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Effect Monitoring in Dual-Task Performance

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Actions aim to produce effects in the environment. To accomplish this properly, we not only have to recruit the appropriate motor patterns, but also we must be able to monitor whether an intended effect has ultimately been realized. Here, we investigated the impact of such effect monitoring on performance in multitasking situations: Multitasking basically means to produce and monitor multiple actions and effects in fast succession. We show that effect monitoring cannot run in parallel, without causing processing decrements, with a second task. Also, monitoring of effects that are spatially incompatible to a response seems to take longer than the monitoring of spatially compatible action effects (Experiments 1 through 4). We further argue that effect monitoring is essential toward learning of response-effect associations, as it captures not only anticipated action effects, but also unpredictable occurrences in the environment (Experiments 5a and 5b).

Keywords: action control, ideomotor theory, action effects, psychological refractory period, effect monitoring

Writing a research paper is hard. Not only do you have to get your arguments in line, provide enlightening empirical observations to support your claims, and illustrate your results in a way that they are easily accessible to the reader, but after that is all done, you still have to write it down.[1] And as every scientist with insufficient typing skills has experienced at least once, that part can be hard work as well. Typing consists of short coordinated actions in fast succession, which, with increasing speed, increasingly resemble multitasking (Logan & Crump, 2009; Salthouse, 1986). As with performance in most multitasking situations, accuracy suffers with shorter intervals between keystrokes (Kinkead, 1975). But especially in this scenario, the typist's work is not done by engaging in the keyboard. After that, the action is not completed, but the effect that this action produced (a letter on the screen) has to be checked against the letter that the typist intended to produce, thereby monitoring for possible errors (Logan & Crump, 2010).

Dual-Tasking Performance

Psychological models of multitasking—or more precisely: dual-tasking—have mostly neglected cognitive processes after a response is executed. Consider the *psychological refractory period* (PRP) effect. If two tasks have to be performed separated by a short or a long stimulus-onset-asynchrony (SOA), performance in the first task (Task 1) is usually unaffected by the degree of temporal overlap, whereas performance in the second task (Task 2) heavily suffers the shorter the SOA, with longer response times (RTs) and higher error rates (Telford, 1931). A widely accepted explanation for this PRP effect assumes at least one central stage of information processing somewhere in between encoding stimuli and executing a motor response, which is capacity-limited and thus cannot run in two tasks at the same time, hence establishing a bottleneck of cognitive processing (Pashler, 1994)—or at least cannot run with the same efficiency (Navon & Miller, 2002; Tombu & Jolicoeur, 2003). Consequently, the concurrent operation of this process is considered to be either impossible or at least avoided for strategic reasons (e.g., Meyer & Kieras, 1997; Miller, Ulrich, & Rolke, 2009) or because of contextual aspects (e.g., Fischer, Gottschalk, & Dreisbach, 2014; Janczyk, 2016a). Most often this particular stage is referred to as response selection. If a situation requires simultaneous performance in two tasks, but response selection of Task 1 is already ongoing, the processing of Task 2 has to wait until this central stage of Task 1 is completed, thereby lengthening RTs in Task 2 as a function of the SOA. On the basis of this idea, an effect is propagated with strongly overlapping tasks, such that any manipulation that influences the length of the perceptual or central stage in Task 1 not only affects the RTs

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[1] All of which the authors hope to achieve on the following pages.

of Task 1, but also of Task 2, as the central stage of Task 1 finishes later and thus occupies the bottleneck longer, which consequently delays the processing of Task 2 as well (*effect propagation logic*; e.g., Janczyk, Renas, & Durst, 2017; Kunde, Pfister, & Janczyk, 2012). Events that happen after a response is executed, such as sensory effects produced by this response, were so far not considered in the respective models.

The Ideomotor Approach

To us, however, taking action effects into account appears important though, because numerous studies have meanwhile documented that action effects can influence performance, even though they appear only after the response is executed (e.g., Badets, Koch, & Toussaint, 2013; Gaschler & Nattkemper, 2012; Janczyk, Durst, & Ulrich, 2017; Janczyk & Kunde, 2014; Janczyk, Skirde, Weigelt, & Kunde, 2009; Paelecke & Kunde, 2007; Pfister, Janczyk, Wirth, Dignath, & Kunde, 2014; Pfister & Kunde, 2013), also in dual-task situations (Janczyk, 2016b; Janczyk, Pfister, Crognale, & Kunde, 2012). For example, it is easier to produce a left visual stimulus with a left response key press (compatible response–effect [R–E] mapping) compared to a right response key press (incompatible R–E mapping; Kunde, 2001), with longer RTs (and more errors) in the incompatible mapping. Such studies are inspired by the idea that humans have no direct access to their muscle activities and thus their motor output. What they can access, however, are codes of the perceptual changes that follow from these muscles activities. This so-called ideomotor approach assumes that motor actions are mentally stored and retrieved in terms of the sensory effects that movements produce. Selecting a response thus means to anticipate its sensory effects (e.g., Harleß, 1861; Hommel, Müsseler, Aschersleben, & Prinz, 2001; James, 1890; Kunde, Elsner, & Kiesel, 2007; Shin, Proctor, & Capaldi, 2010).

Admittedly, there seems to be not much to result from a simple keypress with, let's say, the right index finger, except the tactile and proprioceptive feedback from pressing the key and the click-sound. However, when motor patterns reliably produce certain visual or auditory events, it becomes apparent that anticipation of these effects might in fact contribute to action generation. As in the above-mentioned example, it takes longer to generate a keypress that produces visual feedback which is incompatible rather than compatible to the keypress itself (and its respective body-related effects).

In choice reaction experiments, there are some indications that anticipating action effects takes place right at the point in time where traditional models of dual-task performance place the "response selection" process. First, removing response selection from the reaction time (RT) interval by preparing responses in advance reduces influences of action effects on RTs (Kund, 2004; Shin & Proctor, 2012; Wirth, Pfister, Brandes, & Kunde, 2016). Second, influences of R-E compatibility, which presumably originate from anticipated effect codes, combine additively with SOA when compatibility is manipulated in Task 2 of a PRP design. Such additive influences are traditionally attributed to the response selection stage (Kunde, Pfister, & Janczyk, 2012; Wirth, Pfister, Janczyk, & Kunde, 2015). Interestingly, influences on RTs that originate from action effects that are also used as imperative stimuli do not show such a pattern, possibly because these effect-stimuli need to not to be anticipated, since they are already perceptually present prior to responding (cf. Paelecke & Kunde, 2007, for a discussion). There is thus evidence that (a) even simple keypress responses can be construed as effect-oriented actions, and (b) that the anticipation of effect codes might contribute to the capacity-limited processes causing dual-task decrements (cf. also Janczyk, 2016b; Janczyk, Pfister, Hommel, & Kunde, 2014).

Against this background, it becomes rather obvious that a task is not just completed by emitting a certain motor pattern (the response). Humans generally have to make sure that they eventually attain their intended effects. In other words, they have to monitor the effects of their behavior. Consequently, it would be helpful to maintain codes of action effects in an active state beyond response selection, to use them as reference values to which actually incoming perceptual changes can be compared. In fact, there is evidence suggesting that action effect codes remain in an active state for some time after response execution, possibly to serve as such reference values. For example, Desantis, Roussel, and Waszak (2014) recently showed that predicted effects were more accurately identified than unpredicted effects, up to at least 280 ms after action execution, suggesting that codes of anticipated effects outlast response execution considerably. Moreover, encountering unpredicted action effects seems to induce electrophysiological responses similar to those induced by error feedback (Band, van Steenbergen, Ridderinkhof, Falkenstein, & Hommel, 2009).

Effect Monitoring

On the basis of these considerations, we can infer that if an action produces an action effect (e.g., typing a letter on a screen), action control heavily revolves around the effects that these actions will produce. Stimuli (e.g., preceding letters on the screen) signal which effect (to-be-produced letters and words on the screen) has to be produced, and the anticipation of this effect recruits the motor patterns that are eligible to realize this anticipated effect. But if the sole purpose of an action is to produce an effect, there must be a way to make sure that the intended effect was ultimately realized.

So, although theoretical considerations suggest that humans in general monitor the sensory consequences of their actions, empirical evidence is still scarce. Plus, it is less clear whether such monitoring is a capacity-limited process that itself affects dual-task performance, as was envisaged, however, already by Welford (1952):

> The central mechanisms are liable to become engaged by stimuli fed back from the response, particularly from those parts of it where there is rapid acceleration or deceleration of movement, or where some definite sound or visual or tactile change is produced. The perception of any such feedback data will require central organizing time. (p. 18)

There is also evidence from the dual-task literature that might be reinterpreted as preliminary evidence for a capacity-limited effect monitoring process. For example, in Task 1 of a PRP experiment, participants had to move a lever with either a small (70 mm) or large (185 mm) amplitude as the response to an auditory stimulus (Ulrich et al., 2006). Large amplitudes produced longer RTs and movement times than short amplitudes did, and, most importantly, this effect was also visible in RTs of an unrelated but concurrent Task 2, which required a manual response (index- or middle-finger) to a visually presented letter. These results could index interference from effect monitoring, if we assume that large movements engage a longer lasting monitoring of tactile and proprioceptive feedback, which delays central processing in Task 2 longer compared to movements with small amplitudes (cf. also Bratzke, Rolke, & Ulrich, 2009).

Another interesting observation in this respect is the so-called residual PRP effect: the observation that RTs in a second task increase, the closer the stimulus for the second task occurs after the response in a preceding first task has been completed. Obviously, there cannot be overlap of response selection processes anymore, because the first response is not only selected but already emitted. Response monitoring (or monitoring of response-related feedback) has been proposed to explain this observation (cf. Jentzsch, Leuthold, & Ulrich, 2007, p. 624), with the idea that a response monitoring process accompanies (and sometimes outlasts) the motor stage, in which stimulus and response features are compared. This response monitoring stage is equally assumed to draw on central resources to explain the delay of Task 2 with shorter response-stimulus-onset-asynchronies (RSOAs). The implications of response monitoring and effect monitoring for dual-task performance will be addressed in the General Discussion.

To conclude, there are reasons to assume that action effects play a role not only in the selection of motor actions, but that such effects are also monitored after action execution. In the present paper we argue that such effect monitoring extends beyond body-related proprioceptive or tactile effects, and that it can be a key determinant of dual-task performance.

Overview of Experiments

To support this proposal, we designed dual-task experiments, in which responses in a Task 1 produced visual events that were predictable (Experiment 1 through 4) or not (Exp. 5). Moreover, the stimulus for Task 2 was presented only after the response for Task 1 was completed. This was done to avoid any overlap of Task 1 processes other than effect monitoring with capacity-limited processes in Task 2. As a tool to reveal effect monitoring, we relied on preliminary evidence that response-compatible visual effects are more easily encoded, and take less time to process, as compared to incompatible effects (cf. Hommel & Schneider, 2002; Müsseler, Wühr, Danielmeier, & Zysset, 2005). Thus, we manipulated the spatial compatibility of the effects to the response in Task 1 (thus R1–E1 compatibility). The stimulus of the second task was presented shortly after the first response was given, that is, the second task was processed while effect monitoring presumably takes place. Consequently, performance in this second task should suffer more while/after monitoring incompatible rather than compatible effects in Task 1, if the monitoring process itself creates interference with processing of Task 2. Experiments 2 and 3 then aimed to identify which of the information processing in Task 2 interferes with effect monitoring. Experiment 4 explored the temporal dynamics of such effect monitoring. Finally, Experiment 5 tested whether monitoring occurs for predictable effects only, or for unpredictable events as well.

