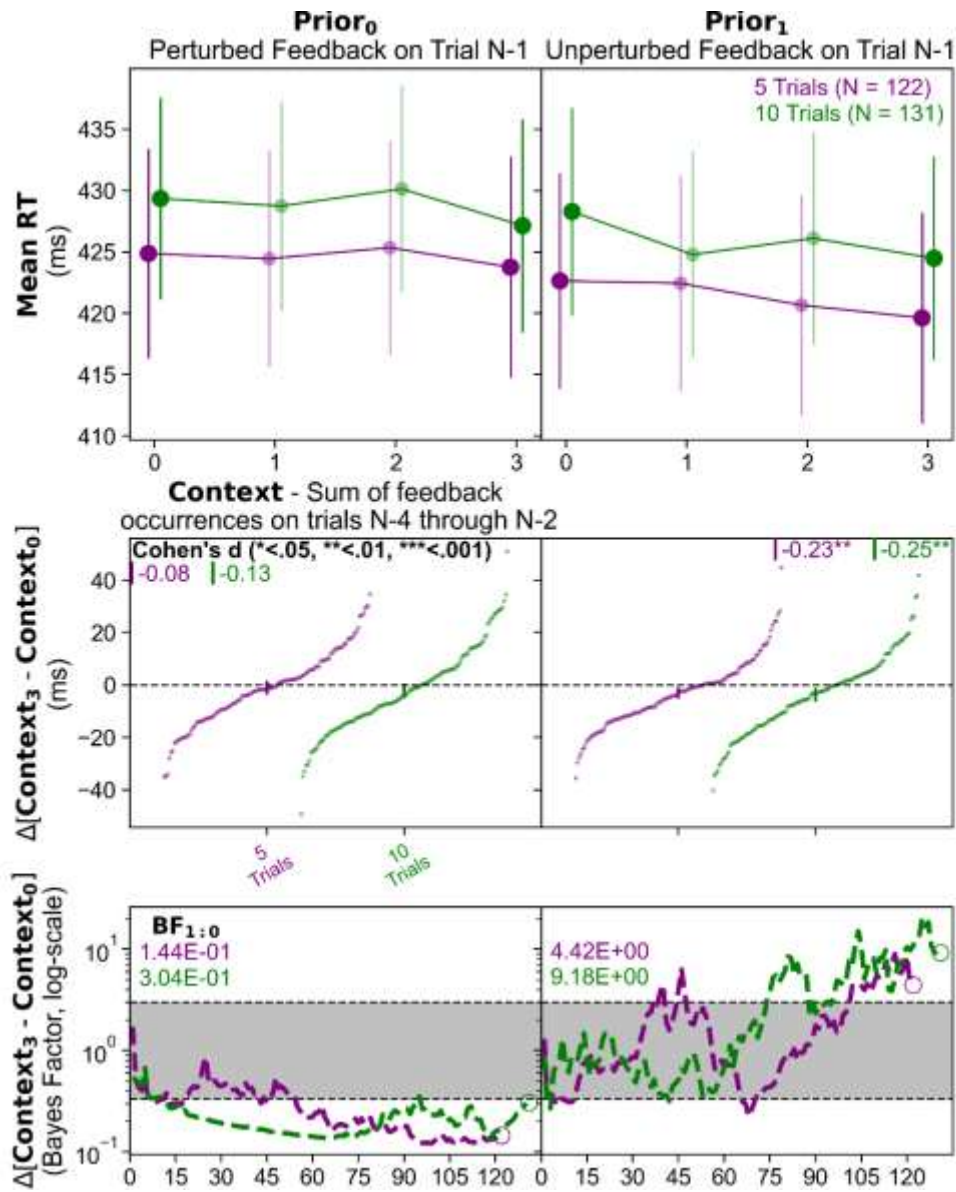
Figure 6. Experiment 2c: A replication of Experiments 2a & 2b but without attentional probes. Top – Response Time by Prior (columns), Context (x-axis) and Cycle Duration (line color). Mid - contrasts and standardized effect size for RT On *Context 3 – Context0* (* < .05, ** <.01, ***<.001). Bottom – 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 [-3.02 MS (13.02); t(121) = -2.55, p = 0.006, Cohen's d = -0.23, (-0.41, -0.05), BF1:0 = 4.4247], as well as the 10-trials group [-3.80 MS (15.20); t(130) = -2.85, p = 0.003, Cohen's d = -0.25, (-0.42, -0.07), BF1:0 = 9.1835], both with conclusive support for the alternative hypothesis.

Next, we tested the effect of context on RT in trial N when Spatially-Unpredictable feedback was given on trial N-1. We find 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 affect response speed for the 5-Trials Cycle Duration group [-1.12 MS (14.36); t(121) = -0.86, p = 0.393, Cohen's d = -0.08, (-0.26, 0.10), BF1:0 = 0.1439] or the 10 trials Cycle-Duration group [-2.22 MS (16.47); t(130) = -1.53, p = 0.128, Cohen's d = -0.13, (-0.31, 0.04), BF1:0 = 0.3041], with conclusive support for the null on both cases.

