

Intentional Binding – Merely A Procedural Confound?

Julian Gutzeit¹, Lisa Weller¹, Jens Kürten¹, Lynn Huestegge¹

¹Julius-Maximilians-Universität Würzburg

Author Note

Julian Gutzeit <https://orcid.org/0000-0002-1434-1645>

We have no known conflict of interest to disclose.

Correspondence concerning this article should be addressed to Julian Gutzeit, Julius-Maximilians-Universität, Röntgenring 11, 97070 Würzburg, Germany. Email: julian.gutzeit@uni-wuerzburg.de

Raw data, analysis scripts, PsychoPy scripts, and stimuli materials are available on

<https://osf.io/pr58y>

Contact Information:

Lisa Weller, Röntgenring 11, 97070 Würzburg, Germany; lisa.weller@uni-wuerzburg.de

Jens Kürten, Röntgenring 11, 97070 Würzburg, Germany; jens.kuerten@uni-wuerzburg.de

Lynn Huestegge, Röntgenring 11, 97070 Würzburg, Germany; lynn.huestegge@uni-wuerzburg.de

Keywords: action and perception, sense of agency, temporal binding, intentional binding

Word Count: 10621

© 2023, American Psychological Association. This paper is not the copy of record and may not exactly replicate the final, authoritative version of the article.

Please do not copy or cite without authors' permission. The final article will be available, upon publication, via its DOI: 10.1037/xhp0001110

Abstract

Sense of agency (SoA) is the feeling of having control over one's actions and their outcomes. Previous research claimed that SoA is reflected in "intentional binding" effects, that is, the subjective compression of time between a voluntary action and an intended outcome. Conventional paradigms, however, typically lack an isolated manipulation of different degrees of agency (or intentionality), as the presence or absence of actions (along with subsequent perceptual changes) represents a potential confound variable. Using a newly developed paradigm, we were able to replicate typical "intentional binding" results in an initial experiment in which such a confound was deliberately included. We then eliminated this confound in a follow-up experiment by keeping the presence of actions and perceptual changes constant between conditions with and without agency while only manipulating subjective SoA. Here, explicit ratings showed that participants indeed felt responsible for effects in the Agency condition but not in the Baseline condition (demonstrating the successful manipulation of SoA), while we no longer found any differences in "intentional binding" effects between conditions. This indicates that previously reported relations between intentional binding and SoA could be merely based on procedural confounds. In particular, temporal compression effects usually interpreted in terms of "intentional binding" may rather result from more basic temporal grouping mechanisms for any (perceptual and/or motor) events that are perceived as meaningfully belonging together (e.g., as parts of a trial episode).

Public Significance Statement

Numerous previous studies showed a subjective compression of the temporal interval between a volitional action and an intentional effect, referred to as intentional binding. In this study, we show that these effects may be merely caused by procedural shortcomings, and that intentional binding must therefore be viewed critically.

1. Introduction

The feeling of being in control over one's actions and subsequent sensory outcomes of these actions is called sense of agency (Gallagher, 2000; Haggard et al., 2002; Moore & Obhi, 2012). To measure sense of agency, different approaches co-exist. For example, subjects can be asked explicitly whether they feel they have caused an event by their action. However, such introspective judgements might be biased (Dewey & Knoblich, 2014; Sato & Yasuda, 2005; Wegner & Wheatley, 1999). Thus, many authors additionally (or even preferably) use implicit agency measurements, such as temporal binding (sometimes, foreshadowing its standard explanation, also referred to as *intentional* binding; Haggard et al., 2002; Haggard & Tsakiris, 2009; Moore & Obhi, 2012; Tsakiris & Haggard, 2003), to assess sense of agency. Intentional binding refers to the phenomenon that the interval between a voluntary action and its subsequent perceptual consequence is perceived as temporally compressed (for review, see Tanaka et al., 2019).

Intentional binding can be explained in the context of a standard model of time estimation. Such a model assumes a pacemaker, which emits constant pulses, and an accumulator, which adds up these pulses and infers an interval duration from them (e.g., Block & Zakay, 1996; Gibbon et al., 1984). It has been theorized that intentional binding occurs due to a slowing down of a specific (Fereday & Buehner, 2017) pacemaker during an interval between causally linked events (Hoerl, 2019), thereby leading to fewer pulses emitted during the inter-event interval. This mechanism could be at play for both time judgement tasks involving direct time estimations (Humphreys & Buehner, 2009) and interval reproduction tasks (Humphreys & Buehner, 2010). As an alternative explanation, it has been speculated that an attentional shift away from perceiving the passing of time towards causal intentional actions might occur, thereby resulting in a missing more pulses of the pacemaker on behalf of the participants (Humphreys & Buehner, 2010; Zakay & Block, 1997). Such a mechanism would also predict shorter time estimations for intervals in tasks with a greater need for attentional resources (due to increased task demands).

To assess intentional binding, one can, for example, measure shifts in the perception of the individual time points of both the action (which should be delayed) and the effect (which should be pre-dated relative to a baseline) by using a clock procedure (Haggard et al., 2002; Libet et al., 1983; Tanaka et al., 2019): Participants report the position of a constantly rotating (visually presented) clock hand that temporally corresponds to the respective event. Alternatively, temporal distortions may be measured by having participants estimate the duration of the time interval between action and effect, for example, by means of reproduction (Engbert et al., 2007; Humphreys & Buehner, 2010). To quantify the presence and amount of such time estimation biases, an experimental condition (involving an action and a subsequent perceptual effect that was intentionally produced with that action) must be compared with a baseline condition. In most studies, one out of two different types of baseline conditions – corresponding to two distinct experimental paradigms – has been implemented. In the Libet clock paradigm (Libet et al., 1983), participants indicate the time point of the onset of an action or a (subsequent) event by reporting the position of a rotating clock hand in the experimental (operant) condition (e.g., Beck et al., 2017; Muth et al., 2020; Ruess et al., 2020; Schwarz et al., 2019; Tsakiris & Haggard, 2003). The time estimation of action and event in the experimental condition is then contrasted with the time estimation of an action without a subsequent effect or an effect without preceding action (*single event baseline*, adopting the terminology from Haggard et al., 2002). The other paradigm commonly used to address temporal binding is the interval estimation procedure, in which participants directly estimate or reproduce the action-outcome interval (e.g., Cravo et al., 2013; Desantis et al., 2012; Engbert et al., 2007; Graham-Schmidt et al., 2016; Humphreys & Buehner, 2010; Poonian & Cunnington, 2013). In the following, we use a representative study to explain the general interval estimation procedure that was also used in the present study.

In a study by Humphrey and Buehner (2009), participants were presented with ‘operant’ and ‘observational’ trials (Humphreys & Buehner, 2009). In operant conditions, participants performed an intentional action that caused a delayed auditory effect. In observational

(baseline) trials, participants just listened to two different auditory stimuli without performing any action at all. After each trial, participants estimated the duration of the interval between either their action and the auditory stimulus (operant condition) or the interval between both auditory stimuli (observational condition). Adopting the terminology from Humphrey and Buehner (2009), we will refer to this type of baseline as an *observational baseline*.

With both experimental designs outlined above, researchers aim to selectively manipulate the sense of agency (or degree of intention) between experimental and (*single-event* or *observational*) baseline conditions, as participants are able to intentionally produce an effect with their action in the experimental condition, while this is not the case in both types of baseline conditions. However, we suspect that the selective manipulation of the sense of agency (or degree of intention) may be severely compromised in these experimental designs due to confounds based on other procedural differences between conditions. We describe possible confounds of the time estimation procedure in detail in the following (for potential additional confounds in the context of the Libet clock paradigm, see General Discussion).

First, the observational baseline involving the timing of an external event does not only lack the presence of agency (or intention), but it additionally lacks the presence of any overt behavior or action whatsoever (apart from what is needed for the time estimation routine later on). However, there is a substantial body of research indicating that body movements can significantly affect time perception. Specifically, there is ample evidence for the possibility of distortion (De Kock et al., 2021; Press et al., 2014; Tomassini et al., 2014; Yokosaka et al., 2015; Yon et al., 2017) or the improvement (Hagura et al., 2012; Tomassini et al., 2012; Wiener et al., 2019) of temporal perception in the context of manual body movements. Moreover, even the mere preparation of body movements appears to bias temporal perception (Hagura et al., 2012; Tomassini & Morrone, 2016). Thus, if experimental conditions differ not only in terms of the degree of agency but also in whether behavior is involved in one condition but not in the other, then resulting differences in performance cannot be solely ascribed to agency or intention, as the presence of body movements may represent a potential confound.

Some studies tried to address this issue by demonstrating intentional binding effects for voluntary actions in the context of control conditions involving involuntary movements and following effects (e.g., Haggard et al., 2002; Tsakiris & Haggard, 2003). However, these studies make use of the Libet clock procedure and might thus be not comparable with interval estimation procedures. Moreover, and apart from the general fact that a passive finger movement may have more in common with a sensory (proprioceptive) event than with a typical action, these findings might be compromised by the introduction of temporal predictability as yet another confound. Subjects could not predict the onset of involuntary actions in these studies as they were induced externally, for example, by transcranial magnetic stimulation. The predictability of events however, can impact time perception (e.g., Pariyadath & Eagleman, 2007). When controlling for temporal predictability in such a design, other researchers no longer found any differences between temporal binding of involuntary and voluntary action conditions (Kirsch et al., 2019).

In addition, there are other studies casting doubt on the *specific* link between sense of agency and intentional binding. In a study by Desantis et al. (2012), participants produced a tone by pressing a button. The authors manipulated the predictability of the effect and found no stronger temporal binding when participants were able to predict the exact auditory effect (as compared to just any auditory effect), showing that intentional binding could be driven by the mere presence of an action, not by the accuracy of the prediction of the action effect. In another study by Suzuki and colleagues (2019), participants underestimated the time interval between a voluntary action (a press of a virtual key in a VR setting) and a subsequent auditory tone compared to an observational baseline, presumably indicating intentional binding. Crucially, however, participants also showed underestimations of the same magnitude of the temporal interval between a pre-recorded ('fake') action matched for spatial and temporal characteristics to those of the intentional action. The authors thus showed that temporal binding might not be specifically linked to intentional action and its outcomes but might rather be accounted for by assuming multisensory causal binding (see also General Discussion).

Taken together, these studies showed that temporal binding can occur between any causally related events (without the effect having to be predictable or the action itself actually having to be performed), thereby casting doubt on the *specific* relationship between intentional binding and sense of agency. However, it is important to note that such a causal approach to explaining intentional (or temporal) binding does not necessarily question the general relationship between sense of agency and temporal binding per se, but rather suggests that sense of agency might represent a special case of causally related events when it comes to temporal binding effects (Hoerl et al., 2020).

Taken together, it appears difficult to ascribe temporal estimation differences solely to the degree (or presence/absence) of agency (or intention) alone. What is (ideally) needed is the presence of a distinct behavior and a subsequent perceptual event in both the experimental *and* baseline condition, while the only aspect that varies is the degree (or presence/absence) of agency/intention establishing a causal link between the action and the subsequent event. In the following, we will describe a novel procedure, which aims to eliminate the confounds referred to above by keeping the presence of body movements and a subsequent perceptual event (and thus the number of corresponding events and tasks) constant between conditions, while only manipulating the subjective causal link between actions and effects (i.e., the intention to produce the effect by executing the action) and thus manipulating only sense of agency proper.