Experiment 1

In Experiment 1, participants responded to a visual stimulus (S1, a puzzle piece with connector) with a finger-press response (R1) which immediately produced a visual effect, namely another puzzle piece to the left or right of S1. In the center of the screen, a puzzle piece with a connector on the left, on the right, or on both sides was displayed, and participants were to add a piece by pressing a response button. The location of this visual effect was predictably either compatible or incompatible to the response (R1–E1 compatibility manipulation in Task 1). With this, we expected to replicate the R1–E1 compatibility effect with faster Task 1 responses when followed by compatible rather than incompatible effects (Ansorge, 2002; Chen & Proctor, 2013; Kunde, 2001; Pfister & Kunde, 2013). This is commonly attributed to effect anticipation, because these effects arrive too late (namely after the response) to impact the response as an actual sensory event.

Briefly after R1, the stimulus for Task 2 was presented (S2, a color patch). Hence, S2 can be processed only after response selection in Task 1 was obviously completed. Still the monitoring of the R1 action effects might be going on, and consequently might influence Task 2 performance (with, as hypothesized in the Introduction, a longer delay of Task 2 after incompatible effects compared to after compatible effects). In other words, if we assume there to be a process that is in charge of monitoring self-produced action effects, there is again overlap between the two tasks, which might allow for manipulations that are introduced in Task 1 to propagate to Task 2. If such manipulations affect Task 2 performance, this would be an indication for an effect monitoring process running after response execution. Moreover, this would imply that the monitoring process is somehow capacity-limited. Otherwise no interference would be expected. Conversely, if the manipulation in Task 1 does not affect Task 2, we can assume that there either is no effect monitoring process, or that it can easily run in parallel with any other cognitive processes, contrary to what Welford (1952) suggested (Model A, Figure 1).

In addition to the R1–E1 compatibility we manipulated the type of choice in Task 1, which was either forced choice or free choice (Berlyne, 1957). In free choice trials, there were two connectors (left and right, cf. Figure 1) which allowed the addition of a puzzle piece at either side. In forced choice trials, there was only one connector, either on the left or on the right, and a puzzle piece had to be added there. Consequently, in forced choice trials, the response was not only compatible or incompatible to the produced effect (a left or right puzzle piece; R1–E1 compatibility), but likewise compatible or incompatible to the stimulus (a left or right connector; S1–R1 compatibility). Hence, forced choice trials alone are difficult to interpret, because a possible effect in Task 2 could be caused (a) by a difficulty to monitor spatially incompatible action effects or (b) by aftereffects of responding to a stimulus with a spatially incompatible response (e.g., more attention devoted to Task 1 in this case, what perhaps results in less attention and deteriorated performance in Task 2).

To isolate the effects of the R1–E1 manipulation and attribute a possible propagation to effect monitoring and not aftereffects of S1–R1 manipulations, we had to take the spatial stimulus features out of the equation. That is why, intermixed with the forced choice trials, we presented free choice trials. Here, in Task 1 participants were confronted with a stimulus that allowed for the addition of either a left or a right piece, and participants could choose freely where they wanted to add a puzzle piece. This way, only R1–E1 but not S1–R1 compatibility was manipulated and a possible

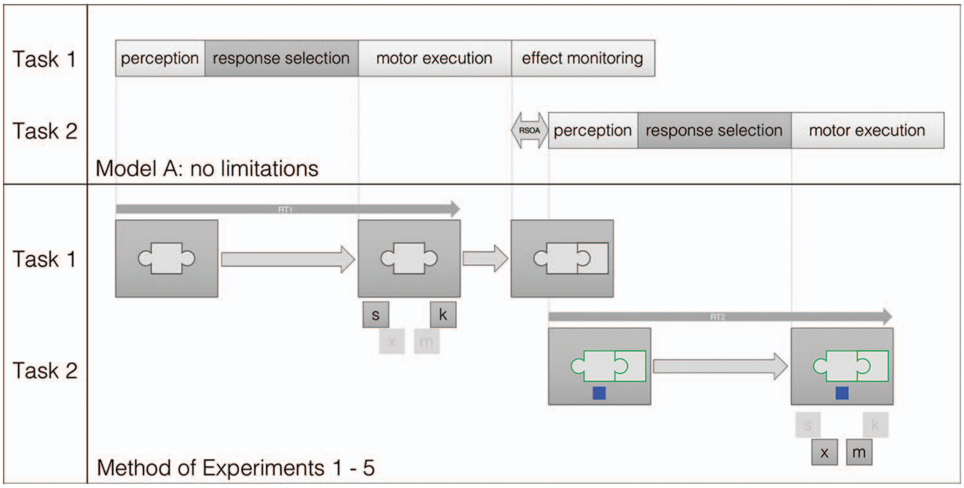

Figure 1. The upper part depicts a dual-task Model A that assumes that effect monitoring in Task 1 is not capacity limited at all. As there is no temporal overlap of capacity-limited processing stages of the two tasks, there should be no propagation of the manipulation of Task 1 to Task 2. The lower part illustrates the trial procedure that was used for Experiments 1–5, and how the procedure relates to the model. In Task 1, a puzzle piece had to be added at the left or right side of a centrally presented piece by pressing a response button. After this response, a stimulus below the puzzle (a color patch in Experiment 1, letters in Experiments 2 through 5) had to be categorized. (RSOA = response-stimulus-onset-asynchrony). See the online article for the color version of this figure.

propagation of a manipulation in Task 1 to Task 2 can be solely attributed to the monitoring of action effects. Note that the manipulation of S1–R1 compatibility was not a confound, but was implemented purposefully to be able to exclude alternative explanation for any performance difference in Task 2 other than monitoring the effect of R1: We expected that the joint contribution of S1–R1 and R1–E1 compatibility would enlarge compatibility influences in forced choice trials relative to free choice trials in Task 1. The interesting question is whether or not the additional S1–R1 compatibility in forced choice trials boosts propagation to Task 2. If it does, this would indicate that aftereffects of previous Task 1 performance (also) propagate to Task 2 to some extent. If it does not (or the R1–E1 compatibility effects in Task 2 is even smaller with forced as compared to free choice), we have reason to assume that the propagated impact in Task 2 reflects a rather pure measure of effect monitoring processes.

Method

Participants

Twenty-four participants were recruited (8 male; no left-handed; mean age = 27.4 years) and received monetary compensation. All participants reported normal vision and were naïve concerning the hypotheses of the experiment. All participants provided written informed consent prior to the experiment.

Apparatus and Stimuli

All stimuli were presented on a 22-in. screen against a black background (see Figure 1, lower panel for illustrations of the following). S1 were pictures of puzzle pieces with a connector at the left, the right, or at both sides, presented centrally on the screen. If the puzzle piece included only one connector, a puzzle piece had to be produced that fitted the connector (forced choice). If two connectors were presented, participants could choose freely whether they wanted to add a piece on the left or on the right (free choice). In free choice trials, participants were encouraged to choose their option spontaneously while maintaining an approximately equal ratio between left and right keypresses. Participants responded with the middle fingers of their left and right hand on the *S* and *K* keys of a standard German QWERTZ keyboard. These keypresses produced puzzle pieces at either the left or the right side of the screen as Effect 1 (E1). The horizontal location of the produced puzzle pieces was in alignment with the positions of the *S* and *K* keys on the keyboard. The R1–E1 mapping (spatially compatible vs. incompatible) was manipulated within participants between blocks, and block order (first half compatible vs. first half incompatible) was counterbalanced between participants.

For Task 2, participants had to categorize a color patch (S2) as either blue or orange. The color patch was presented centrally below S1 and required a response with the left or right index finger on the *X* and *M* keys. The color-response mapping was counterbalanced between participants.

Procedure

The trial procedure is illustrated in Figure 1. A fixation cross marked the beginning of a trial. After 500 ms, S1 was presented centrally on the screen and required the production of E1 via pressing a left or right key. The instructions read as follows:

> In a compatible block, pressing a left key will produce a puzzle piece on the left side, and pressing the right key will add a puzzle piece to

> the right side. In an incompatible block, pressing a left key will produce a puzzle piece on the right side, and pressing the right key will add a puzzle piece to the left side.

The produced puzzle pieces stayed on screen until the trial ended. If after a maximum of 2,000 ms, no key was pressed, the trial counted as an omission and no E1 was displayed. If, in case of forced choice trials, the wrong key was pressed and a puzzle piece appeared that did not fit the connector of S1, the outlines of both puzzle pieces turned red after 50ms as an error feedback. If Task 1 was completed with a correct response, the outlines of both puzzle pieces turned green after 50 ms.

Simultaneously with the color feedback in Task 1 (i.e., with an RSOA of 50 ms after R1), S2 was displayed and called for R2. The two tasks were always presented in that order, and there was no temporal overlap between the tasks (except for the possible effect monitoring process). In case of any errors, written feedback was presented at the end of the trial (e.g., "Puzzle task: Error!", "Color task: Error!", or both) for 500 ms in red color. If both tasks were completed correctly, the next trial, indicated by a fixation cross, started immediately.

Participants completed 20 blocks, 10 blocks with a compatible R1–E1 mapping in Task 1, and 10 blocks with an incompatible mapping, of 60 trials each, with each combination of S1 (connector: left, right, both) and S2 (color: blue, orange) presented 10 times.

Results

In free choice trials, left and right responses were chosen with an approximately equal ratio (left key: 52.5%, right key 47.5%, $t(23) = 1.27$, $p = .217$, $d = 0.26$, for all t tests: $d = \frac{t}{\sqrt{n}}$). For RT analyses, we excluded trials with errors and omissions (Task 1: 3.9%, Task 2: 8.5%). The remaining trials were screened for outliers and we removed trials in which RTs for any task deviated more than 2.5 standard deviations from the corresponding cell mean, computed separately for each participant and experimental condition (4.7%). Overall, 15.6% of the trials were removed. The remaining data was then analyzed separately depending on whether Task 1 was forced or free choice. Mean RTs for the compatible versus incompatible R1–E1 mapping in Task 1 were compared to a paired t test (two-sided), as were RTs after the compatible versus incompatible R1–E1 mapping in Task 2 (see Figure 2). Error rates were analyzed accordingly (for Task 1 only for forced choice trials).

Forced Choice Trials

In Task 1, we observed a significant effect of R1–E1 compatibility, $t(23) = 8.85$, $p < .001$, $d = 1.81$, with faster responses for the compatible mapping (465 ms) compared to the incompatible mapping (575 ms). The manipulation in Task 1 propagated to Task 2, $t(23) = 6.82$, $p < .001$, $d = 1.39$, with faster responses after trials with the compatible mapping (451 ms) compared to trials after the incompatible mapping (490 ms).

Errors were more prominent in Task 1 with the incompatible mapping (8.5%) compared to the compatible mapping (3.1%), $t(23) = 3.26$, $p = .003$, $d = 0.67$, and only marginally more prominent in Task 2 with the incompatible (11.0%) compared to the compatible mapping (8.2%), $t(23) = 1.88$, $p = .073$, $d = 0.39$.