Discussion

Experiment 2c was intended to again test whether the re-analyzed results of Experiments 2a-2b hold, when re-running the same design without attentional probes. Thus, it can be used to rule out the threat that our previous results emerged in full due to task-switching costs due to the use of attentional probes. This experiment also weakens the explanation of reinforcement only occurring due to lack of error signal compared with Experiments 1a-1b where 'ineffective' cycles included no-feedback altogether rather than a spatially perturbed one. Generally, it

provided strong support for our hypothesis that recent spatially-predictable feedback facilitates response speed, pending the occurrence of less recent feedback.

However, the effect of recent experience with 'being effective' was found to be contingent on a response continuing to have the same effect. It also provided us with a somewhat unique pattern, where for the first time we observed a much weaker (and insignificant) facilitation effect in the 10-trials duration group, was nominally slower in general.

Due to the variance in the strength of evidence, the vindication of our filtering treatment of the confound and our wish to have the best possible estimations of the effects, we chose to pool the results from multiple experiments and conduct a meta-analysis of the effect sizes from the two critical contrasts.

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 1,001 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 (N = 489) there was a medium effect (Cohen d = -0.49, p = .005), while for the 10-trials group (N = 512) the effect was (unpredictably) slightly smaller (Cohen's d = -0.38, 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 feedback ('effectiveness') on response speed is considerably affected by the less recent history of effectiveness (see Figure 9, top panel).

Conversely, and in a contrast to our 2022 publication, we now know that the effect of immediately *lack of effectiveness* on RT is not modified by (even substantial) 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 0.12 (p = .275) and -0.04 (p = .935), for the 5-trials and 10-trials groups respectively (see Figure 9, bottom panel).
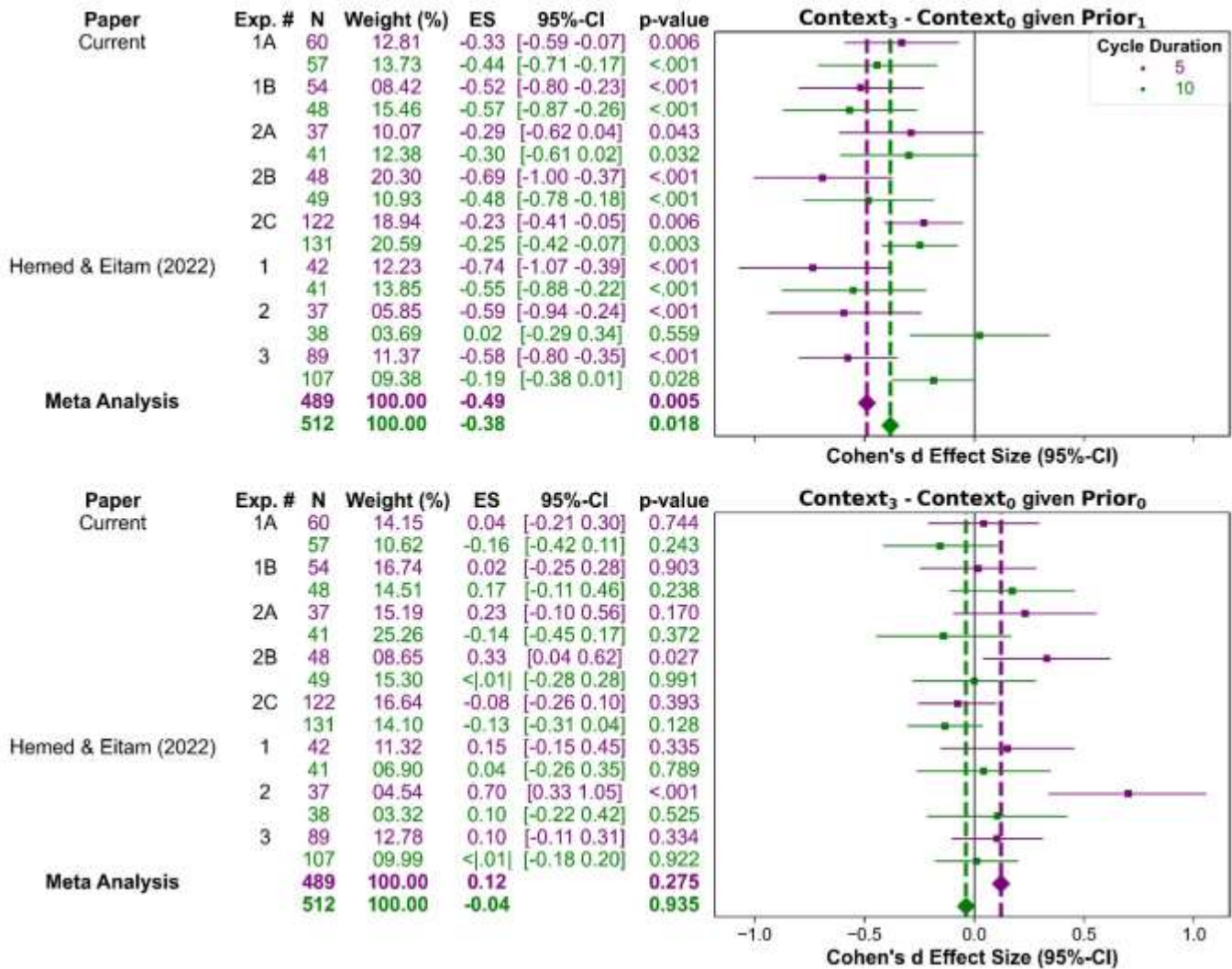
Figure 7: Meta-analysis of the effect of change in feedback on response time. Leftward (negative) values denote a decrease in response time (i.e. faster responding). Rightward (positive) values denote increases in response time (i.e. slower responding). Vertical dashed lines denote the average effect size across all experiments, per cycle duration. Top – RT on trial N following perceptual feedback on trial N-1. Bottom – RT on trial N following no perceptual 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 reinforcement from sensorimotor predictability 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 clarifying how 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 into 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 now seemingly 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 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 lack 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 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 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 role for precision?