In our agency condition, participants performed a button press, which caused the color of an on-screen stimulus to change after a short delay. To assess intentional binding, participants were asked to reproduce the time interval between the button press and the color change. In the newly developed baseline condition, participants also pressed a button voluntarily and observed a color change following their button press, but participants should not feel responsible for this color change. This was achieved by randomly and consistently changing the colors of the stimuli throughout the trial in the baseline condition, independently from the participants' actions. After they pressed the button, the color of the stimulus changed for one

last time, but, crucially, from the viewpoint of the participants it appeared that the stimulus would have changed its color ‘anyway’, that is, regardless of the action of the participant. Henceforth, we will call this novel baseline *non-intentional effect baseline*. As a proof of the effectiveness of this procedure, we expected participants to report a substantially lower sense of agency in this novel baseline condition (vs. the agency condition).

It is important to note that our present paradigm also differed in certain other respects from previous typical agency studies. Apart from the specific nature of the stimuli employed here, we also decided to utilize a temporal interval reproduction procedure. While this differs from other studies using direct temporal interval estimations (e.g., Engbert et al., 2007; Suzuki et al., 2019), temporal reproduction has also been successfully applied in research examining intentional binding (Dewey & Knoblich, 2014; Humphreys & Buehner, 2010). In general, results based on interval estimations were shown to be comparable to those based on interval reproductions, as (motor) interval reproduction techniques yielded similar levels of accuracy and precision as (visual or verbal) interval estimations, at least for the short temporal intervals relevant here (Damsma et al., 2021). However, in order to demonstrate that our paradigm is overall capable of replicating typical intentional binding results repeatedly reported in the literature (despite the differences regarding stimuli characteristics and temporal reproduction procedures), we first conducted an experiment involving a classical *observational baseline* condition. In the second experiment, we report the results involving our new *non-intentional effect baseline*, which eliminates procedural confounds related to the presence/absence of behavior, event segmentation, attention, and cognitive load. The experiments’ sample sizes, variables, hypotheses, data treatments, and analyses were preregistered on Open Science Framework (<https://osf.io/pr58y>) before any data were collected. Raw data, analysis scripts, PsychoPy scripts, and stimulus materials are also available on <https://osf.io/pr58y>. The experiments were approved by the ethics committee of the psychology department of the Julius-Maximilians University of Würzburg (GZEK 2021-87).

2. Experiment 1

To demonstrate the capability of our paradigm to produce classical intentional binding effects, we replicated a standard *observational baseline* condition paradigm in this experiment: A stimulus appeared and changed its color without any action on behalf of the participants; afterwards they estimated the inter-event interval between stimulus appearance and color change by means of interval reproduction. These inter-event interval estimations are then compared with estimations of inter-event intervals between keypresses and produced color changes in an operant (agency) condition.

2.1. Methods

2.1.1. Participants

From preliminary data based on a student's thesis, we calculated effect sizes for temporal binding of $d_z = 0.47$ (two conditions, paired two-sided t-test).

Humphreys and Buehner (2010), however, reported results resulting in larger effect sizes of Cohens $d_z = 0.68 - 0.82$ (Experiment 1: $d_z = t / \sqrt{N} = 4.94 / \sqrt{36} = 0.82$; Experiment 2: $d_z = t / \sqrt{N} = 4.33 / \sqrt{40} = 0.68$). However, since we employed a new paradigm with distracting stimuli (see section 2.1.3), the intentional binding effect might have been smaller than typically observed. Thus, it seemed appropriate to determine our sample size anticipating a smaller effect of roughly $d_z = 0.5$, in line with the preliminary data reported above. We computed a power analysis with R-package *pwr* (Champely, 2020) based on an effect of $d_z = 0.5$ (two-sided paired t-test, two conditions: baseline vs. agency) and $\alpha = 0.05$. It showed that a sample size of $n \approx 33$ would yield a power of $1 - \beta = 0.8$. To achieve an equal set of participants in the eight different counterbalancing conditions (see below) we recruited 40 participants (ensuring power of $1 - \beta = 0.87$). We excluded one participant who had difficulties in understanding the task because she was not a native German speaker.

As we did not foresee this particular problem (we advertised the experiment for German native speakers only, but the non-German subject participated nevertheless), we therefore had to deviate from our preregistration criteria with this exclusion. We recruited one new participant as a replacement to finally include $n = 40$ participants in our data analysis (mean age: 25, SD: 5.04, range: 18-47, 75% female, 95% right-handed). All participants gave informed consent, reported normal or corrected-to-normal vision, and were compensated monetarily or with course credits.

2.1.2. Setup and Stimuli

Participants were seated at a distance of about 70 cm in front of a 24" LCD monitor (resolution 1920 x 1080 pixels, refresh rate 100 Hz). For responses, a standard German QWERTZ keyboard and a computer mouse were used. Stimulus presentation and logging of responses were implemented using PsychoPy 3.0 (Peirce et al., 2019). We used four different stimuli (moon, sun, rocket, house), all sized 30 x 30 pixels (1.17 cm x 1.17 cm, visual angle $\approx 1.35^\circ$). The stimuli were presented in different colors (white, yellow, blue, orange, green, purple) on a black background. All stimuli were displayed 6.8 cm above, below, to the left, and to the right of the screen center.

2.1.3. Task

In each trial, all four different stimuli were presented simultaneously around the center of the screen. Two opposite stimuli (i.e., either the two stimuli above and below the center or those to the left and right) continuously and simultaneously changed their color (blue, orange, green, or purple) at random intervals (500 – 1100 ms). The order of color changes was randomized across all subjects but fixed for each subject during the experiment. These stimuli were irrelevant for the task. The other two opposite stimuli retained their white color throughout. We counterbalanced which stimulus pair (rocket and house vs. sun and moon) changed its color and whether the color changing stimulus pair was positioned above/below or to the left/right of the screen center.

Participants underwent two different conditions (baseline, agency) in four blocks. The conditions alternated between blocks, and the starting condition was counterbalanced across participants, resulting in eight counterbalancing conditions in total (stimulus position x stimulus identity x starting block condition).

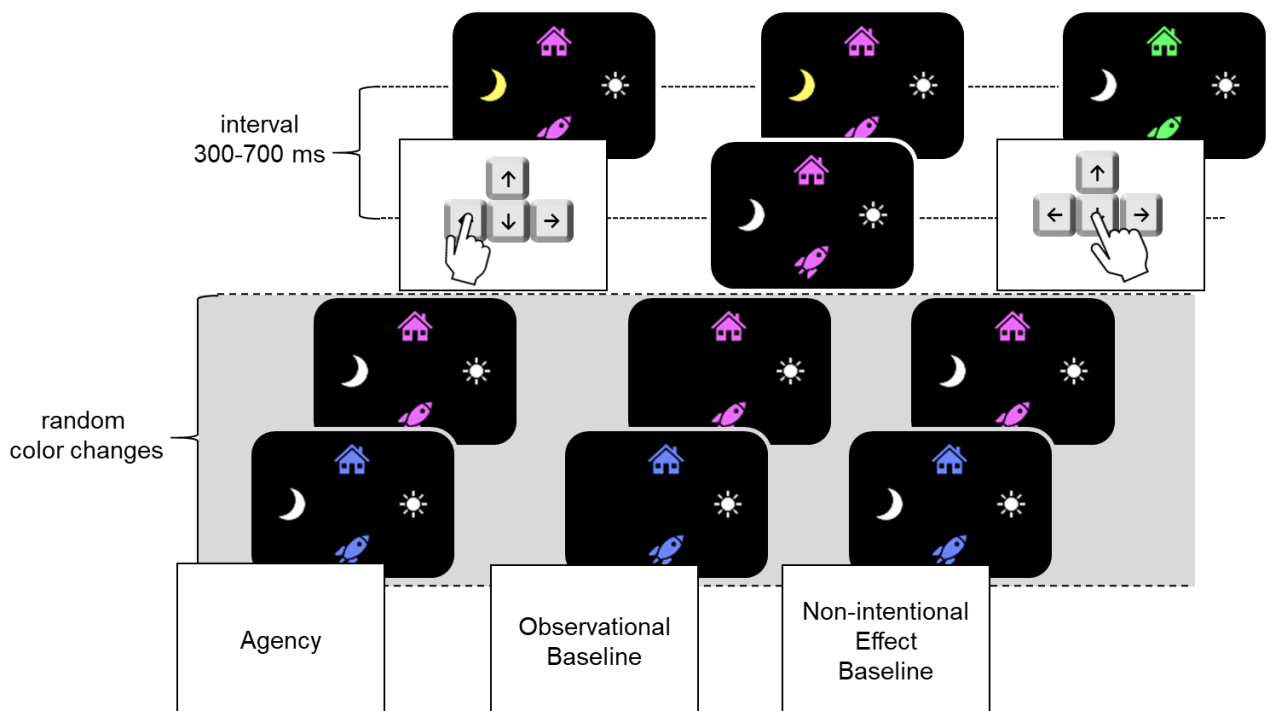
In the *baseline condition*, both stimuli that randomly changed their color and only one of the two white stimuli was displayed. The other white stimulus appeared after a random time interval of 750-1250 ms. After an inter-event interval of 300, 500, or 700 ms, this newly appearing stimulus changed its color to yellow. During the first baseline block, this happened for only one of the stimuli (e.g., the moon) while the opposing stimulus (e.g., the sun) remained on the screen throughout the whole trial. In the next baseline block, this procedure was applied to the stimulus on the other side (e.g., the sun). We instructed participants to pay attention to the inter-event interval between stimulus appearance and its color change.

In the *agency condition*, all stimuli were visible from the start of the trial. Participants pressed an arrow key corresponding to either one of the white stimuli at a time of their choice. To ensure that they would observe at least one color change, however, they were instructed to wait for a short time with their action after each trial start. To ensure compliance, when they pressed the key earlier than 1100 ms after trial start an error message ('Too early!') was displayed and the trial was restarted. Those re-started trials were later excluded from our analyses. Again, after a temporal inter-event interval of 300, 500, or 700 ms following their keypress, the stimulus changed its color to yellow. We instructed participants to pay attention to the inter-event interval between their keypress and the color change of the respective (self-chosen) stimulus. The other stimuli no longer changed color after the key was pressed.

After each trial, subjects estimated the inter-event interval between appearance of the crucial stimulus (baseline) or keypress (agency) and the subsequent color change by holding down the space bar for as long as they thought the inter-event interval lasted. Note that in this experiment, the random color changes of the other stimuli were not relevant for

the participants. Those stimuli stopped changing their color after appearance of the crucial stimulus (baseline condition) or after the keypress (agency condition). We included these irrelevant stimuli to keep the stimuli characteristics constant between this experiment and Experiment 2. For an overview, see Fig. 1.

Prior to the first block, the participants completed 12 training trials (6 for each task) to familiarize themselves with both tasks. All three inter-event intervals (300, 500, 700 ms) were repeated twelve times in each condition, resulting in 36 trials per block and 144 trials in total. In a sixth of the trials (two randomly selected trials for each delay in each block), participants were asked to provide an explicit agency rating after the temporal reproduction task. To this end, they answered the question ‘How much did you feel responsible for the yellow color change in the previous trial?’ (German: ‘Wie stark hast du dich gerade eben im letzten Durchgang für den gelben Farbwechsel verantwortlich gefühlt?’) on a visual analogue rating scale ranging from ‘not at all’ (0%) to ‘a lot’ (100%).¹



¹ While the estimation was recorded as a value between 0% and 100%, participants only saw the verbal poles on the scale, not any numeric labels.