Free Choice Trials

In Task 1, we observed a significant effect of compatibility, $t(23) = 6.62$, $p < .001$, $d = 1.35$, with faster responses for the compatible mapping (451 ms) compared to the incompatible mapping (515 ms). Importantly, the manipulation in Task 1 propagated to Task 2, $t(23) = 6.73$, $p < .001$, $d = 1.37$, with faster responses after trials with the compatible mapping (445 ms) compared to trials after the incompatible mapping (486 ms).

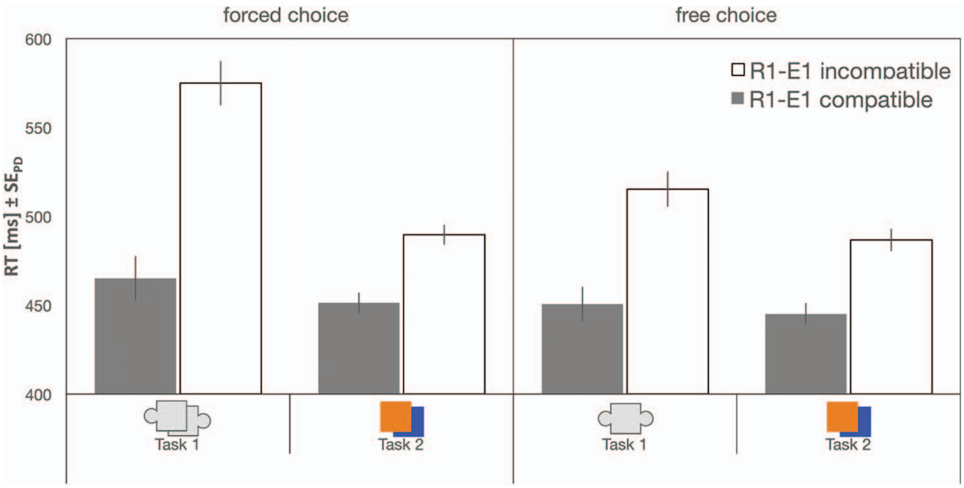

Figure 2. Results of Experiment 1. Response times (RTs) for Tasks 1 and 2, separately for forced and free choice trials. Gray bars represent trials with the spatially compatible R1-E1 mapping in Task 1, white bars represent trials with the incompatible mapping. Error bars denote the standard error of paired differences, computed separately for each comparison of R1-E1 compatibility (Pfister & Janczyk, 2013). See the online article for the color version of this figure.

By design, errors were not possible in the free choice trials in Task 1, and the error rates of Task 2 (6.5%) were not affected by the compatibility manipulation, $t(23) = 1.00$, $p = .327$, $d = 0.20$.

Overall RT Analysis

We compared forced choice and free choice trials to isolate the impact of the R1–E1 manipulation (varied in both types of trials) from the effect of the S1-R1 manipulation (varied only in forced choice trials). Therefore, the RT data was analyzed with a 2 × 2 ANOVA with choice (forced vs. free) and Task 1 R1–E1 compatibility (compatible vs. incompatible) as within-subjects factors. To reduce redundancy (in this and any subsequent overall RT analysis), we focused on effects including the factor choice.

In Task 1, we observed an effect of choice, $F(1, 23) = 17.28$, $p < .001$, $\eta_p^2 = .43$, with overall faster responses for free choice (483ms) compared to forced choice trials (520 ms), and an interaction between choice and Task 1 compatibility, $F(1, 23) = 12.73$, $p < .001$, $\eta_p^2 = .36$, with a stronger compatibility effect in forced choice ($\Delta = 110$ ms) compared to free choice trials ($\Delta = 65$ ms). However, there was neither a main effect of the factor choice nor an interaction with the factor compatibility on RT2 (both $F\text{s} < 1$). This suggests that mere task difficulty or similar alternative explanations cannot account for the observed RT2 difference in free choice trials.

Discussion

In Experiment 1, we employed a dual-task experiment with two temporally and conceptually separate tasks to test for effect monitoring. In Task 1, the spatial R1–E1 compatibility was manipulated. The size of the compatibility effect was somewhat larger here compared to other instances with spatial responses and visual effects (e.g., Kunde, 2001, but see Pfister & Kunde, 2013, 36ms). The size of these effects in RT depends on whether participants attend to these effects and how they interpret them (Janczyk, Yamaguchi, Proctor, & Pfister, 2015; Memelink & Hommel, 2013). The very obtrusive action effects used here were apparently coded as left and right events that did or did not spatially match the actions that produced them.

Further, a propagation of this manipulation to the unrelated Task 2 would speak for a capacity-limited effect monitoring process that runs after response completion. And indeed, the performance in Task 2 suffered after responses that produced an incompatible action effect in contrast to those producing a compatible action effect. This prolonged influence of the R1–E1 manipulation can only be explained by assuming a process after the response in Task 1 has been completed. So not only the anticipation and production of incompatible action effects takes longer compared to compatible action effects (Kunde et al., 2012; Paelecke & Kunde, 2007; Wirth et al., 2015), but also the monitoring of incompatible action effects takes longer than the monitoring of compatible effects. But although the anticipation and production of incompatible action effects is additionally influenced by stimulus features (resulting in larger compatibility effects in forced choice than in free choice trials in Task 1), the difficulty in monitoring self-produced action effects seems to be mainly driven by a mismatch between one's own response and the triggered action effect, as there is no difference in Task 2 between free and forced choice trials. Model A (see Figure 1) consequently can be rejected, as clearly there must be some interference between monitoring after Task 1 and early stages of Task 2 that causes the propagation. The locus of this interference will be addressed in Experiments 2 and 3.

Also, note that in our design, there is another important difference between forced choice and free choice trials. Next to the conceptual difference of the additional S1–R1 compatibility variation, there is also a difference concerning the use of the action effects: While in forced choice trials, it is beneficial to monitor the action effects (as they serve as an error feedback), the same cannot be said in free choice trials. Here, either response was correct, hence monitoring the action effects seems to draw on scarce resources without providing any benefit. In our case, in the long run, monitoring the action effects seems to have been beneficial (in two thirds of all trials, they provided feedback), so participants seem to allocate attention toward them. But the question stands whether monitoring self-produced action effects is a voluntary process that is limited to anticipated and intended action effects, or whether it is an automatic process that cannot be switched off deliberately. This question will be addressed in Experiment 5.

Experiment 2

On the basis of the results of Experiment 1, Model A (see Figure 1), that assumes effect monitoring to be capacity-independent, can be rejected: A hypothetical monitoring process is the only one that overlaps with Task 2, and it must draw on some kind of limited resource, otherwise no delay in Task 2 is expected. What is unclear at this point is the exact nature of process with which such monitoring conflicts. Two equally plausible options come to mind: First, monitoring could present a central bottleneck (Welford, 1952; cf. Figure 3, Model B), meaning it cannot run in parallel with response selection of Task 2. On the basis of the idea that effect anticipation is the mechanistic process underlying response selection, in this scenario, the interference in Task 2 would best be characterized as the difficulty entertain two effect-based processes simultaneously (even though action effects in Task 2 are only proprioceptive; Pfister, Janczyk, Gressmann, Fournier, & Kunde, 2014; Wirth, Pfister, Brandes, & Kunde, 2016). On the other hand, effect monitoring could lead to a general task postponement (Oriet & Jolicoeur, 2003; cf. Figure 3, Model C), meaning it cannot even run in parallel with the perceptual stage of Task 2. Here, the interference in Task 2 would best be described as an inability to concurrently allocate perceptual attention toward two stimuli (effect of Task 1 and stimulus of Task 2).

There is a way to empirically differentiate between the two models. By introducing a perceptual manipulation in Task 2 that lengthens the perceptual stage, diverging predictions can be made: The assumption of a central bottleneck predicts that the additional manipulation can (partly) be compensated for by the cognitive slack between perceptual and central stage in Task 2. In this case, the compatibility manipulation in Task 1 and the perceptual manipulation in Task 2 should combine in an underadditive interaction. However, the assumption of a general task postponement does not allow for a compensation of the additional manipulation. In this model, both manipulations should combine additively. In Experiment 2, we introduced two perceptual manipulations that should influence the length of

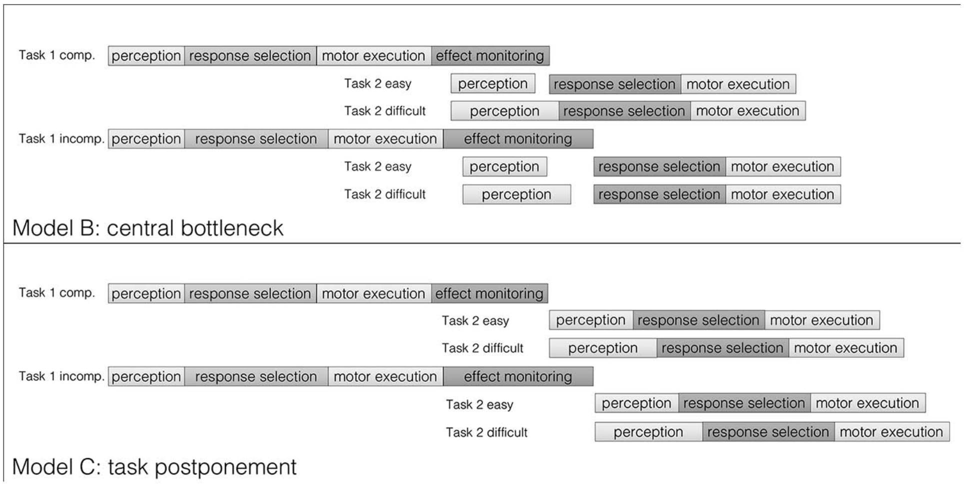

Figure 3. Two alternative models for explaining the results of Experiment 1. Model B assumes that the effect monitoring process presents a central bottleneck (therefore it cannot run in parallel with response selection of Task 2), whereas Model C assumes effect monitoring to cause a general task postponement (hence it interferes with the perceptual stage of Task 2). Both models predict a propagation of the manipulation in Task 1 into Task 2 (as observed in Experiment 1), but they allow for different predictions with the inclusion of a perceptual manipulation in Task 2: Whereas Model B assumes that the additional manipulation can be compensated for by the cognitive slack, resulting in an underadditive interaction between the two manipulations, Model C predicts an additive combination of both manipulations.

only the perceptual stage in Task 2 to test whether Model B or Model C predictions are met. We used stimulus masking and a manipulation of stimulus intensity (e.g., Pashler, 1984; Pashler & Johnston, 1989), both of which have been shown to affect the perceptual stage, to provide a critical test for both models.

Method

Forty-eight new participants were recruited (17 male; 8 left-handed; mean age = 27.7 years) and received monetary compensation. They fulfilled the same criteria as in Experiment 1.

