Consciousness and Cognition 99 (2022) 103299

Contents lists available at ScienceDirect

Consciousness and Cognition

journal homepage: www.elsevier.com/locate/concog

Temporal Binding in Multi-Step Action-Event Sequences is Driven by Altered Effect Perception

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A R T I C L E I N F O

Keywords:
Temporal Binding
Sense of Agency
Intentional Binding
Perception
Multisensory Integration

A B S T R A C T

The perceived compression of the interval between a voluntary action and a subsequent consequence is termed temporal binding and serves as an implicit measure for sense of agency. In everyday life, oftentimes multiple actions are required for goal attainment, i.e., a multi-step sequence of actions has to be performed to evoke the desired effect. However, present-day research mainly assesses the sense of agency for single actions and effects. Preliminary research on the sense of agency in longer action-event sequences is inconclusive. To fill this gap, we studied temporal binding in multi-step action-event sequences. In two experiments (free and forced choice), we employed a temporal binding paradigm in which participants had to press two keys to evoke the corresponding effects. Overall compression of the interval between actions and effects was driven by strong effect binding for both effects, while there was no significant action binding in either of the experiments.

1. Introduction

When humans act to produce perceptual changes in their environment, they experience agency. The sense of agency is thought to comprise of two components, judgements of agency and feelings of agency (Synofzik & Vosgerau, 2012). While the former are conceptualized as explicit statements about a person's agency, the latter reflect a non-conceptual implicit sense of agency. The most common measure for explicit judgements of agency is to ask participants whether they caused a sensory event or not (e.g. Sidarus et al., 2013; Weller et al., 2017). These judgements of agency are thought to rely on retrospective evaluations of a person's causal beliefs. However, such judgements are highly susceptible to various biases and can be influenced by manipulating, for example, the contingency between an action and its consequence (Daprati et al., 1997; Moore, 2016).

Feelings of agency are more difficult to assess, as they are thought to reflect prospective aspects of the sense of agency. Haggard and Tsakiris (2009) proposed temporal binding as an implicit measure of sense of agency (see also Hughes et al., 2013). Temporal binding describes the phenomenon that the interval between causally linked sensory events is perceived as shortened (e.g. Humphreys & Buehner, 2009; Tsakiris & Haggard, 2003). Agency research draws on this observation as the perceived time points of voluntary actions and subsequent effects are believed to shift towards each other, which cannot occur when either of the two entities (action or event) happens in isolation. Specifically, the perception of the action is shifted towards the effect (=action binding) while the perceived timing of the effect is shifted towards the action (=effect binding). These perceptual shifts were initially thought to rely on a

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https://doi.org/10.1016/j.concog.2022.103299
Received 31 March 2021; Received in revised form 2 February 2022; Accepted 8 February 2022
Available online 17 February 2022

successful comparison of predicted and actual consequences as proposed by the comparator model (Blakemore et al., 1999). However, more recent research has shown that the intention to produce an event is no prerequisite for temporal binding, which questions its validity as a pure index of sense of agency, and points instead to the importance of causal relations and multisensory cues for this phenomenon to occur, (e.g. Buehner, 2012; Kirsch et al., 2019). Hoerl et al. (2020), for example, propose a framework for the sense of agency based on causal links that exist both with and apart from voluntary actions. This framework also considers causality beliefs as well as multisensory cues.

Research on multisensory integration explains temporal binding based on the perceptual certainty of the events to be judged, i.e., actions and effects (Ernst, 2006). Action shifts are typically substantially smaller than effect shifts, which in many cases mirrors the difference in perceptual certainty between action and effect (Cao et al., 2020; Ernst & Banks, 2002; Kirsch et al., 2019). While there are various cues such as the planning of a body movement or the proprioceptive feedback of that movement which provide actors with a fairly certain estimate of the actual timing of the action, there are far fewer temporal cues for the effect. Thus, the less certain event is shifted towards the more certain event (cf. Moore & Fletcher, 2012). This finding has been replicated in numerous studies using different task setups, stimuli, effects, and action-effect delays (for an overview see Moore & Obhi, 2012). All of these studies relied on a similar task setup using the Libet clock (Haggard et al., 2002; Libet et al., 1983). Participants watch a clock with a rotating clock hand on a screen while performing keypresses at leisure. In an operant block, these keypresses are followed by an effect, usually a tone. At the end of a trial, participants are asked to report the position of the clock hand at either the time of their action or the effect. In addition to the operant blocks, there are two baseline blocks in which participants either only press a key without eliciting an effect, or they only encounter the effect without prior action. Temporal binding is calculated by comparing the estimation accuracy in the baseline condition with that in the operant condition for action and effect separately.

While agency has been studied extensively for single action-event episodes, much less is known about agency in multi-step actions. However, such action sequences resulting in goal attainment are rather common in our daily life. Oftentimes, we do not expect our actions to have immediate results, or we only find out whether an action sequence was successful when the final goal state is reached. In multi-step actions, several sequential activities are required to produce a certain intended effect. For example, opening a safe requires several turns of the knob before the bolt snaps back and the door eventually opens. Such sequential activities thus include multiple actions and multiple action effects, which become apparent after the final action has been carried out. Goals in multi-step action sequences may be represented at different levels of abstraction, very broadly as simply attaining a certain effect, e.g., reaching the goal, or very narrow as motor commands, e.g., moving a limb. This is what Pacherie (2008) defined as D-, P-, and M-intentions. Distal intentions are directed at overall goals such as opening the safe while proximal intentions and motor intentions break these D-intentions down into sub-goals and finally immediate motor intentions required to specify the respective motor commands for turning the knob (Pacherie, 2008). While it appears easy and straightforward to assess explicit judgements of agency over more complex action-effect sequences, it is not trivial to study implicit feelings of agency for the same sequences as it is not yet clear which mechanisms underlie, for example, temporal binding and how stable the effects are over time. The question we addressed here is how much agency agents experience for individual actions and action effects in such multi-step action-event sequences.