Fig. 1.

Trial structure of the *agency* condition (Experiment 1, Experiment 2), the *observational baseline* condition (Experiment 1), and the *non-intentional effect baseline* condition (Experiment 2). Random color changes stop with the button press (*agency, non-intentional effect baseline*) or the appearance of the stimulus (*observational baseline*).

After participants concluded the experiment, they rated the afore-presented stimuli regarding their valence and arousal on a visual analogue scale and reported any strategies they used for estimating the inter-event intervals during the task. In addition, they rated the task of actively changing the color (i.e., the *agency* condition) with the User-Experience-Questionnaire (UEQ; Laugwitz et al., 2008), a questionnaire rating usability including six factors (Attractiveness, Perspicuity, Efficiency, Dependability, Stimulation, and Novelty), containing 26 bipolar items. These data were merely collected to compare usability in this task with that in a task from another study. We will not report the results in this paper.

2.1.4. Statistical analysis

We analyzed all data using repeated measures ANOVAs or paired t-tests (in case of only two factor levels) in R (Core R Team, 2021). To evaluate temporal reproduction errors as an index of temporal binding, we subtracted the actual inter-event interval from participants' estimations. Values < 0 s thus correspond to an underestimation of the inter-event interval, whereas values > 0 s indicate an overestimation. For the analyses of agency ratings, note that we could only include trials in which the explicit ratings were administered (one sixth of all trials). For our main analyses, we compared agency ratings and temporal reproduction errors as a function of condition (*agency* vs. *baseline*). For each dependent variable (ratings, estimations), we computed a separate repeated measures ANOVA. In exploratory analyses, we investigated the main effect of inter-event interval (300, 500, 700ms) and the interaction of condition and inter-event interval with respect to both agency ratings and temporal reproduction errors.

For all reported results in the following section, we set $\alpha = .05$. For every analysis with more than two factor levels we corrected the degrees of freedom if Mauchly's test of sphericity

yielded $p < 0.05$. In these cases, we report uncorrected degrees of freedom with Greenhouse-Geisser corrected p -values and the correction parameter ϵ . We conducted Bonferroni-adjusted post-hoc pairwise comparisons for significant omnibus tests to examine effects in more detail.

Additional analyses were calculated for the valence and arousal ratings of the stimuli. For these dependent variables we computed two-tailed paired t-tests comparing both stimulus types (agency stimulus vs. random color change stimulus). All (non-exploratory) analyses were pre-registered unless explicitly stated otherwise in the Results section.

2.2. Results

2.3. Data Treatment

For our analyses, we excluded all trials with inter-event interval reproductions > 2.8 s (more than 4×700 ms, the longest possible inter-event interval; 0.05%). We then excluded trials with keypresses occurring earlier than 1.1 s after trial start (6.79%). Based on the remaining data, we computed z-values of temporal reproductions for each participant, inter-event interval, and condition (agency / baseline). We excluded all trials with temporal reproductions of $|z| > 3$ (0.24%). To ensure, that participants understood the temporal reproduction task and reproduced the inter-event intervals properly, we checked whether the duration of reproduced inter-event intervals increased in the same order as the true inter-event intervals (i.e., shorter inter-event interval estimations for 300 ms inter-event intervals compared to 500 ms inter-event intervals compared to 700 ms). None of the participants deviated from this pattern. In total, 7.07% of all trials were excluded. All trial exclusions have been pre-registered.

2.3.1. Temporal Reproduction Errors

We found a significant main effect of condition on temporal reproduction errors, $t(39) = 5.24$, $p = < .001$, $d_z = 0.83^2$, with stronger underestimations in the agency condition ($M = -$

² We report a t-value rather than an F-value to show the direction of the effect. While this represents a deviation from our pre-registered analysis, the p -values are equivalent, and the t -value is equivalent to \sqrt{F} .

0.149 s, SE = 0.025) than in the baseline condition (M = -0.079 s, SE = 0.026; see Fig. 2A). We also found a significant main effect of inter-event interval, $F(2, 78) = 248.07, p < .001, \eta_G^2 = .257$ ($\epsilon = .56$). Pairwise comparisons of each inter-event interval showed that the respective shorter inter-event interval was related to weaker underestimation errors (i.e., less negative or more positive values), compared to the longer inter-event interval(s) (all $t(39) \geq 14.37$, all $p < .001$; all $p_{adj.} < .001$; see Table 1). We found no significant interaction of condition and inter-event interval, $F(2, 78) = 1.62, p = .204, \eta_G^2 < .001$.

Table 1

Pairwise comparisons of Estimated Marginal Means of estimation errors between all inter-event intervals. Bonferroni-adjustments (p (adj.)) were computed with respect to 3 tests (p^*3)

Contrast	Difference [s]	SE	df	t	p	p (adj.)
300 - 500 ms	0.115	0.008	39	14.37	<.001	<.001
300 -700 ms	0.239	0.015	39	16.21	<.001	<.001
500 - 700 ms	0.124	0.008	39	15.49	<.001	<.001

2.3.2. Agency Ratings

We found a significant main effect of condition on explicit agency ratings, $t(39) = -12.24, p = < .001, d_z = -1.94$, with significantly higher ratings for trials in the agency condition (M = 74.39%, SE = 3.79%) compared to the baseline condition (M = 11.37%, SE = 2.79%; see Fig. 2C). We also found a significant main effect of inter-event interval on agency ratings, $F(2, 76) = 5.65, p = .005, \eta_G^2 = .004$. Agency ratings for the 300 ms inter-event interval were significantly higher than for the 700 ms inter-event interval (difference = 3.47%, $t(38) = 2.95, p = .005; p_{adj.} = .016$), while ratings for the 300 ms inter-event interval did not differ significantly from ratings for the 500 ms inter-event interval (difference = 1.72%, $t(38) = 1.77, p = .085; p_{adj.} = .256$). Ratings for the 500 ms inter-event interval did not differ significantly from ratings for the 700 ms inter-event interval (difference = 1.75%, $t(38) = 1.88, p = .067; p_{adj.} = .201$). Finally, we found a significant interaction of condition and interval, $F(2, 76) = 3.64, p = .040, \eta_G^2 = .003$ ($\epsilon = .83$). Post-hoc tests showed that the ratings in the agency condition for 300 ms inter-event intervals compared to 700 ms inter-event intervals

(difference = 6.08%, $t(38) = 2.73$, $p = .010$; $p_{adj.} = .057$) and the ratings for 500 ms inter-event intervals compared to 700 ms inter-event intervals (difference = 3.778, $t(38) = 2.43$, $p = .020$; $p_{adj.} = .118$) were each nominally higher but non-significant (after adjusting for multiple testing). The corresponding differences were not significant in the baseline condition (all other pairwise comparisons of inter-event intervals on each level of the factor condition yielded $t \leq 1.37$, $p \geq .179$, $p_{adj.} \geq 1.000$; for all comparisons see Table 2)

Table 2

Pairwise comparisons of Estimated Marginal Means of agency ratings regarding all inter-event intervals on each level of the factor condition. Bonferroni-adjustments (p (adj.)) were computed with respect to 6 tests ($p \times 6$)

Condition	Contrast	Difference [%]	SE	df	t	p	p (adj.)
baseline	300 - 500 ms	1.130	0.949	38	1.19	0.241	1.000
baseline	300 -700 ms	0.853	0.876	38	0.97	0.337	1.000
baseline	500 - 700 ms	-0.278	0.844	38	-0.33	0.744	1.000
agency	300 - 500 ms	2.301	1.682	38	1.37	0.179	1.000
agency	300 -700 ms	6.079	2.227	38	2.73	0.010	0.057
agency	500 - 700 ms	3.778	1.552	38	2.43	0.020	0.118

2.3.3. Stimulus Ratings

Participants nominally rated agency stimuli as more positive regarding their valence (mean = 71.21%, SE = 2.76%) than baseline stimuli (mean = 65.88%, SE = 2.83%) but this difference was not statistically significant ($t(39) = 1.81$, $p = .078$, $d = 0.287$). Furthermore, agency stimuli were nominally rated as being slightly more arousing (mean = 39.08%, SE = 3.69%) than baseline stimuli (mean = 37.58%, SE = 4.00%), but again, this difference was not significant ($t(39) = 0.39$, $p = .700$, $d = 0.061$).

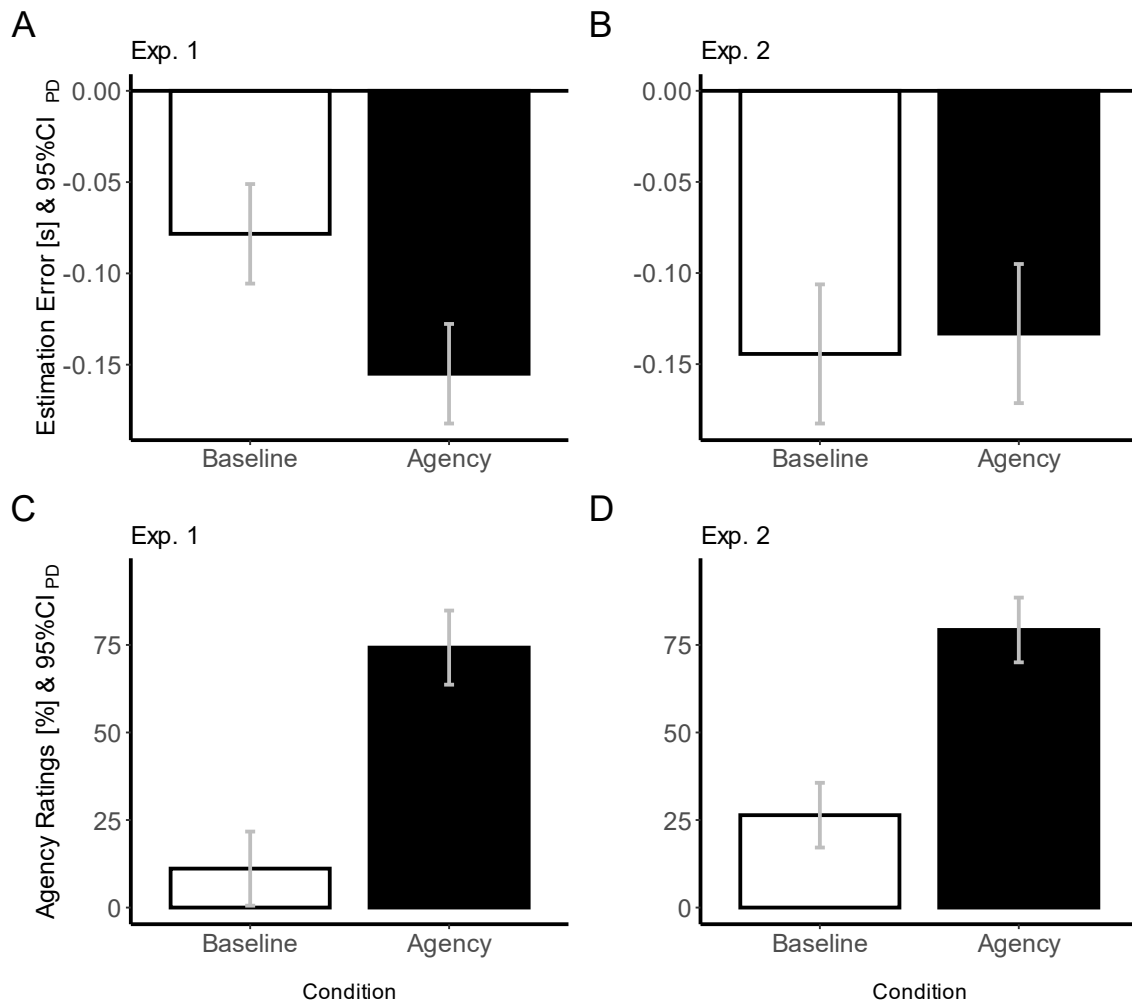


Fig. 2.