Apparatus, S1, and procedure were exactly as in Experiment 1. Task 2 now required two letters (H, S) in white font to be categorized as S2. Crucially, these letters were further manipulated regarding perceptual quality: The letters were either masked or unmasked (with or without an X in the color of the background displayed on top of it, see Figure 4) and they were presented with normal or reduced luminance (displayed in white or dark gray). S2 masking was manipulated within participants, S2 luminance was manipulated between participants. So all participants were presented with both masked and unmasked stimuli, and half of the

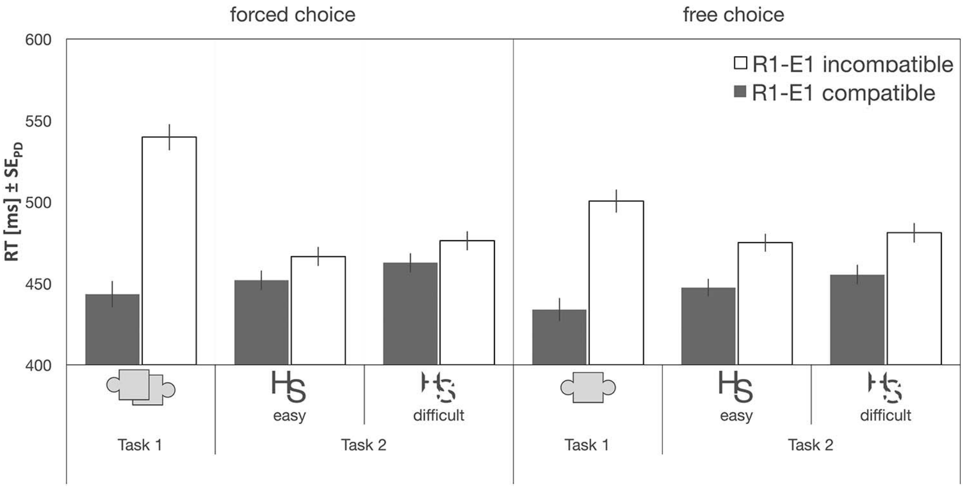

Figure 4. Results of Experiment 2. Response times (RTs) for Tasks 1 and 2, separately for forced and free choice trials. Data for Task 2 is additionally separated by S2 masking. Gray bars represent trials with the spatially compatible R1-E1 mapping in Task 1, white bars represent trials with the incompatible mapping. Error bars denote the standard error of paired differences, computed separately for each comparison of R1-E1 compatibility (Pfister & Janczyk, 2013).

participants received bright targets throughout the experiment, while the other half received dark targets throughout. Participants now completed 10 blocks, five blocks with a compatible R1–E1 mapping in Task 1, and five blocks with an incompatible mapping, of 120 trials each, with each combination of S1 (connector left, right, both), S2 identity (H, S), and S2 masking (easy: not masked, difficult: masked) presented 10 times.

Results

In free choice trials, left and right responses were chosen with a slight preference for the left key (left key: 55.1%, right key 44.9%, $t(47) = 3.33$, $p = .002$, $d = 0.48$). For RT analyses, we again removed trials with errors and omissions (Task 1: 4.2%, Task 2: 8.4%) and outliers (5.9%). Overall, 15.9% of the trials were removed. Data for Task 1 was analyzed as described for Experiment 1. Task 2 was now analyzed with a 2 × 2 × 2 ANOVA with Task 1 R1-E1 compatibility (compatible vs. incompatible) and S2 masking (unmasked, easy vs. masked, difficult) as within-subjects factors and S2 luminance (bright targets vs. dark targets) as a between-subjects factor (see Figure 4). Error rates were analyzed accordingly.

Forced Choice Trials

In Task 1, we again observed a significant effect of compatibility, $t(47) = 12.03$, $p < .001$, $d = 1.74$, with faster responses for the compatible mapping (443 ms) compared to the incompatible mapping (539 ms). As in Experiment 1, the compatibility manipulation in Task 1 propagated to Task 2, $F(1, 46) = 6.25$, $p = .016$, $\eta_p^2 = .12$, with faster responses after trials with the compatible mapping (457ms) compared to trials after the incompatible mapping (471 ms). Further, there was an effect of S2 masking, $F(1, 46) = 60.10$, $p < .001$, $\eta_p^2 = .57$, with slower responses for masked S2s (469 ms) compared to S2s that were not masked (460 ms), and an interaction between S2 masking and S2 luminance, $F(1, 46) = 9.27$, $p = .004$, $\eta_p^2 = .17$, with a stronger effect of S2 masking with bright S2s ($\Delta = 13$ ms) compared to dark S2s ($\Delta = 5$ ms) in Task 2. S2 luminance did not produce a main effect, $F(1, 46) = 1.34$, $p = .253$, $\eta_p^2 = .03$. Notably, both combinations of Task 1 compatibility and the S2 manipulations combined in an additive manner (Fs ≤ 1.80, ps ≥ .186). No other effects were significant (Fs ≤ 1.98, ps ≥ .166).

To provide further evidence for the absence of both interactions between R1-E1 compatibility and the S2 manipulations (masking and luminance), we computed separate Bayesian two-way repeated-measures ANOVAs with the factors compatibility and S2 manipulation using JASP and its defaults values for the Cauchy distribution of possible effect sizes (Love et al., 2015). These analyses revealed evidence for the absence of these interactions, BF_0s > 4.2 (Jeffreys, 1961).

Errors were slightly more prominent in Task 1 with the incompatible mapping (7.7%) compared to the compatible mapping (6.3%), $t(47) = 2.01$, $p = .050$, $d = 0.29$. In Task 2, error rates produced a significant effect of Task 1 compatibility, $F(1, 46) = 51.57$, $p < .001$, $\eta_p^2 = .53$, with more errors for the incompatible mapping (9.1%) compared to the compatible mapping (6.4%). No other effects were significant (Fs ≤ 3.24, ps ≥ .079).

Free Choice Trials

In Task 1, we observed a significant effect of compatibility, $t(47) = 9.42$, $p < .001$, $d = 1.36$, with faster responses for the compatible mapping (433 ms) compared to the incompatible mapping (500 ms). The compatibility manipulation in Task 1 again propagated to Task 2, $F(1, 46) = 23.24$, $p < .001$, $\eta_p^2 = .37$, with faster responses after compatible trials (451 ms) compared to incompatible trials (477 ms). Also, there was an effect of masking, $F(1, 46) = 15.73$, $p < .001$, $\eta_p^2 = .26$, with slower responses for masked S2s (468 ms) compared to S2s that were not masked (461 ms), and an interaction between S2 masking and S2 luminance, $F(1, 46) = 4.62$, $p = .037$, $\eta_p^2 = .09$, with a stronger effect of S2 masking with bright targets ($\Delta = 11$ ms) compared to dark targets ($\Delta = 3$ ms) in Task 2. S2 luminance did not produce a main effect, $F(1, 46) = 1.15$, $p = .288$, $\eta_p^2 = .02$. Notably, both combinations of Task 1 compatibility and the S2 manipulations combined in an additive manner, Fs < 1. No other effects were significant (Fs ≤ 1.15, ps ≥ .288). Again, separate post hoc Bayesian analyses revealed evidence for the absence of these interactions, BF_0s > 4.6.

The compatibility manipulation of Task 1 affected the Task 2 error rates, $F(1, 46) = 4.27$, $p = .044$, $\eta_p^2 = .09$, with more errors after incompatible trials (7.7%) compared to after compatible trials (6.3%). No other effects reached significance (Fs ≤ 1.45, ps ≥ .235).

Overall RT Analysis

The RT data was analyzed in a 2 × 2 × 2 × 2 ANOVA with choice (forced vs. free), Task 1 compatibility (compatible vs. incompatible) and S2 masking (unmasked vs. masked) as within-subjects factors and S2 luminance (bright vs. dark) as a between-subjects factor.

In Task 1, we observed an effect of choice, $F(1, 46) = 29.43$, $p < .001$, $\eta_p^2 = .39$, with overall faster responses for free choice (467ms) compared to forced choice trials (492 ms), and an interaction between choice and Task 1 compatibility, $F(1, 46) = 17.61$, $p < .001$, $\eta_p^2 = .28$, with a stronger compatibility effect in forced choice trials ($\Delta = 96$ ms) compared to free choice trials ($\Delta = 67$ ms). In Task 2, there was an interaction between choice and Task 1 compatibility, $F(1, 46) = 31.08$, $p < .001$, $\eta_p^2 = .40$, with a stronger influence of compatibility in free choice trials ($\Delta = 26$ ms) compared to forced choice trials ($\Delta = 14$ ms).

3.3 Discussion

In Experiment 2, we replicated Experiment 1 with the addition of a perceptual manipulation in Task 2. We replaced the color task with a letter task that required the same motor responses, because this stimulus allowed for an easier perceptual manipulation, namely stimulus masking and reduction of the stimulus' luminance. This was done to differentiate between the two possible loci of interference between effect monitoring after Task 1 and early processing stages of Task 2 (central bottleneck vs. task postponement; see Figure 3).

First, we replicated the results of Experiment 1, and the R1–E1 compatibility effect of Task 1 propagated to Task 2 without temporal overlap between stages other than a presumed effect monitoring process with Task 2 processing. Monitoring spatially incompatible action effects again seems to take longer than monitoring compatible action effects. Second, the compatibility manipulation in Task 1 combined with the perceptual masking manipulation in Task 2 in an additive manner, which indicates a general task postponement (cf. Figure 3): The perceptual manipulation lengthens the perceptual stage for masked targets, but this longer perceptual stage cannot be compensated for by potential cognitive slack in Task 2. Instead, it delays the processing of Task 2 as a whole, as predicted by the task postponement model. It seems as if the propagation of the Task 1 manipulation onto Task 2 indicates an inability to allocate visual attention toward self-produced action effects and a new stimulus at the same time. Further, pinpointing the locus of interference to the perceptual stage lets us roughly estimate the duration of the monitoring process. Monitoring self-produced action effects lasts at least until 50ms after the response (at least for incompatible effects). Otherwise, there would be no overlap between monitoring in Task 1 and the perceptual stage of Task 2, and any monitoring process would be swallowed by the RSOA of 50 ms that we used here. If monitoring for compatible and incompatible effects takes longer than 50ms, then the size of the compatibility manipulation that we find in Task 2 (about 20ms) gives us an estimate of how much longer it takes to monitor incompatible effects compared to compatible effects. However, it could also be the case that monitoring for compatible effects ends earlier than 50ms after the response, and in this case, the RT difference would underestimate the monitoring duration increase (for a more thorough discussion on the temporal dynamics of effect monitoring, see Experiment 4). Notably, the luminance manipulation was not successful in Experiment 2, even though it was in many previous studies (e.g., Pashler, 1984). We attribute this to the fact that we implemented this manipulation between-subjects. Experiment 3 was run to replicate Experiment 2, but we implemented (only) the luminance manipulation within subjects.

4. Experiment 3

In Experiment 2, we tested whether an additional perceptual manipulation in Task 2 could be compensated for by a possible cognitive slack (central bottleneck), or whether it would delay the processing of Task 2 as a whole (general task postponement). Data suggests that it is indeed the perceptual stage of Task 2 that does not run in parallel with effect monitoring after Task 1, indicated by an additive interaction of both manipulations. But maybe the letter task that we used (categorizing H and S by a left and right keypress with perceptual manipulations, masking and brightness-reduction, layered on top of them) is not ideal to reveal a possible underadditive interaction (that the central bottleneck model would predict). It might be that participants acquire a 4-to-2 mapping (e.g., H and masked-H →left, S and masked-S →right) without identifying a masked letter as either H or S, thereby circumventing the actual task (although the significant main effect of masking speaks against such a possibility). The manipulation could further be avoided by not discriminating between H and S, but on focusing on straight versus curvy features of the stimulus. All in all, the letter task that we used might allow for strategic shortcuts, which might not have manipulated, as intended, the duration of the perceptual stage, but rather lengthened the central stage by making the response selection more difficult (either by demanding a 4-to-2 mapping instead of a 2-to-2 mapping, or by having participants respond only to specific features of the stimulus). If this were the case, both models predict an additive interaction between the two manipulations. To rule out this possibility, in Experiment 3 we replicated Experiment 2, but changed Task 2, which now no longer allows for a shortcut. By presenting not two, but 24 different stimuli in Task 2 that had to be identified individually to be categorized into one of two categories, no efficient shortcut rule could be learned. Again, we layered a perceptual manipulation on top of these stimuli to test whether this added manipulation can be compensated for (Model B) or not (Model C).