Preliminary studies approaching this question from different perspectives and by relying on different measures yielded somewhat diverging results. For example, Ruess et al. (2018) studied sequences of one action and two effect tones. They found that even unintended effects occurring shortly after an intended effect are bound to the preceding action. In their experiments, the interval between the action and an intended tone was always fixed, whereas a second tone could follow at a random or fixed interval. In line with previous studies, temporal binding was strongest for fixed intervals, but random delays of 200–800 ms between the two tones also produced perceptual shifts of both effects towards the action. However, effect binding for the second tone was always smaller than for the first tone. The authors attribute this observation to the prolonged overall delay between the action and the second tone which in general diminishes temporal binding (Ruess et al., 2018).

Another study investigating the sense of agency for multiple actions rather than multiple action effects measured sensory attenuation using a psychophysical approach (Garrido-Vásquez & Rock, 2020). Participants performed one single or multiple keypresses to elicit a tone. The authors found sensory attenuation, as another implicit measure of sense of agency, to increase with the number of keypresses required to elicit an effect. However, explicit agency ratings were higher for single actions than for multiple actions.

In yet another preliminary study, Yabe et al. (2017) reported a decomposition of temporal binding in action-event sequences. Participants experienced either action-sound-action triplets or sound-action-sound triplets. In the action-sound-action condition there was neither temporal binding of the sound to the preceding nor the ensuing action, whereas in the sound-action-sound condition both sounds were perceptually shifted towards the action. Yabe et al. (2017) interpreted this observation as a perceptual decomposition of the triplet into two dyads, each necessarily including an action as an anchor. Consequently, both sounds were attracted by the intermediate action in the sound-action-sound condition, whereas an intermediate sound was equally attracted towards both flanking actions in the action-sound-action condition.

Imaizumi et al. (2019) found opposing results in longer sequences of alternations between actions and events. Participants actively or passively pressed a key which produced a tone, and then pressed a key again. One alternation sequence consisted of five keypresses interlaced with four tones. The study employed the interval estimation method, rather than time perception of individual events, as measure for temporal binding. Thereby, the perceived intervals between any two occurrences in the action-event sequence could be assessed. The results suggest different time perception biases on a local (dyadwise) and global (sequencewise) level. When comparing active and passive keypress conditions, there was no temporal binding in individual action-effect dyads within the action-event sequence, whereas there was a perceived shortening of the sequence in total, i.e., temporal shrinkage for the whole sequence. Thus, the active or passive movement manipulation did not seem to have an influence on the perceived duration of the individual intervals, whereas the overall length of the full sequence was perceived to be significantly shortened. These results however raise the question for underlying mechanisms that cause these observations. The paradigm employed by Imaizumi et al. (2019) seems ill-suited for further analyses as the interval estimation method does not allow to disentangle the perceived time points of individual events.

To sum up, in slightly different multi-step action setups different observations have been made. First, when one action produces two effects, effect binding for the second of two effects decreases with the absolute length of the interval between action and the second of two effects (Ruess et al., 2018). Second, explicit agency for an effect drops when it is produced by multiple rather than one action (Garrido-Vásquez & Rock, 2020). Third, effect binding occurs only when that effect is not followed by another action (Yabe et al., 2017). Fourth, in longer action-effect-action-effect sequences, there is no local temporal binding (consistent with Yabe et al., 2017), but possibly a shortening of perceived overall sequence duration (Imaizumi et al., 2019).

In the present paper, we explored the perceived timing of actions and effects in a situation in which multiple actions produce corresponding individual effects following the final action. This is a quite common scenario in social and technical settings. For example, it is common in many imitation situations, where a model demonstrates certain multi-step actions, which are then carried out by an observer, and which can thus be construed as an effect of the model's action (e.g. Pfister et al., 2013). Uttering individual words (e.g., "I solemnly swear…") which are then repeated by a counterpart at an inauguration is an example of verbal imitation. An example in technical environments might be turning the dial on a safe to first unlock and then open it. All in all, these interactions, social or non-social, are characterized by the peculiarity that we do not expect effects from single motor actions but rather from a sequence of multiple actions, e.g., words or gestures. The question we asked here is how sense of agency shapes the perceived time points of actions and effects in such scenarios.

In two experiments, we examined the perceived timing of two actions evoking corresponding effects at the end of the action sequence. We manipulated the level of choice between the two experiments to keep the task as similar as possible but still change the way the action sequence was represented – as one predetermined sequence or as individual freely chosen actions leading to a defined goal. Representing actions as individual freely chosen keypresses should render both the actions as well as the effects more separate as compared to having one sequence where the actions are less distinguishable. In the present study participants moved a cursor through a grid by pressing arrow keys (cf. Fig. 1). Each trial consisted of two keypresses that were afterwards followed by the two respective effects (action-action-effect-effect). Previous research on admittedly somewhat different situations suggests the following: If duration perception dilates on a local scale but compresses on a global scale (Imaizumi et al., 2019), we should find that the perceived timing of the first action moves to a later point in time, and the perceived timing of the last effect moves to an earlier point in time. The perceived timing of intermittent events should be left unaltered. However, if temporal binding in sequences is just a concatenation of binding dyads, temporal binding should occur between the second action and the first effect (Yabe et al., 2017) but not necessarily between the first action and the second effect. If temporal binding is, however, merely influenced by the absolute interval length between an action and any subsequent effect, the perceived timing of the first effect should be more biased towards the action than the perceived timing of the second effect just as the second action should show stronger action binding than the first one (Ruess et al., 2018).