Results of Experiment 1 and 2. Mean reproduction errors (reproduced inter-event interval subtracted from true inter-event interval) as a function of condition (baseline vs. agency) with the observational baseline condition in Experiment 1 (A) and the non-intentional effect baseline in Experiment 2 (B). Agency ratings as a function of condition for Experiment 1 (C) and Experiment 2 (D). All error bars indicate the 95% confidence intervals of paired differences (95%CI_{PD}) for each comparison of baseline and agency condition (Pfister & Janczyk, 2013).

2.4. Discussion

Overall, we were able to replicate typical temporal binding results with our paradigm, which is conceptually identical to typical methodology in the field (see, e.g., Humphreys & Buehner, 2010) by using a traditional observational baseline and an interval reproduction task. Participants underestimated the inter-event intervals between a volitional action and a causal effect to a greater extent than the inter-event intervals between two external visual events. This systematic difference in time perception might be driven by temporal binding as

a correlate of sense of agency (Engbert et al., 2007). This interpretation is principally in line with the substantial differences observed in explicit agency ratings between agency and baseline trials. We thus conclude that our paradigm is equally well suited to examine temporal binding and sense of agency as previous setups.

3. Experiment 2

In Experiment 2, we implemented our new crucial *non-intentional effect baseline* to eliminate the procedural confounds involved in the *observational baseline* (see Introduction). In this new baseline, participants pressed a key corresponding to one of the two stimuli that constantly (and randomly) changed their color, which triggered a final (delayed) color change. Crucially, as the color changes already occurred before the action, participants should not have felt responsible for the color change following their action. In this way, we *selectively* manipulated the degree of sense of agency between both conditions, while the presence of actions and visual effects was kept constant. This paradigm also allowed us to include a third condition, in which we did not instruct participants which key they should press (e.g., a key corresponding to one of the two agency stimuli in the *agency* condition), but in which they were completely free to press any of the *four* arrow keys, corresponding either to one of the two agency stimuli or to one of the two baseline stimuli (*free choice* condition). By implementing this condition, we aimed to further increase the experienced sense of agency, in line with many other studies that have demonstrated an influence of freedom of choice on sense of agency (e.g., Barlas & Obhi, 2013; Schwarz et al., 2019). Furthermore, we were interested in whether participants might press specific keys more frequently that are associated with a subjectively controllable color change (agency condition) compared to keys that seem to have no impact on their environment. This might also be interpreted as an alternative implicit measure of agency, since having an influence on surrounding events has been considered generally preferable to having no influence at all (e.g., Seligman, 1972).

3.1. Methods

3.1.1. Participants

Even though we found large temporal binding effects in Experiment 1 ($d_z = 0.83$), we still conducted the same power analysis as described for Experiment 1 (anticipating more moderate effects of ($d_z = 0.5$), since we changed the task by including movements in both conditions, which might diminish temporal binding effects (see section 2.1.1). We thus recruited 40 participants again. Here, as was pre-registered, we excluded two participants who exhibited difficulties in understanding the time reproduction task (temporal reproductions averaged over each temporal inter-event interval did not follow the same order as the true inter-event intervals, see section 3.2.1) and recruited two more participants to eventually analyze the data of $n = 40$ participants (mean age: 25, SD: 8.23, range: 20-60, 80% female, 97.50% right-handed). All participants gave informed consent, reported normal or corrected-to-normal vision, and were compensated monetarily or with course credits.

3.1.2. Setup and Stimuli

Setup and Stimuli were the same as in Experiment 1 (see section 2.1.2) except where indicated in the following.

3.1.3. Task

As in Experiment 1, all stimuli were presented around the screen center. Two opposing stimuli constantly and randomly changed their color, whereas the other two stimuli retained their white color (cf. section 2.1.3). In this experiment, however, we implemented our new *non-intentional effect baseline* condition. In this condition, participants were asked to press a key corresponding to one of the two stimuli that constantly changed their color. After an inter-event interval of 300, 500, or 700 ms following their keypress, both stimuli changed their color for a last time. Crucially, this last color change appeared to occur in the same random manner as all the color changes of those stimuli before and thus *independent of the participant's keypress*. In the agency condition, participants pressed a key corresponding to one of the white stimuli and thereby changed its color to yellow in the same way as in Experiment 1 (cf. section 2.1.3; see Fig. 1). Note that in this design, participants pressed a key voluntarily and observed an effect after their action in both the agency *and the*

baseline condition. Their feeling of being in control over the observed effect and thereby the perceived intentionality, however, should substantially differ between both conditions. For exploratory analyses, we additionally included a third condition: in free-choice blocks, participants freely chose to press a key, either corresponding to one of the two white or to one of the two color-changing stimuli. Unlike in the other two conditions, they were not instructed which one of the stimulus pairs they should select. With this condition, we were able to investigate whether participants preferred to be able to control the color change of stimuli over not being in control of color changes (as would be evident by observing a preference for key presses related to the white stimuli of the agency condition). All three conditions (baseline, agency, free-choice) were presented block-wise and were repeated twice. Each condition alternated between blocks, and participants started with either the baseline condition or the agency condition. The free-choice block was always presented as third (and sixth) block. After each trial, participants reproduced the observed interval between keypress and color change by holding down the spacebar for the respective amount of time. After every sixth trial on average, the participants were asked for their explicit agency rating on a visual analogue scale, similar to Experiment 1.

Again, participants performed twelve training trials before the start of the first block. In each block, each delay interval (300/500/700 ms) was repeated twelve times, resulting in 36 trials in each block and 216 trials in total. Counterbalancing of stimulus position, stimulus identity, and starting condition was implemented in the same way as in Experiment 1.

After participants finished the sixth block, they rated all four stimuli regarding valence and arousal. In addition, they were also asked to report any temporal reproduction strategies.

3.1.4. Statistical Analyses

For our main analysis, we only included trials of the *agency* condition and the *non-intentional effect baseline*. All main and exploratory analyses regarding reproduction errors and agency ratings were calculated in the same manner as in Experiment 1 (see section

2.1.4). Furthermore, we conducted an exploratory analysis of response preference in the free choice condition by testing for the preference for agency stimuli over random stimuli with a two-sided t-test against 50% chance level (H0: no preference for either stimulus). As an additional exploratory analysis, we also included all trials of the *free choice* condition and conducted all above-mentioned analyses for agency ratings and temporal reproductions with the added factor freedom of choice (free-choice vs. instructed action) as additional exploratory analyses. Note that trials from the *agency* condition and the *non-intentional effect baseline* were included in the factor level *instructed action*. All (non-exploratory) analyses were pre-registered unless explicitly stated otherwise in the Results section.

3.2. Results

3.2.1. Data Treatment

We treated our data in the same way as in Experiment 1 (cf. section 2.3), with some minor differences: we excluded all trials in which a wrong key was pressed (agency key in the baseline condition and vice versa). All trial exclusions can be seen in Table 3. All exclusions have been pre-registered.

Table 3

All exclusions of data in Experiment 2.

Data	Trials (n)	Excluded (n)	Excluded (%)
Raw Data	8640	-	-
Long reproductions	8640	0	0.00
Early press	8098	542	6.27
Wrong press	8083	15	0.19
z-filter (reproductions)	8083	37	0.46
Total	8046	594	6.88

3.2.2. Temporal Reproduction Errors

The following analyses only include trials of instructed action conditions. In this experiment we no longer found a significant main effect of condition, In this experiment we

no longer found a significant main effect of condition, $t(39) = -0.69$, $p = .496$, $d_z = -0.11$ (agency condition: $M = -0.131$ s, $SE = 0.030$; baseline condition: $M = -0.143$ s, $SE = 0.023$; see Fig. 2C)³. This non-rejection of our H_0 is certainly not sufficient to proclaim equivalence between conditions (e.g., Altman & Bland, 1995; Lakens et al., 2020). To investigate whether temporal reproduction errors might indeed be considered roughly equivalent between the agency and the baseline condition, we computed an explorative Bayes Factor using JASP (JASP Team, 2022)⁴. According to the review of Tanaka et al. (2019), temporal binding effects of previous studies using a temporal interval estimation paradigm had an estimated d of 0.300 and a 95% CI of [0.109, 0.408]. The prior is described by a normal distribution with mean = 0.25 and SD = 0.07⁵. This corresponds to a probability of 96.79% that the effect size lies between 0.1 and 0.4. We then tested for $H_1: \mu_{agency} < \mu_{baseline}$. The resulting Bayes Factor indicated an absence of effect for condition, $BF_{01} = 7.388$ ($BF_{10} = 0.135$). Note that this analysis was planned and performed only after the occurrence of the non-significant effect of condition and was not preregistered.

To ensure that there was no effect of condition on intentional binding even in the very clear absence of any reasonable sense of agency (as evident in very low agency ratings), we re-examined interval reproduction errors separately for the 25% quartile of subjects with the lowest agency ratings (mean rating for agency condition = 89.85%, $SE = 1.64\%$, mean rating for baseline condition = 4.65%, $SE = 0.71\%$) in an exploratory analysis. As a result, we still found no significant difference between reproduction errors (agency condition: mean = -0.109 s, $SE = 0.104$; baseline condition: mean = -0.120, $SE = 0.090$), $t(9) = -0.48$, $p = .641$, $d = -0.152$, and a Bayes analysis with the same priors as above indicated anecdotal

³ We report a t -value rather than an F -value to show the direction of the effect. While this represents a deviation from our pre-registered analysis, the p -values are equivalent, and the t -value is equivalent to \sqrt{F} .

⁴ Since we did not anticipate these effects of temporal reproductions, this particular analysis deviates from our pre-registration.

⁵ This prior was kindly suggested to us by one of the reviewers.

evidence (Lee & Wagenmakers, 2014) for an absence of any effect, $BF_{01} = 1.977$ ($BF_{10} = 0.506$).