Method

Twenty-four new participants were recruited (5 male; 2 left-handed; mean age = 24.4 years) and received course credit. They fulfilled the same criteria as in Experiments 1 and 2.

Task 1 was the same as in the previous experiments. As S2, 24 letters from the alphabet (A–L, O–Z; for an equal number of stimuli, the letter N was also never presented, but instructions only referred to the letter M) were used and had to be categorized depending on their position in the alphabet (before or after the letter M). As in Experiment 3, a manipulation of S2 luminance (bright vs. dark) was employed to manipulate the perceptual quality of the stimuli, but it was now manipulated within-subjects, randomized between trials. Participants completed 10 blocks, five blocks with a compatible R1–E1 mapping in Task 1, and five blocks with an incompatible mapping, of 144 trials each, with each combination of S1 (connector left, right, both), S2 identity (24 letters), and S2 luminance (bright vs. dark) presented once.

Results

In free choice trials, left and right responses were chosen with an approximately equal ratio (left key: 52.1%, right key 47.9%, $t(23) = 0.95$, $p = .350$, $d = 0.19$). For RT analyses, we again removed trials with errors and omissions (Task 1: 3.9%, Task 2: 10.3%) and outliers (5.6%). Overall, 17.8% of the trials were removed. Data for Task 1 was analyzed as previously. Task 2 was analyzed with a 2 × 2 ANOVA with Task 1 R1–E1 compatibility (compatible vs. incompatible) and S2 luminance (bright S2s vs. dark S2s) as within-subjects factors (see Figure 5). Error rates were analyzed accordingly.

Forced Choice Trials

As in the previous experiments, in Task 1 we observed a significant effect of compatibility, $t(23) = 7.48$, $p < .001$, $d = 1.53$, with faster responses for the compatible mapping (442 ms) compared to the incompatible mapping (537 ms). The compatibility manipulation in Task 1 produced only a descriptive difference in Task 2, which was not significant, $F(1, 23) = 2.75$, $p = .110$, $\eta_p^2 = .11$. Further, there was an effect of S2 luminance, $F(1, 23) = 20.36$, $p < .001$, $\eta_p^2 = .47$, with slower responses for dark S2s (537 ms) compared to bright S2s (529 ms). Notably, Task 1 compatibility and the S2 manipulation combined again in an additive manner, $F < 1$. A post hoc Bayesian ANOVA revealed evidence for the absence of this interaction, $BF_0 = 3.5$.

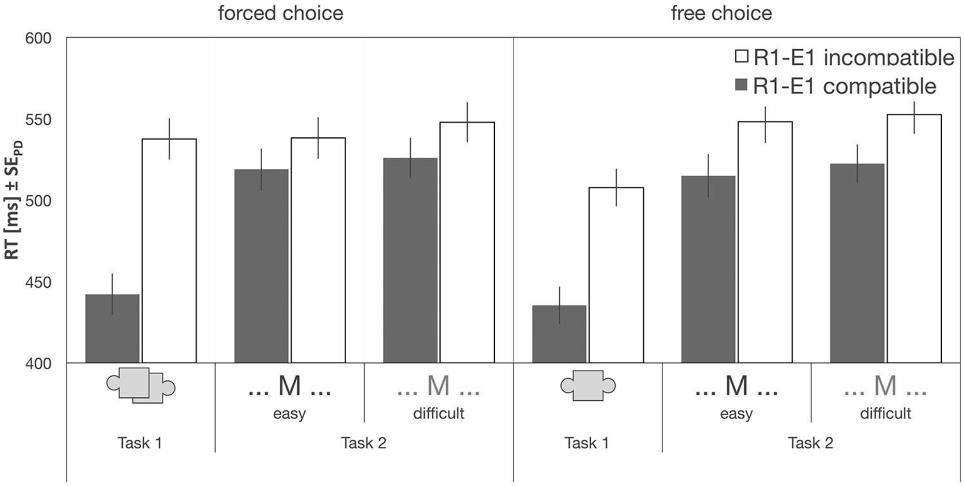

Figure 5. Results of Experiment 3. Response times (RTs) for Tasks 1 and 2, separately for forced and free choice trials. Data for Task 2 is additionally separated by S2 luminance. Gray bars represent trials with the spatially compatible R1-E1 mapping in Task 1, white bars represent trials with the incompatible mapping. Note that "M" (and "N") never occurred as a target, but served as a reference for the target letters. Error bars denote the standard error of paired differences, computed separately for each comparison of R1-E1 compatibility (Pfister & Janczyk, 2013).

Errors were slightly more prominent in Task 1 with the incompatible (7.9%) compared to the compatible mapping (4.0%), $t(23) = 3.10$, $p = .005$, $d = 0.63$. In Task 2, error rates produced a marginally significant effect of S2 luminance, $F(1, 23) = 3.65$, $p = .069$, $\eta_p^2 = .14$, with more errors for dark (11.3%) compared to bright S2s (10.6%). No other effects were significant (Fs $\leq$ 1.63, ps $\geq$ .215).

Free Choice Trials

In Task 1, we observed a significant effect of compatibility, $t(23) = 6.27$, $p < .001$, $d = 1.28$, with faster responses for the compatible (435 ms) compared to the incompatible mapping (508 ms). As in the previous experiments, the compatibility manipulation in Task 1 propagated to Task 2, $F(1, 23) = 6.62$, $p = .017$, $\eta_p^2 = .22$, with faster responses after trials with the compatible (518 ms) compared to trials after the incompatible mapping (550 ms). Further, there was an effect of S2 luminance, $F(1, 23) = 6.05$, $p = .022$, $\eta_p^2 = .21$, with slower responses for dark S2s (537 ms) compared to bright S2s (531 ms). Notably, Task 1 compatibility and the S2 manipulation combined in an additive manner ($F < 1$). Again, post hoc Bayesian analyses revealed evidence for the absence of this interaction ($BF_0 = 3.6$).

In the error rates of Task 2, the compatibility manipulation of Task 1 interacted with the S2 manipulation, $F(1, 23) = 6.01$, $p = .022$, $\eta_p^2 = .21$, with no significant differences after compatible trials ($\Delta = -0.3\%$, $|t|<1$) but higher error rates for the dark compared to the bright S2s after incompatible trials ($\Delta = 1.5\%$), $t(23) = 2.35$, $p = .028$, $d = 0.48$. No other effects were significant (Fs $\leq$ 1.53, ps $\geq$ .229).

Overall RT Analysis

The RT data was analyzed in a 2 × 2 × 2 ANOVA with choice (forced vs. free) and Task 1 compatibility (compatible vs. incompatible) and S2 luminance (bright vs. dark) as within-subjects factors.

In Task 1, we observed an effect of choice, $F(1, 23) = 4.52$, $p = .044$, $\eta_p^2 = .16$, with overall faster responses for free choice (471 ms) compared to forced choice trials (490ms), and a marginally significant interaction between choice and Task 1 compatibility, $F(1, 23) = 3.86$, $p = .062$, $\eta_p^2 = .14$, with a stronger compatibility effect in forced choice trials ($\Delta = 95$ ms) compared to free choice trials ($\Delta = 72$ ms). In Task 2, there was an interaction between choice and Task 1 compatibility, $F(1, 23) = 7.95$, $p = .010$, $\eta_p^2 = .26$, with a stronger influence of compatibility in free choice trials ($\Delta = 32$ ms) compared to forced choice trials ($\Delta = 21$ ms).

Discussion

Experiment 3 replicated Experiment 2, but employed a letter task in Task 2 that did not allow for easy shortcuts: By using not two, but 24 different stimuli in Task 2 that had to be identified individually, no simple mapping rule could be learned to circumvent the perceptual manipulation that we used.

The forced choice RT2s did not show a significant influence of the R1–E1 compatibility, and the reasons for this are not clear at present. The failure of observing a significant result might simply reflect a Type 2 error, because the effect was significant in the forced choice trials of Experiment 1 and 2 (and is also in Experiment 4, see below). We assumed that in forced choice trials, not only the R1–E1 relation is monitored, but also the additional S1-E1 relation, which could arguably reinforce rather than reduce the effect as compared to free choice trials. However, in forced choice trials participants might also devote some capacity to monitor the S1–R1 relationship rather than the R1–E1 relationship, whereby the influence of R1–E1 compatibility in Task 2 reduces, which was not possible in free choice trials, which contain no response- or effect-specific S1. This is a post hoc explanation and certainly more research is necessary to reveal potential differences of effect monitoring in free and forced choice conditions. However, keep in mind that we base our inferences on free choice trials, as they provide the purest measurement of R1–E1 monitoring, because neither S1–R1 nor S1–E1 relations are involved.

In the free choice trials, we replicated that the compatibility manipulation in Task 1 propagates to Task 2, with a longer delay for Task 2 after incompatible compared to compatible effects. Incompatible action effects again seem to take longer to monitor, resulting in a longer monitoring process that propagates into Task 2. And again, this result suggests that this monitoring process presents some kind of interference, because it does not run in parallel with (one of) the early stages of Task 2 processing.[2] Together with the results obtained in Experiment 2, the results of Experiment 3 provide evidence for the task postponement model as sketched as Model C in Figure 3: It seems as if effect monitoring delays the Task 2 processing as a whole, because it occupies visual attention to a degree that no other task can be initiated simultaneously. A difficulty in identifying visual stimuli shortly after a task has been completed has already been reported by other authors (Potter, Chun, Banks, & Muckenhoupt, 1998; Visser, Bischof, & Di Lollo, 1999).

Experiment 4

The previous experiments suggest that monitoring action effects in one task can influence the processing of another, subsequent task. An interesting question then is how long such an influence might last. To explore this question, we varied the time interval between occurrence of the action effect in Task 1 and the stimulus in Task 2, hence the effect stimulus onset asynchrony (ESOA), on a trial-to-trial basis. Conceivably, action effect monitoring in Task 1 will affect Task 2 less likely the more these tasks are temporally separated.

However, on the basis of previous research, it is hard to judge how long effect monitoring lasts and affects subsequent performance. For example, Jentzsch and Dudschig (2009) argued that capacity-limited response monitoring is engaged longer after errors than after correct responses, possibly for several hundred milliseconds (cf. Welford, 1980). Accordingly, they observed increased RTs even 1,000 ms after committing an error as compared to responding correctly. Given these observations we manipulated the ESOA between 50 ms and 950 ms. We assumed that RT2 would decrease with increasing ESOA, and that the influence of the previous R1–E1 compatibility in Task 1 on the subsequent Task 2 would basically reduce, though probably not vanish, with the longest ESOAs.

Finding such temporal dynamics would also help to address another potential concern. R1-E1 compatibility was manipulated blockwise so far. Participants might therefore strategically prepare more for Task 1 in blocks with incompatible effects in Task 1, which might also contribute to the increase of RT2 (for a similar discussion, see Wirth et al., 2015). Such preparation should operate in a blockwise manner and affect RT2 independent of a randomly varying ESOA. Therefore, finding that R1–E1 compatibility affects RT2 depending on temporal overlap would be first evidence against such strategic effects (see also Experiment 5).