2. Experiment 1: Forced choice

Experiment 1 tested for temporal binding in multi-step action-event sequences when participants had to follow a defined path in a grid with cursor movements. That is, participants had to perform two keypresses to move a cursor from a start area to a goal area. Only if these two keypresses were executed correctly, the cursor moved to the targeted locations, otherwise, an error message was displayed. In agreement with the referenced literature on binding in multi-step sequences, we expected to find a global compression of the entire

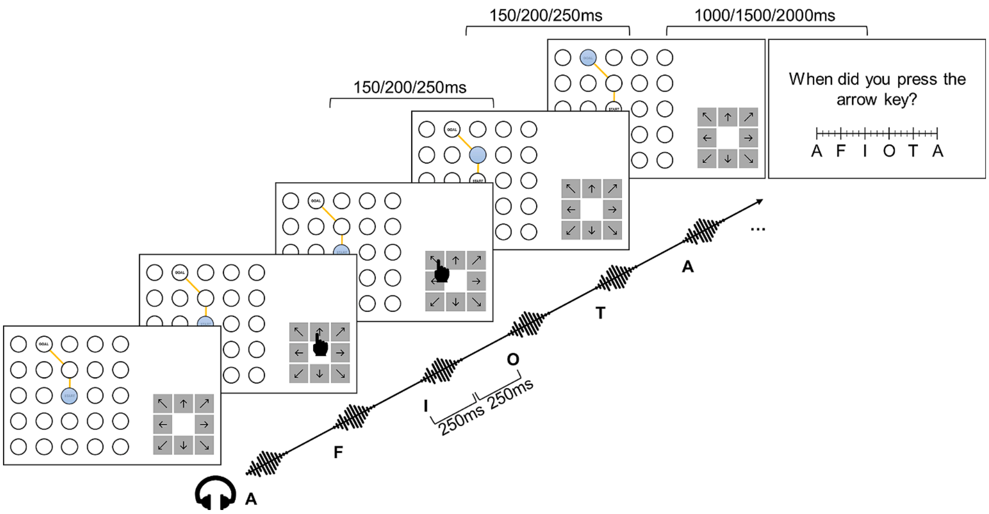

Fig. 1. Trial procedure in the experiments. Participants navigated a cursor (blue circle) through the grid by pressing the arrow keys located next to it. During task execution, participants heard letters over headphones that served to report the timing of either one of the keypresses or one of the cursor movements. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

action-event sequence, i.e., effect binding at least for the second effect and action binding for the first action. However, the different studies lead to competing predictions regarding the intermittent action and effect, thus, we were especially interested in analyzing the perceived timing of these. Therefore, we chose a paradigm, which allowed to assess the perceived timing of each individual event in the sequence.

All experiments were preregistered on the Open Science Framework (OSF) and were approved by the ethics committee of the psychology department of the Julius-Maximilians-University of Würzburg (GZ 2019-09).

2.1. Methods

2.1.1. Participants

Forty-eight participants (16 male, 4 left handed, mean age = 23.9, *SD* = 4.0) recruited over the university's participant pool (SONA) took part in the experiment and received monetary compensation for their voluntary participation. Prior to data collection we conducted a power analysis for paired sample *t*-tests using G*Power 3.1 (Faul et al., 2009). Expecting medium effect sizes (e.g., Ruess et al., 2017), the power analysis was performed with $d = 0.4$ and $\alpha = 0.05$. Consequently, 41 participants would have been required to ensure high power (0.80). However, for counterbalancing of the conditions, we recruited 48 participants. Prior to the experiment, they signed an informed consent form. All participants were naïve to the purpose of the study and were debriefed afterwards.

2.1.2. Apparatus and stimuli

2.1.2.1. Navigation task. Participants were asked to navigate a cursor through a 5 × 5 navigation grid by pressing arrow keys next to the grid (see Fig. 1). In Experiment 1 the cursor had to follow a predefined route that was indicated by orange lines connecting the dots in the grid, thus it was a forced choice task with visual effects, i.e., the cursor movements. Participants completed the task on an iPad Pro using only the index finger of their right hand. The iPad's LED screen, with a 12.9″ diagonal and a resolution of 2732 × 2048 px, was used in landscape mode. We chose the iPad for task execution to eliminate undesired consequences such as the proprioceptive feedback as well as possible auditory effects from pressing and releasing keyboard keys. As shown in Fig. 1, during task execution the screen showed a 5 × 5 grid of circles with a diameter of 100 px on the left part of the screen while a keypad with 8 arrows was displayed to its right. The keys were located slightly to the bottom of the display for ergonomic reasons. At the beginning of each trial, the center circle displayed the German word for start ("Start") and was filled in blue to illustrate a moveable cursor. Additionally, one of the outer 16 circles (except for the circles in the four corners) in the grid displayed the German word for goal ("Ziel"). The two areas were connected with straight orange lines indicating the keypresses that participants had to perform in the respective trial.