As in the previous experiment, we found a significant main effect of inter-event interval on temporal reproduction errors, $F(2, 78) = 160.29$, $p < .001$, $\eta^2 = .245$ ($\epsilon = .61$). Pairwise comparisons of reproduction errors for each inter-event interval revealed the same pattern as in Experiment 1: the respective shorter inter-event interval was associated with a higher temporal reproduction error (indicating a weaker amount of underestimation) than the longer inter-event interval(s) (all $t(39) \geq 10.23$, all $p < .001$; all $p_{adj.} < .001$; see Table 4). The interaction of condition and inter-event interval was also significant, $F(2, 78) = 10.06$, $p < .001$, $\eta^2 = .005$. Pairwise comparisons between temporal reproduction errors for the two conditions on each level of inter-event interval revealed a significant difference in the 300 ms inter-event interval level between temporal reproduction errors during the baseline condition ($M = -0.036$ s; $SE = 0.023$) and the agency condition ($M = 0.009$ s; $SE = 0.028$), $t(39) = -2.69$, $p = .010$, $p_{adj.} = .031$ (for all pairwise comparisons see Table 5). Note, however, that this significant effect points into the *opposite* direction of the expected effect, and thus runs counter the hypothesis of a stronger underestimation in the agency (vs. baseline) condition.

For exploratory analyses, we additionally included all *free choice* trials and added the two-level factor freedom of choice (free choice/instructed action) to all analyses reported above. One participant produced at least one empty cell and was thus excluded for this particular analysis. We will only report the effects regarding the factor freedom of choice here. Specifically, we found no main effect of freedom of choice, $F(1, 38) = 1.23$, $p = .275$, $\eta^2 = .001$, no interaction of freedom of choice and condition, $F(1, 38) = 0.09$, $p = .768$, $\eta^2 < .001$, and no interaction of freedom of choice and inter-event interval, $F(2, 76) = 0.40$, $p = .673$, $\eta^2 < .001$. However, we found a significant three-way-interaction of freedom of choice, condition, and inter-event interval, $F(2, 76) = 4.01$, $p = .022$, $\eta^2 = .001$. Pairwise comparisons of temporal reproduction errors between both conditions in each inter-event interval level (separated for choice) showed that the previously found significant difference

between agency and baseline stimuli in the 300 ms level was neither significant in instructed action trials (difference = -0.045 s, SE = 0.017, $t(38) = -2.62$, $p = .013$, $p_{adj.} = .075$), nor in free choice trials (difference = -0.018 s, SE = 0.016, $t(38) = -1.09$, $p = .281$, $p_{adj.} = 1.000$). For all pairwise comparisons of the three-way-interaction, see Table 6. In summary, freedom of choice had no substantial influence on temporal reproduction errors.

Table 4.

Pairwise comparisons of Estimated Marginal Means of temporal reproduction errors between all three inter-event intervals. Bonferroni-adjustments (p (adj.)) were computed with respect to 3 tests (p^*3).

Contrast	Difference [s]	SE	df	t	p	p (adj.)
300 - 500 ms	0.122	0.012	39	10.23	<.001	<.001
300 -700 ms	0.249	0.019	39	13.40	<.001	<.001
500 - 700 ms	0.127	0.010	39	13.20	<.001	<.001

Table 5.

Pairwise comparisons of Estimated Marginal Means of temporal reproduction errors between baseline and agency conditions on each level of inter-event interval. Bonferroni-adjustments (p (adj.)) were computed with respect to 3 tests (p^*3)

interval	Contrast	Difference [s]	SE	df	t	p	p (adj.)
300 - 500 ms	baseline - agency	-0.045	0.017	39	-2.69	0.010	0.031
300 -700 ms	baseline - agency	-0.005	0.020	39	-0.26	0.793	1.000
500 - 700 ms	baseline - agency	0.014	0.023	39	0.61	0.547	1.000

Table 6.

Pairwise comparisons of Estimated Marginal Means of temporal reproduction errors between baseline and agency conditions on each level of inter-event interval on each level of freedom of choice (FoC; IA : instructed action, FC: free-choice). Bonferroni-adjustments (p (adj.)) were computed with respect to 6 tests (p^*6).

interval	FoC	Contrast	Difference [s]	SE	df	t	p	p (adj.)
300 - 500 ms	IA	baseline - agency	-0.045	0.017	38	-2.62	0.013	0.075
300 -700 ms	IA	baseline - agency	-0.006	0.021	38	-0.28	0.780	1.000
500 - 700 ms	IA	baseline - agency	0.016	0.024	38	0.68	0.501	1.000
300 - 500 ms	FC	baseline - agency	-0.018	0.016	38	-1.09	0.281	1.000
300 -700 ms	FC	baseline - agency	0.008	0.016	38	0.50	0.618	1.000
500 - 700 ms	FC	baseline - agency	-0.010	0.015	38	-0.66	0.510	1.000

3.2.3. Agency Ratings

The following analyses only include trials from instructed action conditions. Again, we found a significant main effect of condition on agency ratings, $t(39) = -11.54$, $p < .001$, $d_z = -1.82$, with much higher ratings in agency trials ($M = 79.04\%$, $SE = 2.20\%$) than in baseline trials ($M = 26.39\%$, $SE = 3.18\%$; see Fig. 2D). We found no significant main effect of inter-event interval on agency ratings, $F(2, 78) = 1.16$, $p = .311$, $\eta^2 = .002$ ($\epsilon = .81$), and no significant interaction of condition and inter-event interval, $F(2, 78) = 0.23$, $p = .734$, $\eta^2 < .001$ ($\epsilon = .77$).

For additional exploratory analyses we added all free choice trials to our data and recalculated all above mentioned repeated measure ANOVAs with the additional factor freedom of choice (free choice/instructed action). We will only report effects of this factor here. We excluded 16 participants from this particular analysis as they produced at least one empty cell. We found a significant main effect of the factor freedom of choice, $F(1, 23) = 5.20$, $p = .032$, $\eta^2 = .008$, with higher ratings for free choice trials ($M = 56.05\%$, $SE = 2.54\%$) than for instructed action trials ($M = 52.40\%$, $SE = 1.89\%$). We also found a significant interaction of freedom of choice and condition, $F(1, 23) = 16.93$, $p < .001$, $\eta^2 = .050$. Pairwise comparisons of ratings between the levels of freedom of choice on each level of condition revealed significantly lower ratings for baseline trials with instructed actions compared to baseline trials with free choice (difference = -13.07% , $t(23) = -4.73$, $p < .001$; $p_{adj.} < .001$). Ratings in agency trials were nominally higher for instructed actions than for free choice conditions, although this difference was not statistically significant (difference = 5.78% , $t(23) = 2.05$, $p = .052$; $p_{adj.} = .105$). There was no significant interaction of freedom of choice and inter-event interval ($F(2, 46) = 0.19$, $p = .831$, $\eta^2 < .001$), and no significant three-way interaction of freedom of choice, condition, and inter-event interval ($F(2, 46) = 2.13$, $p = .131$, $\eta^2 = .002$).

3.2.4. Stimulus Preferences and Stimulus Ratings

Participants chose agency stimuli over baseline stimuli in 54.47% (SE = 1.81%) of the free choice trials. This preference was significantly higher than 50% ($t(39) = 2.48$, $p = .018$, $d = 0.391$). Agency stimuli were rated more positively regarding valence (mean = 73.92%, SE = 2.76%) than baseline stimuli (mean = 63.08%, SE = 4.21%; $t(39) = 2.13$, $p = .039$, $d = 0.337$), and as significantly less arousing (mean = 40.34%, SE = 4.26%) than baseline stimuli (mean = 57.63%, SE = 4.08%; $t(39) = -2.79$, $p = .008$, $d = -0.442$).

3.3. Discussion

Surprisingly, and in stark contrast to Experiment 1, we were not able to find any differences in reproduction errors between the agency and the (conceptually better, see Introduction) baseline condition in the second experiment. However, similar to the first experiment, the explicit agency ratings differed substantially between the agency condition and the baseline condition. The latter observation clearly confirms that the manipulation of (the subjective) sense of agency was highly successful in this experiment, and that typical differences in time estimation or temporal reproduction errors were to be expected in this experiment (in line with previous research regarding sense of agency (Moore & Obhi, 2012; Tanaka et al., 2019), *if intentionality was really linked to the time estimation effect*). The fact that we could not find corresponding differences in temporal reproduction in the present paradigm therefore strongly indicates that previous temporal binding effects from interval reproduction or estimation tasks were merely due to a lack of an isolated manipulation of sense of agency (or intentionality as a link between action and effect) between agency and baseline conditions, and thus driven by procedural confounds. In contrast to previous studies, we found no enhancement of sense of agency by free choice in terms of agency ratings and temporal binding (e.g., Barlas & Obhi, 2013).

4. General Discussion

Intentional binding is considered to be an implicit measure of sense of agency (Moore & Obhi, 2012). Initially, intentional binding was regarded as the result of the interplay

between intentions, voluntary actions, and predictive motor mechanisms (Frith et al., 2000; Haggard & Tsakiris, 2009). Here, we argued that typical research in the field has, up to this point, not selectively manipulated sense of agency when comparing performance in agency conditions with baseline conditions. Instead, several other confound variables were assumed to potentially play a role in addition. For that reason, we developed a new paradigm that allowed us to better control for such potential confounds. Specifically, our new design (as implemented in Experiment 2) involved an action demand and a subsequent perceptual effect in both the agency and the baseline condition, while only sense of agency was selectively manipulated.

In a first step (Experiment 1), we replicated typical temporal binding findings using a interval reproduction task and a standard *observational baseline* (i.e., a baseline lacking any action demands) within our paradigm to show that the specific methodology employed here is well suited to produce the typical temporal binding effects that have repeatedly been demonstrated in the literature: As expected, temporal inter-event reproductions were significantly lower in operant (*agency*) trials compared to the passive *observational* trials. We then implemented our new crucial *non-intentional effect baseline* in Experiment 2 to eliminate the confound between the sense of agency manipulation with the presence/absence of an action demand in the agency/baseline condition, respectively. We thereby compared one condition in which, subjectively, actions were followed by effects that were causally linked to these actions from the viewpoint of the participants (*agency condition*) with another condition in which the same actions were followed by the same subsequent effects, but without creating the impression that the actions were causally linked to the ensuing effects (*non-intentional effect baseline*), thereby manipulating *only* sense of agency between conditions while keeping all other parameters (e.g., stimulus characteristics, bodily movements, number of events) constant. Importantly, the success of the intended manipulation is clearly reflected in the explicit agency ratings that substantially differed between conditions. Strikingly, however, despite this marked difference in sense of agency,

there were no meaningful differences in temporal inter-event interval reproductions between our operant *agency condition* and our *non-intentional effect baseline* condition (not even for participants with most extreme differences in explicit agency ratings). Instead, we even found evidence for effects pointing into the *opposite* direction (i.e., relative to the direction predicted by intentional binding theories) for short inter-event intervals, where a stronger underestimation of 300 ms inter-event intervals was observed in the *non-intentional effect baseline* (vs. the agency condition). These results strongly suggest that intentional binding effects in many past studies using the interval estimation (reproduction) procedure were, at least to some extent, not due to differences in experienced sense of agency but rather due to procedural confounds. Note that our results not only contradict the classic explanation of temporal binding effects as resulting from the participants' belief that their *voluntary actions* cause the ensuing perceptual event (Frith et al., 2000; Haggard & Tsakiris, 2009). Instead, the results are also incompatible with a broader explanation (causality account) suggesting that any perceived causality between two events (i.e., even when the first event is *not* an action) should cause temporal binding (Hoerl et al., 2020). In the following section, we discuss several potential explanations for these results and show that some other recent studies are also quite compatible with our present conclusions.