Method

Twenty-four new participants were recruited (3 male; 2 left-handed; mean age = 23.2 years) and received course credit. They fulfilled the same criteria as in the preceding experiments.

To address the temporal dynamics of effect monitoring, we varied the time interval between E1 and S2 (ESOA). In addition to the interval of 50 ms (used in all prior experiments) we now stretched the interval out up to 950 ms (in 100 ms steps). ESOAs now varied randomly from trial to trial within a block. Therefore, we also changed Task 2 back to the two-choice letter discrimination task (H vs. S, see Experiment 2) to ensure that every combination of Task 1 compatibility, ESOA and S2 was displayed equally often within a block. As in Experiment 2, forced and free choice trials were used, but S2 quality was not manipulated. Participants completed 20 blocks, 10 blocks with a compatible R1–E1 mapping in Task 1, and 10 blocks with an incompatible mapping, of 60 trials each, with each combination of S1 (connector left, right, both), S2 identity (H, S), and ESOA (50 ms, 150 ms, 250 ms, 350 ms, 450 ms, 550 ms, 650 ms, 750 ms, 850 ms, 950 ms) presented once.

Results

In free choice trials, left and right responses were chosen with a descriptive preference for the left key (left key: 52.3%, right key 47.7%), $t(23) = 1.85$, $p = .077$, $d = 0.38$. For RT analyses, we again removed trials with errors and omissions (Task 1: 5.8%, Task 2: 8.2%) and outliers (5.1%). Overall, 17.7% of the trials were removed. Data for Task 1 was analyzed as usual. Task 2 was now analyzed with a 2 × 10 repeated-measures ANOVA using the multivariate approach with Task 1 R1–E1 compatibility (compatible vs. incompatible) and ESOA (50 ms vs. 150 ms vs. 250 ms vs. 350 ms vs. 450 ms vs. 550 ms vs. 650 ms vs. 750 ms vs. 850 ms vs. 950 ms) as within-subjects factors (see Figure 6). The multivariate approach was chosen here to control for potential violations of sphericity, which could not occur in all remaining analyses which tested factors with two levels only. Error rates were analyzed accordingly.

Forced Choice Trials

In Task 1, as expected we observed a significant effect of compatibility, $t(23) = 7.77$, $p < .001$, $d = 1.59$, with faster responses for the compatible mapping (443 ms) compared to the incompatible mapping (559 ms). And again, the compatibility manipulation in Task 1 propagated to Task 2, $F(1, 23) = 9.03$, $p = .006$, $\eta_p^2 = .28$, with faster responses after trials with the compatible mapping (476ms) compared to trials after the incompatible mapping (504 ms). There was a main effect of ESOA, $F(9, 15) = 32.79$, $p < .001$, $\eta_p^2 = .95$, as well as a descriptive, but statistically nonsignificant interaction between both factors, $F(9, 15) = 2.15$, $p = .092$, $\eta_p^2 = .56$, with a greater slowdown after incompatible effects relative to after compatible effects for shorter ESOAs (for

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[2] When probing the interaction between compatibility and the S2 manipulations on the pooled data set of Experiments 2 and 3, we observe very strong evidence for an additive combination of these factors in both forced choice trials, $BF_0 = 5.4$, and free choice trials, $BF_0 = 5.1$.

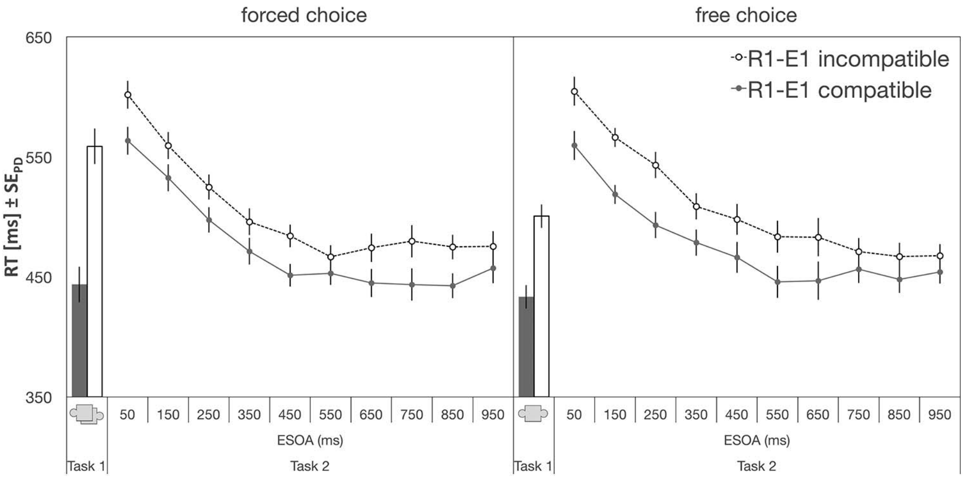

Figure 6. Results of Experiment 4. Response times (RTs) for Tasks 1 and 2, separately for forced and free choice trials. The data is additionally separated by ESOA. Gray bars/dots represent trials with the spatially compatible R1-E1 mapping in Task 1, white bars/dots represent trials with the incompatible mapping. Error bars denote the standard error of paired differences, computed separately for each comparison of R1-E1 compatibility (Pfister & Janczyk, 2013).

shortest ESOA: $\Delta = 38$ ms, $t(23) = 3.30$, $p = .003$, $d = 0.67$; for longest ESOA: $\Delta = 18$ ms, $t(23) = 1.42$, $p = .170$, $d = 0.29$).

Errors were more prominent in Task 1 with the incompatible mapping (8.4%) compared to the compatible mapping (3.2%), $t(23) = 4.91$, $p < .001$, $d = 1.00$. In Task 2, error rates did not produce any significant effects (Fs ≤ 1.74, ps $\geq .164$).

Free Choice Trials

In Task 1, we observed a significant effect of compatibility, $t(23) = 5.05$, $p < .001$, $d = 1.03$, with faster responses for the compatible mapping (433 ms) compared to the incompatible mapping (509 ms). Yet again, the compatibility manipulation in Task 1 propagated to Task 2, $F(1, 23) = 10.80$, $p < .001$, $\eta_p^2 = .48$, with faster responses after trials with the compatible mapping (477 ms) compared to trials after the incompatible mapping (509 ms). There was a main effect of ESOA, $F(9, 15) = 18.16$, $p < .001$, $\eta_p^2 = .92$, as well as an interaction between both factors, $F(9, 15) = 2.94$, $p = .031$, $\eta_p^2 = .64$, with a greater slowdown after incompatible effects relative to after compatible effects for shorter ESOAs (for shortest ESOA: $\Delta = 45$ ms, $t(23) = 3.72$, $p = .001$, $d = 0.76$; for longest ESOA: $\Delta = 13$ ms, $t(23) = 1.37$, $p = .183$, $d = 0.28$). Further, the linear trend involved in this interaction was significant, $F(1, 23) = 13.60$, $p = .001$, $\eta_p^2 = .37$, indicating a decreasing influence of compatibility with increasing levels of ESOA.

Errors in Task 2 produced neither of the main effects, Fs ≤ 1.76, ps $\geq .159$, and the interaction just missed conventional levels of significance, $F(1, 23) = 2.52$, $p = .055$, $\eta_p^2 = .60$.

Overall RT Analysis

The RT data was analyzed with a $2 \times 2 \times 10$ mixed ANOVA using the multivariate approach with choice (forced vs. free), Task 1 compatibility (compatible vs. incompatible) and ESOA (50 ms vs. 150 ms vs. 250 ms vs. 350 ms vs. 450 ms vs. 550 ms vs. 650 ms vs. 750 ms vs. 850 ms vs. 950 ms) as within-subjects factors. In Task 1, we observed an effect of choice, $F(1, 23) = 21.92$, $p < .001$, $\eta_p^2 = .48$, with overall faster responses for free choice (467 ms) compared to forced choice trials (501 ms) and an interaction between choice and Task 1 compatibility, $F(1, 23) = 16.76$, $p < .001$, $\eta_p^2 = .42$, with a stronger compatibility effect in forced choice trials ($\Delta = 115$ ms) compared to free choice trials ($\Delta = 67$ ms). No effects including the factor choice were significant in Task 2, Fs ≤ 1.62, ps $\geq .216$. Overall, the interaction between Task 1 compatibility and ESOA was significant, $F(9, 23) = 2.84$, $p = .036$, $\eta_p^2 = .63$, and again this interaction followed a linear trend, $F(1, 23) = 7.62$, $p = .011$, $\eta_p^2 = .25$, indicating a decreasing influence of compatibility with increasing levels of ESOA.

Discussion

In Experiment 4, we explored how long action effect monitoring might affect a subsequent task. To do so, we manipulated the time interval between presentation of an action effect and the stimulus for a subsequent task. We again observed that spatial R1–E1 compatibility in Task 1 modulated Task 2 performance, with slower responses after incompatible effects. Moreover, this influence of R1–E1 compatibility in Task 2 was not constant across ESOA. It became smaller and less reliable with increasing ESOA, in particular when comparing the shortest and longest ESOA levels. With an ESOA of 950 ms the monitoring process seems to have been almost always completed under the present conditions.

The decline of the R1–E1 compatibility influence was less pronounced with forced choice trials. But keep in mind that in these trials participants might not only monitor the effects they produce, but also the relation of the stimulus to the response they gave, which might take even longer. As explained before, we therefore base our inferences regarding effect monitoring on free choice trials, where there is no such stimulus. Here, the decline with ESOA is quite systematic. This, we believe, also renders explanations in terms of strategic preparation for Task 1 in incompatible blocks unlikely.

Interestingly, the slope of the ESOA function for the short ESOAs (up to the inflection point of about 550 ms) is less than -1, as one would expect on the basis of an postponement model that assumes a fixed duration of the monitoring process (Figure 3, for every 1ms less occupied by effect monitoring, RT2s should decrease 1ms). Such observation has already been discussed and modeled by Jolicoeur and Dell'Acqua (1998) who conclude that this data pattern can result if the duration of the bottleneck process (in our case, effect monitoring) has a high variance relative to its mean duration. Assuming that effect monitoring does not have a fixed duration, but varies from trial to trial, the overall probability of ongoing effect monitoring after a short ESOA is higher than after a longer ESOA, explaining the shallow slope (see Figure 6). Also, this assumption explains the descriptive residual influence of compatibility even after the inflection point (cf. Jentzsch & Dudschig, 2009). Jolicoeur and Dell'Acqua (1998) observed similar influences up to SOAs (corresponding to our ESOA manipulation) of 1,600 ms. This is possible with a sufficiently high variability of the bottleneck process causing a postponement, so that there is an above zero probability that this process occurs even at long SOAs.

Overall, the temporal dynamics of RT2 and the declining R1-E1 compatibility effect with ESOA do not speak against the task postponement model, but rather demonstrate one important feature, namely a variable duration of the proposed monitoring process.

Experiments 5a and 5b

Experiments 1 through 4 tested whether monitoring of self-produced, predictable and thus anticipatable action effects presents a bottleneck in dual-task performance. Even though monitoring these effects has no obvious benefit for the subsequent task, participants seem to pay attention to them. However, in forced choice trials, these effects convey information on the correctness of the response, while they convey no such information in free choice trials, as every response was equally acceptable here. By intermixing forced and free choice trials, we might have artificially provoked attention toward the self-produced action effects even in trials where they hold no information, because they were surrounded by a majority of trials where they were informative.