2.1.2.2. Auditory timer. Over the course of the trial, participants heard a loop of five timed letters over headphones. These were the German letters A, F, I, O, and T, which served as a timer to reference the perceived timing of the keypresses and cursor movements. This was done by indicating the exact position on a visual scale displaying all letters at the end of each trial – as shown in the last frame in Fig. 1. Participants were explicitly instructed to use the entire scale including the spaces between the letters to make their judgement as accurate as possible. Each of the letters was presented for 250 ms followed by 250 ms silence resulting in a loop length of 2500 ms[1]. This auditory timer presentation resulted in a temporal resolution where one pixel of the visual scale was equal to 2 ms. A representative example of the auditory timer as well as the exact instructions are available at this project's OSF page.

2.1.3. Procedure

The experiment consisted of one session lasting approximately 1.5 h. We used a procedure similar to the so-called Libet clock procedure (Haggard et al., 2002; Libet et al., 1983). However, instead of presenting a visual clock for the time estimations, we used an auditory timer, which allows to examine temporal binding for visually demanding tasks (Muth et al., 2021).

Throughout the experiment, participants encountered eight different conditions (see Fig. 2): (1) Action1 operant: Cursor movements followed participants' keypresses and the perceived timing of the first keypress was assessed. (2) Action1 baseline: Participants executed the required keypresses. However, they were not followed by any cursor movement. The perceived timing of the first keypress was assessed. (3) Action2 operant and (4) Action2 baseline followed the same logic as the first two conditions but in these conditions, the perceived timing of the second keypress was assessed. (5) Effect1 operant: Cursor movements followed participants' keypresses and the perceived timing of the first cursor movement was assessed. (6) Effect1 baseline: After a random interval of 1500–2500 ms the cursor movement along the predefined path occurred without participants' keypresses and the perceived timing of the first cursor movement was assessed. (7) Effect2 operant, and (8) Effect2 baseline followed the same logic as the two Effect1 conditions; however, in the latter, the perceived timing of the second cursor movement was assessed. The order of conditions was counterbalanced across participants.

Trials started with the presentation of the navigation grid on the left, the keypad on the right side of the screen and the letter sequence over headphones. The sequence's starting letter was selected randomly. Participants saw the start and goal area and the path to follow. Paths included all possible paths to any of the outer circles excluding those that required pressing any arrow key twice. Consequently, there were 24 different trials with two repetitions per block, i.e., 48 trials per block. At the beginning of the first block,

[1] For more details on the method, see Muth et al. (2021).

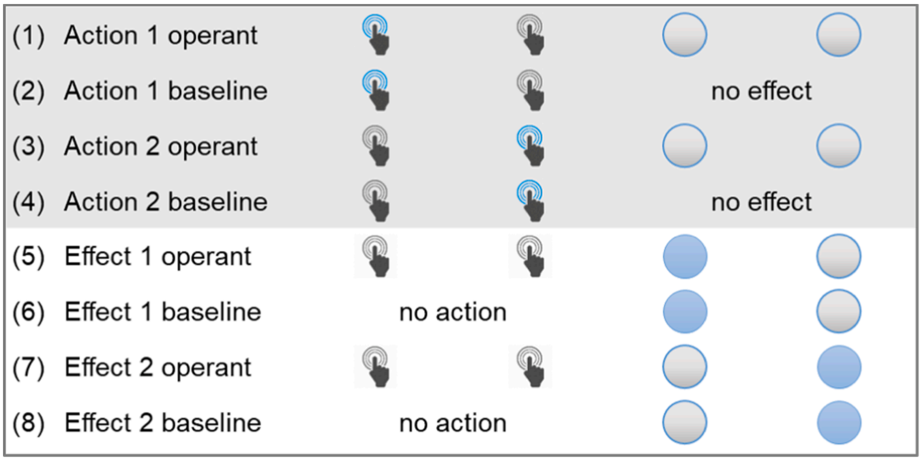

Fig. 2. Participants experienced 8 different conditions throughout the experiment. Four conditions in which they were asked to judge the timing of their action (1–4) and four conditions in which we assessed the perceived timing of the effects (5–8). Blue icons indicate the event to be reported in the respective condition. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

participants performed eight practice trials which were not presented during the following experimental blocks to familiarize with the task. These practice trials were all response repetitions, i.e., trials in which they had to press the same key twice. Additionally, the first two trials of the experimental blocks were also response repetitions serving the purpose of letting participants adjust to the new condition. We selected these paths to ensure that participants saw each path in the experimental blocks equally often. Practice trials were not included in the final analysis.

In the operant blocks and action baseline blocks, participants were asked to wait at least three letters and then press the two corresponding arrow keys at their leisure. Once both keypresses were executed in the operant conditions, the cursor moved first to the location of the first keypress and then to that of the second keypress. The cursor movement followed the second action with a random delay of 150, 200, or 250 ms and was displayed until the onset of the second cursor movement. The second cursor movement followed the first one with the same delay and was also displayed for either 150, 200, or 250 ms. In the action baseline conditions, the auditory timer continued for a random interval of 1500, 2000, or 2500 ms after the second keypress. In the effect baseline conditions, participants were to wait for the cursor movement without performing any action. Here cursor movements started 1500–2500 ms, i.e., 3–5 letters, after the auditory timer started with a random delay of 150, 200 or 250 ms between the cursor movements. If participants did not succeed in pressing the two correct keys, an error message reading the German word for error ("Fehler") in red was displayed in front of the grid for 750 ms and the trial aborted. Trials concluded with presentation of the visual scale of 1250 px width with 6 large tick marks for each letter of the auditory sequence as shown in Fig. 1. Note that the "A" was displayed twice so participants could make judgements both before and after the "A". Participants were asked to report the timing of either one of the two actions or one of the two effects by selecting the corresponding position on the scale. Temporal estimations were used to calculate temporal binding for both actions and both effects (see Data analysis. for more detail). Temporal binding was calculated as difference in estimation error between baseline and operant conditions.