One alternative explanation for temporal binding in the context of research on sense of agency refers to a multisensory integration phenomenon. According to this account, different sensory signals are integrated when they are related to a single multimodal event (Alais & Burr, 2004). This approach explains temporal binding between a voluntary action (which is also associated with accompanying sensory, e.g. proprioceptive, signals) and a subsequent effect in terms of an integration of these two events as they belong to a single higher-level event due to their causal link. Such findings were reported in studies using interval estimation (e.g., Suzuki et al., 2019) and Libet clock procedures (e.g., Kirsch et al., 2019; Klaffehn et al., 2021; Wolpe et al., 2013). Note that this explanatory approach extends the causality account (see above), since causality is assumed to establish one possible

reason for event integration. However, perceived causality is not a necessary condition for two sensory inputs being represented as a single multimodal event. Taking this approach into account, temporal binding might have occurred in our *agency* condition as well as in our *non-intentional effect* baseline condition. The keypress and the (temporal) subsequent color change in both conditions might have been integrated into one *trial event*, even without any subjective causal link but instead merely due the fact that both events belong to a single trial of the experiment. In this context, the certainty of temporal perception (Klaffehn et al., 2021) as well as the temporal prediction (Kirsch et al., 2019) of each event may play an important role for the extent of temporal binding effects. In particular, relatively more uncertain and less predictable events should be perceived as shifting towards more certain and more predictable events more strongly. One might argue that actions in our experiments were temporally perceived with higher precision than visual events (appearance of stimuli or color changes), as they were controlled by the participants and are accompanied by various sensory effects (e.g., proprioceptive, tactile, auditory byproducts of key presses). Furthermore, actions were always temporally predictable as subjects were free to choose when to press the key. On the other hand, color changes and stimulus appearances might have been temporally less precise, as they only occurred in the visual domain and were not temporally predictable. This might have caused a relatively stronger ‘pull effect’ for actions (compared to visual events), and this might thus explain the stronger temporal effect binding in each condition that included action events (*agency*, *non-intentional effect baseline*) compared to our condition with only visual events (*observational baseline*). This explanation casts substantial doubts on intentional binding findings of studies using interval estimations with a classical observational baseline as the occurrence of actions is not kept constant in these experimental designs (see Introduction).

Another potential reason for subjective temporal compression between events might be attentional orienting. This approach postulates that actions as well as external stimuli draw attention, and that attention has the potential to shift our spatial (Sheliga et al., 1995)

and temporal (e.g., Zakay & Block, 1995) perception. In a recent study by Schwarz and Weller (2022), the authors modified a temporal binding paradigm that involved a Libet clock with classic *single event baselines*. Specifically, they implemented a (task irrelevant) color change of the handle of the Libet clock as an attention-drawing (but not causally related) event into both the *single action condition* and the *single effect condition* and manipulated the time point of this event. They were able to predictably evoke temporal binding towards this color-changing event, which ultimately even resulted in reversed binding effects when presenting the color change before the keypress or after the effect (an auditory tone) presentation. Even though results from this study are not directly comparable to the results of the present study due to the use of a Libet clock procedure compared to interval reproductions (Siebertz & Jansen, 2022; for detailed discussion, see below), one might still assume that actions and visual effects in our paradigm might have also served to draw different ‘amounts’ of attention and thus might have influenced time perception to different extents. In our experiments, actions were self-paced and chosen (more or less) freely. Thus, attention allocation to actions might have been driven more endogenously while attention allocation towards (external) visual effects might have been driven (at least partly) exogenously (Ruz & Lupiáñez, 2002). Consequently, when comparing the *observational baseline* with other conditions in which an action occurred, differences in temporal estimations or reproductions might be attributed to differences in attentional orienting rather than to differences in sense of agency. When comparing the *agency condition* and the *non-intentional effect baseline* in our present study, one would expect similar degrees of attentional shifts to each action and to each event and thus similar degrees of inter-event reproduction errors, as was observed in our data.

Another possible attentional shift could have occurred towards the irrelevant color changes of the *unintended-effect baseline* stimuli in Experiment 2. These color-changing stimuli were irrelevant in Experiment 1 in both conditions and did not need to be monitored. In Experiment 2, however, subjects had to focus specifically on this stream of color changes.

Thereby, these irrelevant color changes could also have attracted attention and thus affected temporal reproduction. However, it is important to note that these irrelevant color changes occurred exclusively prior to the participants' action. Thus, there was no irrelevant color change between the action and the relevant effect and no color change afterwards (the relevant effect was always the color change immediately after the action, and this was always the last color change). Thus, an attentional shift towards an irrelevant color change should in fact have led to systematically *longer* interval reproductions (shifting the subjective start of the inter-event interval forward in time), since the irrelevant color change occurred exclusively before the action (cf. Schwarz & Weller, 2022). However, since we found *underestimations* of the interval (comparable to classical intentional binding effects), an attentional shift towards the irrelevant color changes seems unlikely.

The systematic biases of temporal perception related to temporal attention found in the present study (i.e., the temporal compression phenomena for intervals between two linked events) might ultimately serve a purpose of perceptual 'contrast enhancement', thereby coding two events as belonging together by means of a temporal perceptual shift towards each other (see Introduction for a brief discussion of the underlying cognitive machinery based on standard pacemaker/accumulator models of time perception). This effect might additionally support event segmentation processes to enhance the understanding of our dynamic and complex environment (Kurby & Zacks, 2008; Zacks & Swallow, 2007). Again, this potential (functional) explanation, which should be tested more explicitly in the future, casts serious doubts on intentional binding findings in previous studies, in which such attentional sources of influence were not controlled for (see Introduction).

Finally, another explanation for the absence of any differences in interval reproductions between the *agency condition* and the *non-intentional effect baseline* could be that participants might have believed that they could control the (final) color change in the *non-intentional effect baseline* as well. According to this account, one might argue that they

experienced sense of agency in both conditions (*agency* and *non-intentional effect baseline*), and that they also experienced intentional binding in both conditions. As a matter of fact, participants indeed *did* trigger the final color change by pressing the key in the *non-intentional effect baseline*. However, this de facto causal connection should not have been recognizable by the participants due to a) the subsequent unpredictable (random) inter-event interval of 300-700 ms, and b) the random color changes prior to their action. In line with the latter reasoning, the explicit agency ratings also provide clear evidence that participants actually did not experience much responsibility for the (final) color change in the *non-intentional effect baseline* compared to the color change in the *agency condition*. However, while agency ratings in the *unintended effect baseline* condition are substantially and significantly lower than ratings in the *agency condition*, they are nominally higher compared to the *observational* condition. This might indicate that there might be some amount of residual agency in this particular baseline. In an exploratory analysis, we therefore re-analyzed reproduction errors for participants with the lowest explicit agency ratings. Still, we found no differences in intentional binding, indicating that even if there was some residual sense of agency in the *unintended effect baseline* in the total sample, this should have had no substantial effect on intentional binding. Moreover, the absence of sense of agency in the *unintended effect baseline* is further supported by the clear preference for pressing keys associated with color changes of the *agency* stimuli over the stimuli from the *baseline* condition in the *free-choice condition* in Experiment 2. This behavior might be due to a preference of being in control (vs. not in control) of the surroundings (e.g., Seligman, 1972), which indicates that subjects *did* show different levels of sense of agency across the different types of color changes. In line with this, the stimuli of the *agency condition* (vs. stimuli of the *non-intentional effect baseline*) were rated as being significantly more positive in terms of their emotional valence after the experiment. For these reasons, we are confident that our paradigm successfully manipulated the degree of subjective sense of agency between conditions.

One might argue that the data of a single experiment (Experiment 2) does not suffice to ultimately challenge a long-standing tradition of interpreting temporal binding effects in the sense of agency literature. However, our present critique of interpreting temporal binding effects in terms of 'intentional binding' fits in well with many other recent studies that cast doubts on any direct and exclusive connection between temporal compression phenomena and sense of agency: For example, temporal binding effects have recently been observed also for effects that were caused by involuntary (unintentional) actions (when controlling for temporal predictability of actions and effects; Kirsch et al., 2019), and even non-actions (Weller et al., 2020), thereby challenging the idea that intentional actions represent a prerequisite for finding temporal binding effects. In addition, temporal binding was found for unintended effects (Ruess et al., 2020), indicating that effect intentions are not necessary either. Furthermore, temporal binding has also been reported for merely *observed* actions and effects (Poonian & Cunnington, 2013), a finding that should not occur when the participants' own intentions should play a crucial role. Finally, there are several reports of non-correlations between explicit agency ratings and temporal binding effects (e.g., Dewey & Knoblich, 2014; Schwarz et al., 2019). Taken together, the present results actually represent another (albeit central) piece in a grander puzzle that reveals a lack of actual empirical support for the claim that temporal binding effects in many classic sense of agency studies are solely driven by the strength (or presence) of intentionality or sense of agency.

Our present findings may have strong implications for past research that was based on interval estimation (or reproduction) measures to assess temporal binding effects as an indication of sense of agency. For example, the seminal review paper by Tanaka et al. (2019) listed 10 studies (Berberian et al., 2012; Braun et al., 2014; Cravo et al., 2009, 2013; Engbert et al., 2008; Humphreys & Buehner, 2009; Imaizumi & Tanno, 2019; Morioka et al., 2018; Wen et al., 2015; Zhao et al., 2013; as published between January 2002 and April 2019 in the Pubmed, PsycINFO, and Scopus databases) that reported 106 binding results with the interval estimation procedure and observational baseline in total (Tanaka et al., 2019,

supplementary material <https://osf.io/8pnzg>). However, this number should rather be interpreted as a lower bound of possibly relevant previous results that may be biased based on confounds, as studies using interval reproduction procedures (as opposed to interval estimation procedures) (e.g., Dewey & Knoblich, 2014; Humphreys & Buehner, 2010; Poonian & Cunnington, 2013) or studies published later than April 2019 (e.g., Hayashida et al., 2020; Jenkins & Obhi, 2021; Lorimer et al., 2020; Vastano et al., 2020; Vogel et al., 2021) were not included in this review. In conclusion, any study that used an interval estimation procedure and included an observational baseline only should be carefully re-assessed regarding the validity of its conclusions with respect to intentional binding.

Although our results cast substantial doubts on intentional binding results from studies using interval estimation (or reproduction) procedures, these findings cannot directly be generalized to intentional binding assessed with a Libet clock procedure. Some studies that measure intentional binding using the Libet Clock paradigm showed stronger temporal binding in the context of sense of agency manipulations compared to, for example, temporal binding in the context of two causal events (see above). For instance, Desantis et al. (2011) showed that participants experienced stronger intentional binding between a key-press and a subsequent tone when they believed that they produced the tone themselves as compared to another person, when in fact the tone was always produced by another person. Another study by Lush et al. (2017) showed stronger intentional binding for voluntary actions compared to passive and (posthypnotic) involuntary actions by participants, highlighting the significance of motor intentions for (stronger) temporal binding. Both of these studies therefore suggest that some portion of these temporal binding effects may indeed be driven by sense of agency proper. Moreover, there are studies (also using the Libet clock procedure) that show an influence of reduced action-outcome contingency (by reducing the occurrence of action-triggered events) on intentional binding (e.g., Beck et al., 2017) and explicit agency ratings (e.g., Moore et al., 2009). One could argue that our baseline condition in Experiment 2 might effectively have led to less *experienced* (subjective) contingency, not by reducing but by

increasing the occurrence of events *independent* of the action (while the objective contingency was matched with the agency condition). While we found an effect on agency ratings here, we did not see any evidence for intentional binding. Further research might be necessary to investigate whether a classic contingency manipulation (through the reduction of action-triggered effect occurrences, as in previous Libet clock studies) would also lead to reduced sense of agency (especially regarding explicit agency ratings) in our paradigm. Notably, previous studies that reported effects regarding implicit measures used the Libet clock procedure to assess temporal binding, not interval estimations or reproductions. Siebertz and Jansen (2022) provided evidence for the idea that those two procedures could be unrelated to some extent and tap into different underlying mechanisms. Based on our findings and the procedural confounds outlined so far, it could be argued that the Libet clock paradigm might generally be better suited to assess implicit intentional binding, as also suggested by stronger and more reliable effects compared to intentional binding results based on interval estimation procedures (Tanaka et al., 2019).