In Experiment 5, we tested whether action effects are still monitored if these effects hold no information, neither regarding the responses' correctness nor their identity. Therefore, we only used free choice trials and varied R1–E1 compatibility randomly trial by trial. If effect monitoring is a process that captures any self-produced action effect, we should still find an effect of the Task 1 manipulation on Task 2. However, if effect monitoring encompasses predicted events only, this propagation effect should vanish.

Experiments 5a and 5b are very similar, only Task 2 varied. Task 2 required the classification of letters in Experiment 5a and of words in Experiment 5b. The letters in Experiment 5a were either bright or dim to further confirm the postponement model (cf. Experiment 3). The words in Experiment 5b were either positive or negative as an additional opportunity to test if incompatible action effects facilitate the processing of subsequent negative events just as it has been reported for incompatible Stroop-like stimuli (Fritz & Dreisbach, 2013; for a related discussion, see Wirth, Steinhauser, Janczyk, Steinhauser, & Kunde, 2017).

Method

Twenty-four new participants were recruited for Experiment 5a (6 male; 4 left-handed; mean age = 25.4 years) and Experiment 5b (7 male; 2 left-handed; mean age = 23.1 years). They fulfilled the same criteria as in the preceding experiments.

Stimuli, apparatus, and procedure were as in Experiment 3, but now the factor R1–E1 compatibility was not blocked, but varied randomly from trial to trial. This way, participants could not anticipate whether the current trial employed the compatible or the incompatible mapping. Therefore, only free choice trials were presented, so that S1 did not indicate the possible location of E1. (If the stimulus showed only one connector, an effect on the opposite side would look like an error). This resulted in an experiment, where participants chose freely from trial to trial which key they wanted to press in Task 1, thereby producing a random action effect and 50ms after that the stimulus for Task 2 occurred. Again, E1 was not relevant for the completion of Task 2.

In Experiment 5a, 24 letters had to be classified as being before or after the letter M in the alphabet, and they were either bright or dark (cf. Experiment 3). In Experiment 5b, one of 24 German words (adapted from Wirth, Foerster, Rendel, Kunde, & Pfister, 2017) had to be categorized as either positive or negative. These S2 target words were chosen because they were unambiguously positive (ratings >6 on a nine-point scale, prerated by an independent sample) or negative (ratings <3 on a nine-point scale). Participants completed 10 blocks of 96 trials each, with each R1–E1 mapping of Task 1 (compatible, incompatible) and S2 (24 targets) presented twice.

Results

Experiment 5a

Left and right responses were chosen with an approximately equal ratio (left key: 52.5%, right key 47.5%, $t(23) = 0.52$, $p = .609$, $d = 0.11$). For RT analyses, we again removed trials with errors and omissions (Task 1: 0%, Task 2: 9.1%) and outliers (5.7%). Overall, 14.3% of the trials were removed. Data was analyzed akin to the free choice trials in Experiment 3. Task 2 RTs were analyzed with a 2 × 2 ANOVA with Task 1 R1–E1 compatibility (compatible vs. incompatible) and S2 luminance (dark vs. bright) as within-subjects factors (see Figure 7, left panel). Error rates were analyzed accordingly.

In Task 1, we did not observe a compatibility effect, $|t| < 1$. However, incompatible effects in Task 1 tended to delay responding in Task 2 (582 ms) as compared to compatible effects (577 ms), $F(1, 23) = 4.19$, $p = .052$, $\eta_p^2 = .15$. Further, there was an effect of S2 luminance, $F(1, 23) = 8.29$, $p = .008$, $\eta_p^2 = .27$, with slower responses for dark targets (582 ms) compared to bright targets (576 ms). Notably, Task 1 compatibility and the S2 ma-

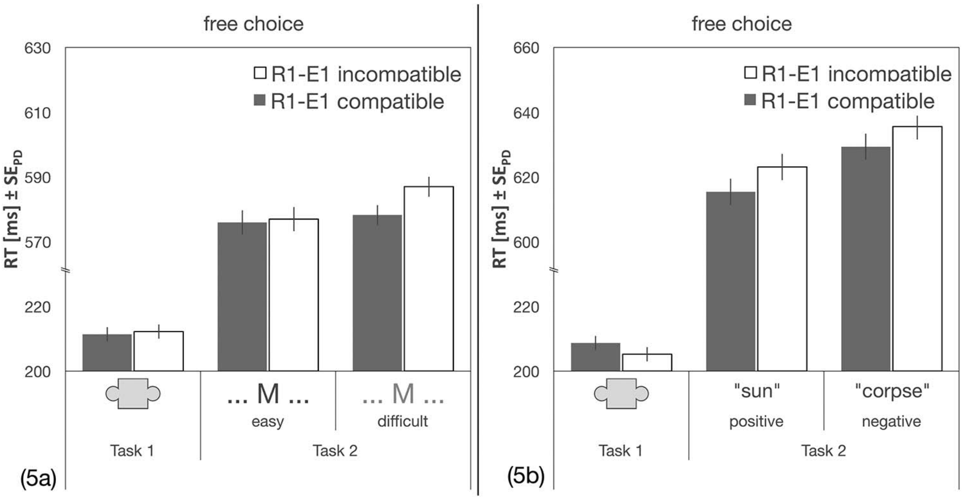

Figure 7. Results of Experiment 5a (left panel) and 5b (right panel). Response times (RTs) for Tasks 1 and 2. Data for Task 2 is additionally separated by S2 luminance (Experiment 5a) or S2 valence (Experiment 5b). Gray bars represent trials with the spatially compatible R1-E1 mapping in Task 1, white bars represent trials with the incompatible mapping. Note that "M" never occurred as a target, but served as a reference for the target letters. Error bars denote the standard error of paired differences, computed separately for each comparison of R1-E1 compatibility (Pfister & Janczyk, 2013).

nipulation combined in an additive[3] manner, $F(1, 23) = 2.56$, $p = .123$, $\eta_p^2 = .10$. The analysis of error rates did not reveal any significant results ($F\text{s} \leq 1.74$, $p\text{s} \geq .200$).

Experiment 5b

Left and right responses were chosen with an approximately equal ratio (left key: 53.3%, right key 46.7%, $t(23) = 1.04$, $p = .310$, $d = 0.21$). For RT analyses, we again removed trials with errors and omissions (Task 1: 0%, Task 2: 14.2%) and outliers (4.9%). Overall, 19.1% of the trials were removed. Task 2 RTs were analyzed with a 2 × 2 ANOVA with Task 1 R1–E1 compatibility (compatible vs. incompatible) and S2 valence (positive vs. negative) as within-subjects factors (see Figure 7, right panel). Error rates were analyzed accordingly.

There was no R1–E1 compatibility effect in Task 1, $t(23) = 1.56$, $p = .131$, $d = 0.32$, but importantly, incompatible effects in Task 1 delayed responding in Task 2 (629 ms) as compared to compatible effects (622 ms), $F(1, 23) = 9.05$, $p = .006$, $\eta_p^2 = .28$. RT2 was also marginally significantly slower with negative S2 valence (632 ms) rather than positive S2 valence (619 ms, $F(1, 23) = 3.93$, $p = .060$, $\eta_p^2 = .15$). Both effects combined additively ($F < 1$). The analysis of error rates did not reveal any significant results ($F\text{s} \leq 1.01$, $p\text{s} \geq .325$).

Discussion

Experiment 5 tested whether effect monitoring is limited only to anticipatable action effects, or whether it captures any self-produced effect as well. We varied R1–E1 compatibility randomly from trial to trial so that action effects could no longer be anticipated. The assumption of an automatic monitoring process would still predict a propagation of the Task 1 manipulation onto Task 2, although the assumption of effect monitoring as only working for predictable action effects would predict no propagation.

As the Task 1 action effects could not be anticipated, no compatibility effect emerged in Task 1, of course. Still, incompatible action effects delayed the processing of Task 2 relative to compatible action effects. Compared to the previous experiments, this compatibility effect was reduced, but still evident.[4] There are several potential reasons for why this effect is smaller with unpredictable as compared to the predictable effects in the previous experiments. Most notably, the visual effects now conveyed no relevant information neither regarding the identity nor the correctness of the executed action. This renders them less useful for monitoring own performance, and possibly discourages effect monitoring in general. Other reasons are discussed in the General Discussion. Future research could investigate whether these results are driven by the visual effect onset, or whether the occasional omission of an action effect would still produce carryover to Task 2. This would suggest that the monitoring for an anticipated, but not actually encountered, effect could also impact another task.

The influence of R1–E1 compatibility on Task 2 was independent of S2 quality (Experiment 5a) and S2 valence (Experiment 5b). The independency of S2 quality agrees with the postponement model already supported by Experiments 2 and 3. The indepen-

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[3] The nonsignificant interaction might be because of a lack of statistical power (cf. descriptive results in Figure 7, left panel). However, for the interpretation of Experiment 5a, only the main effect of compatibility is crucial here and any interaction, if taken seriously, would run counter to what we would expect if we subscribed to Model B (see Figure 3).

[4] An ANOVA on the compatibility effect in RT2 of free choice trials between Experiments 1-3 versus Experiment 5 revealed that indeed, compatibility effects are larger when effects were predictable (Experiments 1-3: 31 ms, Experiment 5: 7 ms), $t(143) = 3.89$, $p < .001$, $d = 0.32$.

dency of S2 valence supports the idea that incompatible response effects do not prime the processing of negative events, as might have been expected when such incompatible events caused negative affect (Fritz & Dreisbach, 2013). However, we do not want to draw strong inferences from this lack of interaction in Experiment 5b. Still, Experiment 5b was useful as it increased the confidence in the impact of randomly varying response-effect compatibility on Task 2.

General Discussion

The present experiments investigated the impact of monitoring action effects on dual-task performance. We used an experimental setup in which a secondary task had to be carried out while the sensory effects of a preceding task were presented, so that temporal overlap between a secondary task and monitoring of the sensory effects of the primary task was possible whereas overlap between other stages (in particular response selection of both tasks) was not. We consistently observed that the spatial relationship between Task 1 responses and their effects (R1–E1 compatibility) not only affected Task 1 performance (e.g., Kunde, 2001), but also impacted on Task 2 performance. Producing spatially incompatible action effects slows down responding in Task 2 compared to producing compatible effects. This observation has two implications. First, action effects, at least under the present conditions, are processed and not simply ignored. Second, this processing of action effects must draw on limited resources (with spatially incompatible action effects occupying the capacity-limited monitoring process longer than spatially compatible action effects), otherwise no traces of R1–E1 compatibility in Task 2 could be observed. The results of Experiment 2 and 3 suggest that processing of the secondary task is postponed as a whole, because no indication of parallel perceptual processing of Task 1 effects and Task 2 stimuli was found. Thus, effect monitoring presents itself not as a central processing bottleneck (interfering with concurrent response selection), but rather as a perceptual limitation. Experiment 4 addressed the temporal dynamics of effect monitoring, and finally, Experiment 5 revealed that even unpredictable action effects are monitored (though to a reduced extent or probability), despite not providing information about task performance.