Additionally in operant blocks, participants answered three questions on a visual scale from −50 to 50 rating their authorship, control, and causation over the cursor movements every eight trials. These judgements served as manipulation check, to confirm participants' perceived causality and control over the events. As mentioned above, such explicit agency ratings are susceptible to demand effects and are thus not the focus of our research interest.

2.2. Data analysis

Firstly, we calculated estimation errors as the difference between temporal estimation and actual timing of the respective event. We discarded erroneous trials, that is, trials in which participants did not succeed in pressing both correct keys. Additionally, trials in which estimation errors exceeded 2.5 *SD*s of the participant's cell mean in the respective condition were also dropped from the analyses. Secondly, we calculated means for each estimation condition individually. Subsequently, we subtracted each participant's mean estimation error in the baseline condition from that in the respective operant condition to obtain temporal binding. Positive values can be interpreted as perceived shift to a later point in time, while negative values indicate that an occurrence was perceived to have happened earlier in the operant than in the baseline condition. Two-tailed paired *t*-tests were calculated for each action and effect separately. Effect sizes for all *t*-tests were computed as $d_z = \frac{t}{\sqrt{n}}$.

Additionally, post-hoc Bayes analysis were used to inspect the evidence for and against the null hypothesis. Bayes factors with a scale parameter of 0.707 were calculated using JASP computer software (JASP Team, 2018). As per convention, a Bayes factor of $BF_{10} < \frac{1}{3}$ can be interpreted as evidence in favor of the null hypothesis, while Bayes factors greater than 3 yield at least moderate evidence for the alternative hypothesis (Dienes, 2014).

2.3. Results

Errors (3.1%) and outliers (2.9%) were very rare. Consequently, error rates will not be further analyzed here (see Dixon, 2008 for comments regarding floor and ceiling effects in the analysis of error data). Descriptively, errors occurred mainly in the action baseline conditions and happened more often for the second than for the first action. This is not very surprising, as participants had to perform both actions while the cursor remained at the starting position. Thus, on some trials, they pressed the first key again, indicating that they might not have been sure whether the first keypress was recorded. Additionally, they had to anticipate the first cursor movement to execute the second keypress correctly. The manipulation check was successful. Explicit agency judgements were high across almost all participants, $M_{\text{authorship}} = 24.9$ (20.9), $M_{\text{control}} = 24.8$ (21.2), $M_{\text{causation}} = 31.9$ (19.5), indicating that participants indeed felt authorship and control over the cursor movements.

2.3.1. Action binding

There was no significant difference in mean estimation errors between the operant and the baseline conditions neither for the first action, $t(47) = 1.19$, $p = .237$, $d_z = 0.17$, $\Delta = 20$ ms, $BF_{10} = 0.31$, nor for the second action, $t(47) < 1$, $\Delta = -8$ ms, $BF_{10} = 0.17$. Follow up Bayes analyses indicated at least moderate evidence for the null hypothesis in both cases. Additionally, the observed results did not differ between the two actions, $t(47) = 1.15$, $p = .166$, $d_z = 0.17$, $\Delta = 28$ ms, $BF_{10} = 0.29$, indicating moderate evidence for the null hypothesis. Consequently, we did not observe a perceived shift of either action towards the ensuing effects.

2.3.2. Effect binding

Data showed significantly larger constant estimation errors for operant conditions compared to baseline conditions for both effects. As shown in Fig. 3, the first effect was perceived to have happened earlier in blocks where participants had to press keys to move the cursor compared to conditions in which participants simply observed the cursor movements, $t(47) = -6.34$, $p < .001$, $d_z = -0.92$, $\Delta = -194$ ms, $BF_{10} = 1.74e^5$. The same held true for the second cursor movement. There was a perceptual shift of the second cursor movement towards an earlier point in time when the cursor movements were preceded by participants' keypresses, $t(47) = -4.70$, $p < .001$, $d_z = -0.68$, $\Delta = -117$ ms, $BF_{10} = 903.14$. However, the magnitude of these two shifts differed significantly, i.e., the forward shift of the first cursor movement was larger than that of the second cursor movement, $t(47) = -2.68$, $p = .010$, $d_z = -0.39$, $\Delta = -78$ ms, $BF_{10} = 3.79$.

2.4. Discussion

In Experiment 1 we aimed at studying temporal binding in sequences of two actions and two effects. We did not observe a shift of the perceived timing of actions, neither of the first nor of the second one. Contrary, we did observe effect binding for both effects, which was stronger for the first than for the second effect.

Interpreting the non-significant action binding in the light of multisensory integration would suggest smaller variability of estimation errors in the Action2 operant condition compared to the Action2 baseline condition as in the former agents should have more perceptual certainty due to additional temporal cues in form of the preceding action and the ensuing effect. A post-hoc analysis[2] contradicted this notion, variances were larger in the operant compared to the baseline condition. However, estimation errors for the first action did indeed show a numeric trend in the predicted direction. That is, the first action in the operant condition was descriptively shifted to a later point in time. However, this shift was merely marginal.