That said, there are potential confounds in some Libet clock studies as well. As described in the Introduction, Libet clock procedures mainly use *single event baselines* (cf., Haggard et al., 2002) consisting of only an action (without subsequent effect) or only an effect (without a preceding action) to contrast results with time estimations of actions or effects in the *operant* condition. As in interval estimation studies with *observational baselines*, those conditions thus not only differ in the subjective sense of agency but also in the presence and absence of an action (yielding the same potential issues as outlined in the Introduction). Another potentially serious confound in the *single-event baseline* is the number of events experienced in the experimental vs. control condition. While the experimental condition involved both the action and the following effect, the control condition either involves only the action or the effect. Thus, participants systematically perceive more events in the experimental condition than in the baseline condition. The number of events (i.e., the extent of segmentation) occurring during an interval, however, is well known to affect temporal

perception, even though corresponding studies appear to be inconclusive under which conditions more events lead to shortened (e.g., Bangert et al., 2019, 2020; Liverence & Scholl, 2012) or extended (e.g., Bangert et al., 2020; Faber & Gennari, 2017) time perception. Apart from the mere number of events, overall task demands differ between experimental and baseline conditions, too, as in the experimental condition participants perform an action, observe an effect, and estimate time, while in the baseline conditions they perform one task less (i.e., either movement or effect observation). This might additionally promote temporal distortions due to different levels of attention (Block & Gruber, 2014; Brown, 1985; Schwarz & Weller, 2022; Zakay & Block, 1995) and cognitive load (for meta-analysis, see Block et al., 2010). Further research is necessary to investigate whether these confounds have a comparable impact on results of Libet Clock studies as those addressed in the present (interval estimation) study.

In sum, we were able to show that previously observed temporal binding effects as observed in classic sense of agency studies may have occurred not as a result of different degrees of intention (or sense of agency) between conditions, but rather as a result of procedural confounds (such as the presence/absence of action demands across conditions). After controlling for these confounds in the experimental setup of Experiment 2, we no longer found any distinct link between sense of agency and temporal binding. Probably, such temporal compression phenomena are instead more closely related to temporal 'contrast enhancement' (grouping) purposes for any (perceptual and/or motor) events that are perceived as meaningfully belonging together (e.g., as representing parts of a common trial episode).

5. References

- Alais, D., & Burr, D. (2004). The ventriloquist effect results from near-optimal bimodal integration. *Current Biology: CB*, *14*(3), 257–262.
<https://doi.org/10.1016/j.cub.2004.01.029>
- Altman, D. G., & Bland, J. M. (1995). Absence of evidence is not evidence of absence. *BMJ (Clinical Research Ed.)*, *311*(7003), 485. <https://doi.org/10.1136/bmj.311.7003.485>
- Bangert, A. S., Kurby, C. A., Hughes, A. S., & Carrasco, O. (2020). Crossing event boundaries changes prospective perceptions of temporal length and proximity. *Attention, Perception, & Psychophysics*, *82*(3), 1459–1472.
<https://doi.org/10.3758/s13414-019-01829-x>
- Bangert, A. S., Kurby, C. A., & Zacks, J. M. (2019). The influence of everyday events on prospective timing “in the moment.” *Psychonomic Bulletin & Review*, *26*(2), 677–684. <https://doi.org/10.3758/s13423-018-1526-6>
- Barlas, Z., & Obhi, S. S. (2013). Freedom, choice, and the sense of agency. *Frontiers in Human Neuroscience*, *7*(AUG), 514. <https://doi.org/10.3389/fnhum.2013.00514>
- Beck, B., Di Costa, S., & Haggard, P. (2017). Having control over the external world increases the implicit sense of agency. *Cognition*, *162*, 54–60.
<https://doi.org/10.1016/j.cognition.2017.02.002>
- Berberian, B., Sarrazin, J.-C., Le Blaye, P., & Haggard, P. (2012). Automation technology and sense of control: A window on human agency. *PloS One*, *7*(3), e34075.
<https://doi.org/10.1371/journal.pone.0034075>
- Block, R. A., & Gruber, R. P. (2014). Time perception, attention, and memory: A selective review. *Acta Psychologica*, *149*, 129–133.
<https://doi.org/10.1016/j.actpsy.2013.11.003>

- Block, R. A., Hancock, P. A., & Zakay, D. (2010). How cognitive load affects duration judgments: A meta-analytic review. *Acta Psychologica, 134*(3), 330–343.
<https://doi.org/10.1016/j.actpsy.2010.03.006>
- Braun, N., Thorne, J. D., Hildebrandt, H., & Debener, S. (2014). Interplay of Agency and Ownership: The Intentional Binding and Rubber Hand Illusion Paradigm Combined. *PLoS ONE, 9*(11), e111967. <https://doi.org/10.1371/journal.pone.0111967>
- Brown, S. W. (1985). Time perception and attention: The effects of prospective versus retrospective paradigms and task demands on perceived duration. *Perception & Psychophysics, 38*(2), 115–124. <https://doi.org/10.3758/BF03198848>
- Champely, S. (2020). *pwr: Basic Functions for Power Analysis*. <https://cran.r-project.org/package=pwr>
- Core R Team. (2021). A Language and Environment for Statistical Computing. In *R Foundation for Statistical Computing* (Vol. 2, p. <https://www.R-project.org>). R Foundation for Statistical Computing. <http://www.r-project.org>
- Cravo, A. M., Claessens, P. M. E., & Baldo, M. V. C. (2009). Voluntary action and causality in temporal binding. *Experimental Brain Research, 199*(1), 95–99.
<https://doi.org/10.1007/s00221-009-1969-0>
- Cravo, A. M., Haddad, H., Claessens, P. M. E., & Baldo, M. V. C. (2013). Bias and learning in temporal binding: Intervals between actions and outcomes are compressed by prior bias. *Consciousness and Cognition, 22*(4), 1174–1180.
<https://doi.org/10.1016/j.concog.2013.08.001>
- Damsma, A., Schlichting, N., van Rijn, H., & Roseboom, W. (2021). Estimating Time: Comparing the Accuracy of Estimation Methods for Interval Timing. *Collabra: Psychology, 7*(1), 21422. <https://doi.org/10.1525/collabra.21422>

- De Kock, R., Zhou, W., Joiner, W. M., & Wiener, M. (2021). Slowing the body slows down time perception. *ELife*, *10*, e63607. <https://doi.org/10.7554/eLife.63607>
- Desantis, A., Hughes, G., & Waszak, F. (2012). Intentional binding is driven by the mere presence of an action and not by motor prediction. *PloS One*, *7*(1), e29557. <https://doi.org/10.1371/journal.pone.0029557>
- Desantis, A., Roussel, C., & Waszak, F. (2011). On the influence of causal beliefs on the feeling of agency. *Consciousness and Cognition*, *20*(4), 1211–1220. <https://doi.org/10.1016/j.concog.2011.02.012>
- Dewey, J. A., & Knoblich, G. (2014). Do Implicit and Explicit Measures of the Sense of Agency Measure the Same Thing? *PLOS ONE*, *9*(10), e110118. <https://doi.org/10.1371/journal.pone.0110118>
- Engbert, K., Wohlschläger, A., & Haggard, P. (2008). Who is causing what? The sense of agency is relational and efferent-triggered. *Cognition*, *107*(2), 693–704. <https://doi.org/10.1016/j.cognition.2007.07.021>
- Engbert, K., Wohlschläger, A., Thomas, R., & Haggard, P. (2007). *Agency, Subjective Time, and Other Minds*. <https://doi.org/10.1037/0096-1523.33.6.1261>
- Faber, M., & Gennari, S. P. (2017). Effects of learned episodic event structure on prospective duration judgments. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *43*(8), 1203–1214. <https://doi.org/10.1037/xlm0000378>
- Fereday, R., & Buehner, M. J. (2017). Temporal binding and internal clocks: No evidence for general pacemaker slowing. *Journal of Experimental Psychology: Human Perception and Performance*, *43*(5), 971–985. <https://doi.org/10.1037/xhp0000370>
- Frith, C. D., Blakemore, S. J., & Wolpert, D. M. (2000). Explaining the symptoms of schizophrenia: Abnormalities in the awareness of action. *Brain Research Reviews*, *31*(2–3), 357–363. [https://doi.org/10.1016/S0165-0173\(99\)00052-1](https://doi.org/10.1016/S0165-0173(99)00052-1)

- Gallagher, S. (2000). Philosophical conceptions of the self: Implications for cognitive science. *Trends in Cognitive Sciences*, 4(1), 14–21. [https://doi.org/10.1016/S1364-6613\(99\)01417-5](https://doi.org/10.1016/S1364-6613(99)01417-5)
- Gibbon, J., Church, R. M., & Meck, W. H. (1984). Scalar Timing in Memory. *Annals of the New York Academy of Sciences*, 423(1), 52–77. <https://doi.org/10.1111/j.1749-6632.1984.tb23417.x>
- Graham-Schmidt, K. T., Martin-Iverson, M. T., Holmes, N. P., & Waters, F. A. V. (2016). When one’s sense of agency goes wrong: Absent modulation of time perception by voluntary actions and reduction of perceived length of intervals in passivity symptoms in schizophrenia. *Consciousness and Cognition*, 45, 9–23. <https://doi.org/10.1016/j.concog.2016.08.006>
- Haggard, P., Clark, S., & Kalogeras, J. (2002). Voluntary action and conscious awareness. *Nature Neuroscience*, 5(4), 382–385. <https://doi.org/10.1038/nn827>
- Haggard, P., & Tsakiris, M. (2009). The Experience of Agency: Feelings, Judgments, and Responsibility. *Current Directions in Psychological Science*, 18(4), 242–246. <https://doi.org/10.1111/j.1467-8721.2009.01644.x>
- Hagura, N., Kanai, R., Orgs, G., & Haggard, P. (2012). Ready steady slow: Action preparation slows the subjective passage of time. *Proceedings of the Royal Society B: Biological Sciences*, 279(1746), 4399–4406. <https://doi.org/10.1098/rspb.2012.1339>
- Hayashida, K., Nishi, Y., Masuike, A., & Morioka, S. (2020). Intentional Binding Effects in the Experience of Noticing the Regularity of a Perceptual-Motor Task. *Brain Sciences*, 10(9), Article 9. <https://doi.org/10.3390/brainsci10090659>
- Hoerl, C. (2019). Temporal binding and the perception/cognition boundary. In *The Illusions of Time* (pp. 275–287). Springer.