Altogether, we suggest a scenario as shown as Model C in Figure 3 to explain the present set of results. After response execution, participants engage in a process of action effect monitoring, where an actual, observable action effect is compared against the intended, anticipated effect. As long as this monitoring takes place, processing of a secondary task is postponed. In our experiments, the duration of this blocking depends on the (spatial) relationship between action and action effect (and probably other factors that remain to be explored). The monitoring bottleneck is released earlier when a compatible rather than an incompatible effect is encountered. Before discussing further theoretical implications, we want to discuss potential alternative explanations and caveats.

Alternative Explanations

First, one may argue that what we observed was just that processing a certain visual event (the action effect of Task 1) could not run concurrently with Task 2. In other words, similar interference effects might occur with every perceptual event prior to Task 2, irrespective of whether these events were action effects or not. One argument to counter this idea would be that the to-be-processed perceptual events (i.e., puzzle pieces popping up left or right) were always the same irrespective of whether they occurred as a compatible or incompatible action effect. Thus, not the perceptual event as such determined dual-task decrements, only its spatial relationship to the response did. Also, despite being the same sensory events, whether or not the location of these events was predictable strongly determined to which extent they affected performance in Task 2. The predictable effects in Experiments 1 to 4 had a stronger impact relative to the same but unpredictable effects in Experiment 5. These observations make clear that not the perceptual event as such is crucial here, but the relationship of these events to the causal motor patterns in terms of compatibility and predictability. After all, even if one considers the monitoring process we propose here as nothing special but just a capacity-limited perceptual process, we still think it is important to make it clear that such processes are engaged to monitor postresponse events and likely contribute to dual-task performance, something that, as we believe, has not been demonstrated that clearly before. Still, it might be that the results we obtained here are driven by the modality overlap between the perceptual processes (both require visual attention) and might change without modality overlap (e.g., visual effect, auditory subsequent stimulus). This question has to be addressed in future research.

Another caveat might be as follows. As evidenced by the RTs in Task 1 it is obviously harder to foreseeably produce incompatible as compared to compatible action effects. Could not the Task 2 decrements with incompatible Task 1 effects reflect an unspecific aftereffect of having done a more difficult task just before? Again, we think this is unlikely. Remember that Task 1 differed in other respects than R1–E1 compatibility, such as either including S1–R1 compatibility (in forced choice trials) or not (free choice trials; see the introduction of Experiment 1). While this additional compatibility manipulation in forced choice tasks obviously influenced Task 1 performance, its additional impact did *not* propagate to Task 2 (or was even reduced in forced choice trials in Experiment 2 and 3). In other words, even if other factors on top of R1–E1 compatibility made Task 1 difficult, these additional burdens did not deteriorate Task 2 performance. This clearly speaks against explanations in terms of unspecific aftereffects of having experienced a more difficult Task 1 which, for example, required more attention. What deserves more research, however, is why the influence of R1–E1 compatibility on Task 2 was less pronounced across studies (in terms of number of significant effects) in forced choice as compared to free choice trials.

One elaboration of such an account might be that participants generally adopted a more conservative response criterion in R1–E1 incompatible blocks. However, the data do not support this account. A more conservative response criterion should increase RTs but reduce error rates, but in contrast, RTs became longer and error rates increased in incompatible R1-E1 blocks. Moreover, even when R1–E1 compatibility could obviously not impact Task 1 performance because the effects were unpredictable (Experiment 5), the compatibility of the actually occurring effects did still impact Task 2 performance. Finally, the influence of R1–E1 compatibility declined with increasing separation of the two tasks (Experiment 4), which it should not, if this influence was caused by the degree of preparation of Task 1. Together, these observations clearly suggest that the Task 2 decrements are not just unspecific effects of a difficult Task 1, but are rather specific to the processing (monitoring) of these events.

From a different perspective, participants had to switch from Task 1 to Task 2 within each trial in our experiments (as there was no temporal overlap between the tasks), and the increased Task 2 RTs after incompatible effects might be interpreted as increased task switching costs (cf. Kiesel et al., 2010). However, this interpretation also inherently assumes that action effects are considered, and we show here that monitoring is the process that conceivably causes these additional costs. If mere task difficulty makes task switching more difficult (e.g., by increased task-set inertia), we should again see an impact of other Task 1 manipulations on Task 2 (forced vs. free choice, S1–R1 compatibility) which, however we did not consistently observe.

Another way of looking at task switching is to construe Task 1 choice (free vs. forced) as separate task types. Viewed like that, we have trials where task type pairs repeat from the previous trial (free choice–Task 2 → free choice–Task 2) or switch (free choice–Task 2 → forced choice–Task 2). So there might be what has been coined task-pair switching costs in Task 1. More importantly, these costs might occur in Task 2 as well (Hirsch, Nolden, & Koch, 2017; Hirsch, Nolden, Philipp, & Koch, 2017). Such task-pair switch costs were in fact present in RT2, but they were independent of the spatial compatibility of response effects in Task 1.[5] Thus, participants might have represented free and forced choice trials as distinct tasks, but this did not affect effect monitoring.

Theoretical Implications

We now turn to the theoretical implications. The first question to be discussed is: why are action effects monitored at all? We already noted that the most obvious reason is to gather information about the success of one's own behavior. But another likely reason is to gather information about the consequences of one's own actions even if they are not, or not yet, predictable (cf. Experiment 5). Hence there might be two important functions of action effect monitoring. Take, for example, a switch that is mounted on the wall of a room. Usually, these kinds of switches control the lighting. Pressing the switch illuminates the room, and this association is a linkage of response and effect that most of us are very well attuned to. But what if, in our exemplary room, the switch did not trigger the lights, but powered the ceiling fan? We would enter the room and press the switch, but our expectation of a lit-up room would be heavily violated, because no lights turn on. However, something different and unexpected happens: we feel a soft breeze that the ceiling fan produces. This new and unexpected action effect is now not ignored, but some attention is allocated toward it, and this new covariation serves as a basis for future predictions (Hommel, 1998, 2003). When we enter the room the next time and press the switch, we then expect a soft breeze instead of an illuminated room, and the initially unexpected but nonetheless automatically registered covariation allows for a now predictable change in the environment. Consequently, every time the switch is pressed, attention is deliberately allocated toward the soft breeze to check whether the obtained association between switch and breeze is still intact. On the basis of this, effect monitoring might not only serve as a method to compare intended and achieved effects, but also serve as a method to learn these associations in the first place. In this view, effect monitoring can be understood as an automatic process, that can be adjusted flexibly when a sense of agency is added to the mix, and it is in charge of both *screening for* and *validation of* covariations between responses and effects in the environment.

One noteworthy aspect of the present data is that in Experiments 1 through 4 free choice RTs were shorter than forced choice RTs—a finding that contrasts with many previous observations (e.g., Berlyne, 1957; Janczyk, Dambacher, Bieleke, & Gollwitzer, 2015; Janczyk, Durst, et al., 2017; Janczyk, Nolden, et al., 2015; Pfister & Kunde, 2013; but see Naefgen, Caissie, & Janczyk, 2017, Experiment 1, also in a dual-task setting). Although the consistency of this observation points to some systematic reason for it, we can only offer speculations at present. One obvious difference to other studies is that the task and correct choice was prompted by spatial features of the stimulus, that is, the puzzle connector to the left or right in forced choice trials and to both sides in free choice trials. As noted earlier, this also varied the S1–R1 compatibility between the forced and the free choice task. If this were indeed the reason, we would expect the typical RT difference again, if always two connectors are presented but the color of the puzzle piece determines task and correct response.

Another interesting observation is that incompatible effects tended to delay responding in Task 2 as compared to compatible effects more (and significantly so in Experiment 2 and 3) when Task 1 was free choice rather than forced choice. This can be explained by assuming that during the effect monitoring stage, the R1–E1 mapping has to be recruited to compare the observed action effect to the intended action effect, based on the response that was given (*validation* function): In forced choice trials, the R1–E1 mapping has to be activated during the response selection of Task 1, to derive the response that is appropriate for the production of the required effect. When now a response is executed and an effect appears, the monitoring of this effect is comparably easy, because the R1–E1 mapping is already activated and can be employed for comparison immediately. However, for free choice trials, this is not necessarily the case: Here, in Task 1 the R1–E1 mapping does not have to be imperatively activated to choose between the two response alternatives, because both are appropriate (even though, participants seem to base their responses on the R1–E1 mapping in some of the free choice trials, otherwise we would not find a compatibility effect in Task 1). Assuming that participants choose, as instructed, one of the two end states before they respond, in a fraction of trials, but press one of the two buttons impulsively in the rest of the trials, without specifying beforehand which effect they wish to produce, this explains the on average weaker compatibility effect for Task 1, with a part of slow, derived responses, and a part of impulsive, fast responses. But also, this explains the stronger delay of Task 2. In the slow, derived responses, the

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[5] A 2 × 2 × 2 ANOVA on Task 2 RTs with Task 1 compatibility (compatible vs. incompatible), Task 1 transition (repetition vs. switch) as within-subjects factors, and experiment (1-3) as between subjects factor revealed faster responses with Task 1 repetitions as compared to Task 1 switches for both forced, $F(1, 92) = 5.09$, $p = .026$, $\eta_p^2 = .05$, and free choice trials, $F(1, 92) = 15.68$, $p < .001$, $\eta_p^2 = .15$, but no reliable interactions of task transition with the factor compatibility, $F\text{s} < 3.22$, $p\text{s} > .076$.

R1–E1 mapping has been activated already, so, as in the forced choice trials, a comparison between the anticipated and the actual effect can start immediately. However, for the impulsive, fast responses, the R1–E1 mapping has not been activated in advance. So for these trials, the R1–E1 mapping has to be recruited after the response execution, to see how the response relates to the effect and whether that R1–E1 mapping is still valid. So the fast and impulsive responses in Task 1 come with a greater effort to monitor the effects afterward, resulting in smaller compatibility effects for Task 1 at the cost of greater delays for Task 2 in free choice trials.

Finally, our results allow for a parsimonious integration of previous observations under the heading of response monitoring, which we already discussed in the introduction (Bratzke et al., 2009; Jentzsch et al., 2007; Ulrich et al., 2006). These observations altogether suggest that capacity-limited processes during and after the response in Task 1 delay a subsequent Task 2. This account assumes that stimulus and response features are reactivated and compared during the response production to ensure accuracy. However, on the basis of our results, these observations could equally be explained by effect monitoring: Even though not all actions produce a perceivable effect in the environment, all actions produce sensory feedback (proprioceptive, tactile) in the agent. Response monitoring could therefore be explained, not by a reactivation and comparison of stimulus and response features, but by assuming that, even in absence of a perceivable effect in the environment, proprioceptive and tactile action effects are monitored by checking them against the intended, anticipated proprioceptive and tactile consequences of the performed action. In this view, response monitoring can be construed as a special case of effect monitoring, namely monitoring of proprioceptive or tactile response feedback, under the broader umbrella of effect monitoring processes. With the current data, we can only make a case for tasks that contain a motor response. But even for tasks that are not directly motor related, we could assume that these nonmotor responses involve motor simulation that again may be monitored.

Conclusion

To conclude, the current experiments show that effect monitoring does influence performance and systematically delays the processing of a subsequent, unrelated task. This effect monitoring is conceivably in charge of both screening for and validation of covariations between responses and effects in the environment and thereby serves not only as a way to check whether an intended effect has ultimately been realized, but also as a mechanism to learn R-E associations in the first place.

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