To shed further light on this, a second experiment was conducted in a free choice instead of forced choice design. Previous studies have found temporal binding to be stronger in an interval estimation task when participants could freely choose one of four actions to be executed as compared to situations in which the action was forced upon them (Barlas et al., 2018). This may increase the chances to observe specifically action binding for which Experiment 1 may have been too insensitive to detect (but see Schwarz et al., 2019 for a lack of influence of free vs. forced choice).

3. Experiment 2: Free choice

Experiment 2 was designed to test for temporal binding in sequences of multiple actions and subsequent effects when participants freely chose how to get from a start to goal area. To this end, participants could perform any two keypresses which would result in the desired end position of the cursor. If this was the case, the cursor moved along the path chosen by the participant, otherwise, an error message was shown. In contrast to Experiment 1, where participants might have represented the two keypresses as one predefined action chunk, we expected participants to maintain the final goal of reaching a certain target position of the cursor, while relying on more distinguished representations of the two actions. As free choice has been suggested to increase temporal binding, we expected the same to occur for this now (partly) free choice design as compared to Experiment 1.

[2] Analysis and reports can be retrieved from the project's OSF page: https://osf.io/2vey8/.

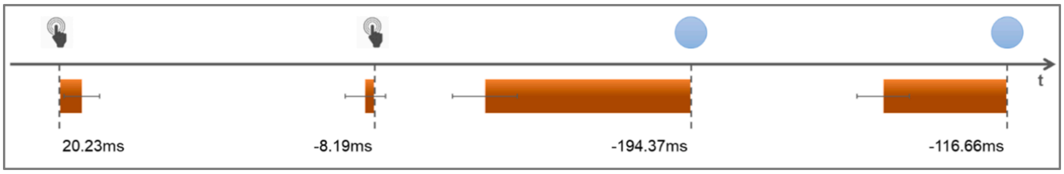

Fig. 3. Temporal Binding in Experiment 1. Action binding and effect binding relative to the respective baseline conditions. Dotted lines mark the perceived timing of the occurrence in the baseline conditions. Bars going from left to right indicate a perceived delay of the occurrence, while bars going from right to left indicate a perceived shift to an earlier point in time. Error bars depict standard errors of paired differences (Pfister & Janczyk, 2013).

3.1. Methods

3.1.1. Participants

A new set of forty-eight participants (14 male, 6 left-handed) with a mean age of 24.6 years (*SD* = 7.5) who fulfilled the same criteria as in Experiment 1 were recruited.

3.1.2. Apparatus and stimuli

The task was the same as in Experiment 1, only this time, there was no connecting line from the start to the goal area and participants could thus freely choose which two keys to press to reach the goal area.

3.1.3. Procedure

In principle, the procedure of Experiment 2 followed that of Experiment 1 with the difference that this time the path from start to goal area was not defined. While start and goal location were set, participants could freely choose how to navigate from one to the other. As in Experiment 1, the goal location could be any of the outer 16 circles sparing the four corners. Participants were asked not to press arrow keys twice so that each of the resultant 12 goal positions could be reached in two ways as in the first experiment. Each block consisted of 48 trials plus two practice trials at the beginning of the block to familiarize with the new condition. Additionally, this time we included a practice block of 16 trials at the beginning of the experiment which was the same for all participants, it was an Action1 operant block and goal locations were the four circles located directly above, to the left and the right, and below the start area. Practice trials were not included in the analysis.

During trials, participants were asked to wait at least three letters before performing two distinct keypresses (no repetitions) which would move the cursor to the goal position. Timing, cursor presentation, and the scale for temporal judgements were the same as in Experiment 1.

3.2. Data analysis

We analyzed the data in Experiment 2 according to the analysis plan described in Experiment 1.

3.3. Results

Errors (3.3%) and outliers (2.7%) again were rare and thus not further analyzed. Errors occurred mainly in action baseline blocks for the second action. However, it is noteworthy, that in this experiment, there were two first actions that were considered correct, as there were always two ways to get from start to goal area. Again, explicit agency judgements were high across almost all participants, $M_{\text{authorship}} = 32.7$ (19.5), $M_{\text{control}} = 29.7$ (24.1), $M_{\text{causation}} = 36.6$ (17.3), suggesting that participants felt authorship and control over the cursor movements.

3.3.1. Action binding

Fig. 4 shows the estimation errors in the operant conditions relative to the baseline conditions. There was no significant difference

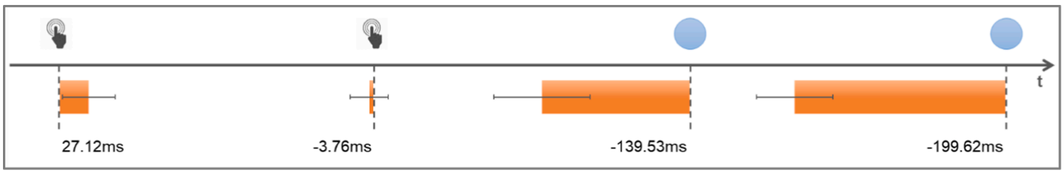

Fig. 4. Temporal Binding in Experiment 2. Action binding and effect binding relative to the respective baseline conditions. Dotted lines mark the perceived timing of the occurrence in the baseline conditions. Bars going from left to right indicate a perceived delay of the occurrence, while bars going from right to left indicate a perceived shift to an earlier point in time. Error bars depict standard errors of paired differences (Pfister & Janczyk, 2013).

in the size of estimation errors between baseline and operant conditions neither for the first action, $t(47) = 1.09$, $p = .282$, $d_z = 0.16$, $\Delta = 27$ ms, $BF_{10} = 0.27$, nor for the second action, $t(47) < 1$, $\Delta = -4$ ms, $BF_{10} = 0.16$. Bayes factors indicated moderate to strong evidence for the null hypothesis. A comparison between the perceived shifts of both actions did not show any significant difference, $t(47) = 1.05$, $p = .297$, $d_z = 0.15$, $\Delta = 31$ ms, $BF_{10} = 0.26$.