It is interesting that here (differently from our previous and misguided analysis), we do not find that the 10-trials group slowed down when switching to a No-Feedback cycle (i.e., $Context_3$ VS. $Context_0$, given $Prior_0$). 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 in full an outcome of the interference from task-switching between attentional probes and the main task. One could hypothesize that the data filtering process we used on Experiments 1a and 2a-2b obliterated this

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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.

difference, but it was still not found on Experiments 1b and 2c where we did not use any probe trials (and hence also no post-probe trials).

While larger changes in response speed for the 10-trials group (where precision is higher) were predicted, the current data does not support the precision hypothesis as the effect sizes we obtained (see Meta-Analysis) for facilitation are, if anything, larger for the 5-trials group compared with the 10-trials group (.49 and .38, respectively). While it is still 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 currently but a speculation[6].

Conversely, some support for the precision hypothesis may be garnered from the fact that the 10-trials group RT's were more 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 focused research on the modulation of task-switching costs by different levels of precision in sensorimotor effects.

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 as introducing positive and negative reward prediction errors iteratively, as participants receive unexpected feedback or no

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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 add to the Comparator model's variants.

feedback for their actions (without arguing that our manipulation modifies the 'Sense of Agency').

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),) the action which led to a maximal reward was periodically changed which was intended to induce reward prediction errors (RPE's); DA activity indeed matched a recency weighted prediction, based on positive and negative RPE in past trials(Parker et al., 2016 found, under some conditions, underweighing of past negative RPEs).

Here we find behavioral evidence for the influence of previous reinforcing events (i.e. before the immediately previous trial) *immediately* after a reinforcer occurs (i.e., action-effects; $Prior_1$), but no such influence of previous reinforcing events when a response has just led to no feedback ($Prior_0$).

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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).

A different issue is that one may altogether reject the notion that own-action effects are reinforcing, and argue that our task simply introduces a series of sensory prediction errors (SPE's). But an alternative that rejects a role for reinforcement altogether (which is different than rejecting the involvement of 'reward') must explain how increasing 'effects' accumulate to facilitate RT while introducing SPE does not decrease it. Regardless, it is indeed possible that SPE (or its reduction) plays a role in the reinforcement process we suggest. In contrast to RPE, the monitoring of SPE following motor actions was attributed to the Cerebellum (Blakemore et al., 1998; Ishikawa, Tomatsu, Izawa, & Kakei, 2016; Schlerf, Ivry, & Diedrichsen, 2012; Sokolov, Miall, & Ivry, 2017; Tseng, Diedrichsen, Krakauer, Shadmehr, & Bastian, 2007). Interestingly– 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 may 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 conclude 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; Karsh et al., 2016; 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'). Maybe, but harnessing a sensory prediction error (SPE) or its derivate as input to reinforce error-decreasing motor programs. Such an 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-clarified; Hemed et al., 2020). Our experience with erring and its correction 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-correction will promote transparency and hopefully more accurate and efficient scientific progress.

Significance Statement

Human beings live in dynamic environments and their brains need to evaluate the effectiveness of their motor actions to survive. It is not clear whether and how information about the effectiveness of a movement is integrated over time and used to increase and decrease the movement as its effects become more or less reliable (i.e. predictable), respectively. Here we amend a previous paper of ours in which manipulated the 'effect' participants' responses had over their environment and measured their response speed to imperative cues but also inserted a confound due to the influence of attentional probes. Our emended results continue to support the finding that human behavior is highly sensitive to subtle changes in the effectiveness of its motor-plans. We also find that the integration of recent experience interacts in a complex manner with previous experience. Specifically, the pattern we report is is reminiscent of the stronger weighting of recent evidence found in animal studies on reward prediction when effects accumulate; but diverges from it when effectiveness is suddenly gone – where (even consistent) previous effectiveness seems not to play a role at all in reinforcing the suddenly ineffective behavior. These behavior results supply further evidence for the differential working of reinforcement on different levels of the response selection process. movement reinforcement is largely extinct within a single unrewarded trial.

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

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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