3.3.2. Effect binding

Estimation errors in the operant condition were significantly larger than in the baseline condition for both cursor movements. Hence, the perception of the first effect was shifted towards the preceding actions, $t(47) = -3.05$, $p = .004$, $d_z = -0.44$, $\Delta = -140$ ms, $BF_{10} = 9.03$, likewise, the perception of the second cursor movement was also shifted to an earlier point, $t(47) = -5.52$, $p < .001$, $d_z = -0.80$, $\Delta = -200$ ms, $BF_{10} = 1.17e^4$. A comparison between the effect binding for the first and second effect did not reach significance, $t(47) = 1.31$, $p = .198$, $d_z = 0.19$, $\Delta = 60$ ms. However, the post-hoc Bayes analysis did not find strong evidence for or against the null hypothesis, $BF_{10} = 0.35$.

3.4. Between experiment analysis

Following the two experiments, we conducted a between-experiment analysis to compare action binding as well as effect binding and see whether choice (free vs. forced) had an influence on the reported temporal binding. We expected choice to increase temporal binding.

To test this, we conducted a 2 × 2 × 2 mixed ANOVA with experiment as between-subjects factor and event (action vs. effect) and position (1st vs. 2nd occurrence) as within-subjects factors. Whenever there was a violation of the sphericity assumption, we applied Greenhouse-Geisser correction.

Results only partly supported our hypothesis. Letting participants freely choose which path to take did indeed influence temporal binding. However, it did not lead to a general increase of binding. Not surprisingly, there was a significant main effect of event (action vs. effect), $F(1,94) = 57.61$, $p < .001$, $\eta_p^2 = 0.38$, which did however not interact with the factor experiment, $F(1,94) < 1$. In contrast to this, there was no significant main effect for position (first vs. second), $F(1,94) < 1$. However, the interaction of position with experiment was significant, $F(1,94) = 4.35$, $p < .040$, $\eta_p^2 = 0.04$. This interaction was driven by the reversal of the pattern for effect binding. Thus, even though freedom of choice had an influence on the reported estimation errors, it did not increase temporal binding in general (Table 1).

3.5. Discussion

Experiment 2 was designed to facilitate temporal binding and elucidate the results found in Experiment 1 by introducing a free choice component to result in more separated action and effect representations while maintaining the same goal. We replicated most of the findings from Experiment 1. We again observed effect binding for both the first and the second effect. That is, the perceived timing of both these cursor movements was shifted towards the preceding actions. However, this time, there was no difference in the magnitude of the perceived shift between the first and second cursor movement. If anything, the perceived time point of the second effect moved more towards the preceding actions than that of the first effect. Thus, while the use of free choices shaped the degree of effect binding of the two effects, it had no influence on action binding. However, it is worth noting that the free choice manipulation we introduced might not have been as strong as in previous studies. To keep the experiments as similar as possible, we limited the number of possible choices to two by displaying a final goal to be reached. Hereby, participants could indeed select from two options for their first keypress, however, this choice restricted the second keypress to a single option.

4. General discussion

The present study examined temporal binding in action-effect sequences consisting of two actions followed by the same number of effects. Participants navigated a cursor from a start to a goal area by clicking on arrow keys on a touchscreen. They then judged the timing of either a keypress or a cursor movement, either when they occurred in isolation or as part of the action-event sequence. Between the two experiments, we manipulated whether the action sequence was predetermined, i.e., forced choice, or whether participants could freely choose which keys to press to get to the goal area.

Previous research has indicated that temporal binding does not only occur for single actions and subsequent effects, but also for single actions followed by multiple effects and sequences of alternating actions and effects. In line with these reports, we observed that

Table 1
Mean estimation errors (*SD*) for each condition in Experiment 1 & 2. Temporal binding is calculated by subtracting the estimation error in the baseline condition from that in the operant condition.

		Action 1	Action 2	Effect 1	Effect 2
Experiment 1	Baseline	−128,36 (102,27)	−83,13 (109,60)	−175,21 (92,18)	−129,33 (137,66)
	Operant	−108,14 (127,40)	−91,32 (152,00)	−369,57 (193,57)	−245,99 (152,94)
Experiment 2	baseline	−168,49 (145,43)	−94,06 (148,32)	−209,72 (129,01)	−118,62 (137,80)
	operant	−141,37 (210,44)	−97,82 (177,27)	−349,25 (286,96)	−318,24 (255,19)

the perceived timing of the cursor movements shifted towards the actions, if the actions preceded the cursor movements. However, we did not find a perceived action shift towards the ensuing effects. In addition, freedom of choice did not increase the overall temporal binding, rather it interacted with the serial order of the two effects. That is, with forced choice the first effect showed a greater temporal shift than the second effect while this pattern was non-significantly reversed with free choices.

Consequently, we could replicate results of previous studies showing that temporal binding does occur in multi-step sequences of actions and events. However, the architecture of these binding episodes appears to be more complex than previously suggested. According with the idea of global shrinking of the perceived time of an entire multi-step sequence (Imaizumi et al., 2019) we did observe that the final effect moves perceptually towards preceding actions. Yet, we barely found any action binding for the first action, while we found strong perceptual shifts of the first effect, which is inconsistent with idea of local dilation of perceived time between adjacent events. Moreover, we did observe that the first effect of a multi-step sequence is perceived as occurring earlier, which accords with the idea that multi-step actions are a concatenation of individual dyads (Yabe et al., 2017). Yet, we also observed large temporal binding for the second effect (numerically even larger than that for the first effect in Experiment 2), which is inconsistent with the construction of dyads between adjacent actions and effects. Finally, we did observe stronger binding of the first effect, consistent with the idea that binding of the second of two effects shrinks because it is necessarily more separated in time from the preceding action(s) (Ruess et al., 2018). Yet, we did so only in forced choice conditions (Experiment 1) but not in free choice conditions (Experiment 2).

The specific use of temporal binding allows us to dig deeper into how agents implicitly reconstruct the causality of events in multi-step sequences. We consider two such reconstructions plausible. First, both effects might be construed to be caused by the second action (or an action-sequence consisting of nominally distinct but grouped actions – A1 and A2; cf. Verwey et al., 2015). In that case, temporal binding for the first effect should be significantly larger than for the second effect as proposed by Ruess et al. (2018). Second, the first action (A1) might be construed as causal for the first effect (E1) and the second action (A2) might be construed as causal for the second effect (E2). In that case, A1E1-binding should be smaller than A2E2-binding, as the absolute interval length between the first action and first effect was on average significantly longer than the absolute interval length between second action and second effect (592 ms and 400 ms). Results found in Experiment 1 are more consistent with the first model proposing that A2 was perceived as causal for both E1 and E2. Yet, although objective timing of effects was held constant in both experiments, causality was construed differently in Experiment 2. Here results were more in line with the second model, proposing that E1 was construed a consequence of A1, and E2 was construed a consequence of A2.

Why did this change in causal reconstruction occur? While in the first experiment, already the first cursor movement perfectly reliably indicated that and how the final goal area would be reached, the effect sequence was probably represented differently in the second experiment, i.e., as two individual effects of the chosen actions. During trials in the second experiment, participants had to make choices and probably form intentions to move the cursor along a path of their choice. Here, participants had to monitor both cursor movements more closely to ensure that they followed the freely chosen keypresses. Therefore, there was less reason to interpret the two cursor movements as one movement in the second experiment and more reason to follow the cursor movements more closely in the second experiment. Additionally, the causal link between first and second cursor movement might become more apparent in the second experiment, as in Experiment 1 participants knew how the cursor would move even before forming an intention or having pressed any key, whereas in Experiment 2, the first cursor movement determined participants' path to success. Thus, we assume that the causal link between the two cursor movements was stronger in Experiment 2 as compared to Experiment 1 (Hoerl et al., 2020).

While the sequence of cursor movements was predetermined in Experiment 1, it was not in Experiment 2, probably prompting cursor movements to grab more attention in Experiment 2. This would be in line with the results of Ruess et al. (2018) who found less binding for a second, irrelevant, effect of a single action. Apart from attention, the effects might also have acquired positive valence, which has shown to increase temporal binding (e.g., Christensen et al., 2016; Takahata et al., 2012; Yoshie & Haggard, 2013, but see also Moreton et al., 2017). E2 was more indicative of goal achievement (which likely comes with positive affect) in Experiment 2, whereas E1 was more indicative goal achievement in Experiment 1.

It is noteworthy that we did not replicate previous observations of a general increase of temporal binding when either the identity or timing of the actions is freely chosen rather than forced (Barlas et al., 2017, 2018). Note though, that recent studies have challenged this impact of free vs. forced choice, suggesting that it is by far no ubiquitous observation (Antusch et al., 2021; Schwarz et al., 2019).

4.1. Limitations

The presented study design comes with limitations. Most notably, we chose an action sequence as baseline condition, i.e., a situation in which participants had to perform two keypresses that were not followed by a visual effect. It is possible that temporal binding might already occur between these two events in the baseline condition. Humans struggle to give the exact time point even of voluntary movements, especially when sensory certainty is very low (Wolpe et al., 2013). Any additional temporal cue e.g., internal cues, such as the plan to execute the movement or motor commands, and external cues, such as proprioceptive feedback of the tablet surface or subsequent events, influence judgements on these time points (Farrer et al., 2013). Therefore, it seems plausible that some kind of temporal integration occurred between the two actions even in the baseline conditions. Consequently, any binding towards subsequent effects that could possibly have occurred in the operant condition might have been overshadowed.

5. Conclusion

The present line of research investigated temporal binding in multi-step action-event sequences in a free choice and a forced choice paradigm. Results revealed temporal binding in both experiments. For the first time we could show that events at the end of multi-step motor sequences even with temporal delays are perceived as self-generated. This perceived compression of the interval between participants' actions and subsequent effects was due to the perception shift of the events, i.e., the effect binding. There was no action binding for either action in either experiment. This calls for further research on the mechanisms behind and the cognitive architecture of temporal binding per se and especially its extension to multi-step sequences.

Funding

This research was funded through a scholarship of the German Academic Scholarship Foundation (Studienstiftung des deutschen Volkes) awarded to FM. We would like to thank for the financial support.

CRediT authorship contribution statement

Felicitas V. Muth: Conceptualization, Methodology, Formal analysis, Investigation, Data curation, Writing – original draft, Writing – review & editing, Visualization, Project administration, Funding acquisition. **Robert Wirth:** Software, Writing – review & editing. **Wilfried Kunde:** Supervision, Writing – review & editing, Resources.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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