- Hoerl, C., Lorimer, S., McCormack, T., Lagnado, D. A., Blakey, E., Tecwyn, E. C., & Buehner, M. J. (2020). Temporal Binding, Causation, and Agency: Developing a New Theoretical Framework. *Cognitive Science*, *44*(5). <https://doi.org/10.1111/cogs.12843>
- Humphreys, G. R., & Buehner, M. J. (2009). Magnitude Estimation Reveals Temporal Binding at Super-Second Intervals. *Journal of Experimental Psychology: Human Perception and Performance*, *35*(5), 1542–1549. <https://doi.org/10.1037/a0014492>
- Humphreys, G. R., & Buehner, M. J. (2010). Temporal binding of action and effect in interval reproduction. *Experimental Brain Research*, *203*(2), 465–470. <https://doi.org/10.1007/s00221-010-2199-1>
- Imaizumi, S., & Tanno, Y. (2019). Intentional binding coincides with explicit sense of agency. *Consciousness and Cognition*, *67*, 1–15. <https://doi.org/10.1016/j.concog.2018.11.005>
- JASP Team. (2022). *JASP* (0.16.3). <https://jasp-stats.org/>
- Jenkins, M., & Obhi, S. S. (2021). Exploring the relationship between perceived Action-Outcome distance and Agency: Evidence from temporal binding. *Consciousness and Cognition*, *94*, 103177. <https://doi.org/10.1016/j.concog.2021.103177>
- Kirsch, W., Kunde, W., & Herbert, O. (2019). Intentional binding is unrelated to action intention. *Journal of Experimental Psychology: Human Perception and Performance*, *45*(3), 378. <https://doi.org/10.1037/xhp0000612>
- Klaffehn, A. L., Sellmann, F. B., Kirsch, W., Kunde, W., & Pfister, R. (2021). Temporal binding as multisensory integration: Manipulating perceptual certainty of actions and their effects. *Attention, Perception, and Psychophysics*, *83*(8), 3135–3145. <https://doi.org/10.3758/S13414-021-02314-0/TABLES/4>
- Kurby, C. A., & Zacks, J. M. (2008). Segmentation in the perception and memory of events. *Trends in Cognitive Sciences*, *12*(2), 72–79. <https://doi.org/10.1016/j.tics.2007.11.004>

- Lakens, D., McLatchie, N., Isager, P. M., Scheel, A. M., & Dienes, Z. (2020). Improving Inferences About Null Effects With Bayes Factors and Equivalence Tests. *The Journals of Gerontology: Series B*, 75(1), 45–57.
<https://doi.org/10.1093/GERONB/GBY065>
- Laugwitz, B., Held, T., & Schrepp, M. (2008). Construction and evaluation of a user experience questionnaire. *Lecture Notes in Computer Science (Including Subseries Lecture Notes in Artificial Intelligence and Lecture Notes in Bioinformatics)*, 5298 LNCS, 63–76. https://doi.org/10.1007/978-3-540-89350-9_6
- Lee, M. D., & Wagenmakers, E.-J. (2014). *Bayesian Cognitive Modeling: A Practical Course*. Cambridge University Press.
- Libet, B., Gleason, C. A., Wright, E. W., & Pearl, D. K. (1983). Time of conscious intention to act in relation to onset of cerebral activity (readiness-potential): The unconscious initiation of a freely voluntary act. *Brain*, 106(3), 623–642.
<https://doi.org/10.1093/brain/106.3.623>
- Liverence, B. M., & Scholl, B. J. (2012). Discrete events as units of perceived time. *Journal of Experimental Psychology: Human Perception and Performance*, 38(3), 549–554.
<https://doi.org/10.1037/a0027228>
- Lorimer, S., McCormack, T., Blakey, E., Lagnado, D. A., Hoerl, C., Tecwyn, E. C., & Buehner, M. J. (2020). The developmental profile of temporal binding: From childhood to adulthood. *Quarterly Journal of Experimental Psychology*, 73(10), 1575–1586. <https://doi.org/10.1177/1747021820925075>
- Lush, P., Caspar, E. A., Cleeremans, A., Haggard, P., Magalhães De Saldanha da Gama, P. A., & Dienes, Z. (2017). The Power of Suggestion: Posthypnotically Induced Changes in the Temporal Binding of Intentional Action Outcomes. *Psychological Science*, 28(5), 661–669. <https://doi.org/10.1177/0956797616687015>

- Moore, J. W., Lagnado, D., Deal, D. C., & Haggard, P. (2009). Feelings of control: Contingency determines experience of action. *Cognition*, *110*(2), 279–283.
<https://doi.org/10.1016/j.cognition.2008.11.006>
- Moore, J. W., & Obhi, S. S. (2012). Intentional binding and the sense of agency: A review. *Consciousness and Cognition*, *21*(1), 546–561.
<https://doi.org/10.1016/j.concog.2011.12.002>
- Morioka, S., Hayashida, K., Nishi, Y., Negi, S., Nishi, Y., Osumi, M., & Nobusako, S. (2018). Changes in intentional binding effect during a novel perceptual-motor task. *PeerJ*, *6*, e6066. <https://doi.org/10.7717/peerj.6066>
- Muth, F. V., Wirth, R., & Kunde, W. (2020). Temporal binding past the Libet clock: Testing design factors for an auditory timer. *Behavior Research Methods*, 1–20.
<https://doi.org/10.3758/s13428-020-01474-5>
- Pariyadath, V., & Eagleman, D. (2007). The Effect of Predictability on Subjective Duration. *PLOS ONE*, *2*(11), e1264. <https://doi.org/10.1371/journal.pone.0001264>
- Peirce, J., Gray, J. R., Simpson, S., MacAskill, M., Höchenberger, R., Sogo, H., Kastman, E., & Lindeløv, J. K. (2019). PsychoPy2: Experiments in behavior made easy. *Behavior Research Methods*, *51*(1), 195–203. <https://doi.org/10.3758/s13428-018-01193-y>
- Pfister, R., & Janczyk, M. (2013). Confidence intervals for two sample means: Calculation, interpretation, and a few simple rules. *Advances in Cognitive Psychology*, *9*(2), 74–80. <https://doi.org/10.5709/acp-0133-x>
- Poonian, S. K., & Cunnington, R. (2013). Intentional binding in self-made and observed actions. *Experimental Brain Research*, *229*(3), 419–427.
<https://doi.org/10.1007/s00221-013-3505-5>

- Press, C., Berlot, E., Bird, G., Ivry, R., & Cook, R. (2014). Moving Time: The Influence of Action on Duration Perception. *Journal of Experimental Psychology. General*, 143(5), 1787–1793. <https://doi.org/10.1037/a0037650>
- Ruess, M., Thomaschke, R., & Kiesel, A. (2020). Intentional binding for unintended effects. *Timing and Time Perception*, 8(3), 341–349. <https://doi.org/10.1163/22134468-bja10005>
- Ruz, M., & Lupiáñez, J. (2002). A review of attentional capture: On its automaticity and sensitivity to endogenous control. *Psicológica*, 23(2), 283–309.
- Sato, A., & Yasuda, A. (2005). Illusion of sense of self-agency: Discrepancy between the predicted and actual sensory consequences of actions modulates the sense of self-agency, but not the sense of self-ownership. *Cognition*, 94(3), 241–255. <https://doi.org/10.1016/j.cognition.2004.04.003>
- Schwarz, K. A., & Weller, L. (2022). Distracted to a fault: Attention, actions, and time perception. *Attention, Perception, & Psychophysics*. <https://doi.org/10.3758/s13414-022-02632-x>
- Schwarz, K. A., Weller, L., Klaffehn, A. L., & Pfister, R. (2019). The effects of action choice on temporal binding, agency ratings, and their correlation. *Consciousness and Cognition*, 75, 102807. <https://doi.org/10.1016/j.concog.2019.102807>
- Seligman, M. E. (1972). Learned helplessness. *Annual Review of Medicine*, 23(1), 407–412.
- Sheliga, B. M., Riggio, L., Craighero, L., & Rizzolatti, G. (1995). Spatial attention-determined modifications in saccade trajectories. *Neuroreport: An International Journal for the Rapid Communication of Research in Neuroscience*, 6(3), 585–588. <https://doi.org/10.1097/00001756-199502000-00044>

- Siebertz, M., & Jansen, P. (2022). Diverging implicit measurement of sense of agency using interval estimation and Libet clock. *Consciousness and Cognition*, *99*, 103287.
<https://doi.org/10.1016/j.concog.2022.103287>
- Suzuki, K., Lush, P., Seth, A. K., & Roseboom, W. (2019). Intentional binding without intentional action. *Psychological Science*, *30*(6), 842–853.
- Tanaka, T., Matsumoto, T., Hayashi, S., Takagi, S., & Kawabata, H. (2019). What Makes Action and Outcome Temporally Close to Each Other: A Systematic Review and Meta-Analysis of Temporal Binding. *Timing and Time Perception*, *7*(3), 189–218.
<https://doi.org/10.1163/22134468-20191150>
- Tomassini, A., Gori, M., Baud-Bovy, G., Sandini, G., & Morrone, M. C. (2014). Motor commands induce time compression for tactile stimuli. *The Journal of Neuroscience: The Official Journal of the Society for Neuroscience*, *34*(27), 9164–9172.
<https://doi.org/10.1523/JNEUROSCI.2782-13.2014>
- Tomassini, A., Gori, M., Burr, D., Sandini, G., & Morrone, M. C. (2012). Active movement restores veridical event-timing after tactile adaptation. *Journal of Neurophysiology*, *108*(8), 2092–2100. <https://doi.org/10.1152/jn.00238.2012>
- Tomassini, A., & Morrone, M. C. (2016). Perceived visual time depends on motor preparation and direction of hand movements. *Scientific Reports*, *6*(1), 27947.
<https://doi.org/10.1038/srep27947>
- Tsakiris, M., & Haggard, P. (2003). Awareness of somatic events associated with a voluntary action. *Experimental Brain Research*, *149*(4), 439–446.
<https://doi.org/10.1007/s00221-003-1386-8>
- Vastano, R., Ambrosini, E., Ulloa, J. L., & Brass, M. (2020). Action selection conflict and intentional binding: An ERP study. *Cortex*, *126*, 182–199.
<https://doi.org/10.1016/j.cortex.2020.01.013>

- Vogel, D. H. V., Jording, M., Esser, C., Weiss, P. H., & Vogeley, K. (2021). Temporal binding is enhanced in social contexts. *Psychonomic Bulletin & Review*, 28(5), 1545–1555. <https://doi.org/10.3758/s13423-021-01928-7>
- Wegner, D. M., & Wheatley, T. (1999). Apparent mental causation: Sources of the experience of will. *American Psychologist*, 54(7), 480–492. <https://doi.org/10.1037/0003-066X.54.7.480>
- Weller, L., Schwarz, K. A., Kunde, W., & Pfister, R. (2020). Something from nothing: Agency for deliberate nonactions. *Cognition*, 196. <https://doi.org/10.1016/j.cognition.2019.104136>
- Wen, W., Yamashita, A., & Asama, H. (2015). The influence of action-outcome delay and arousal on sense of agency and the intentional binding effect. *Consciousness and Cognition*, 36, 87–95. <https://doi.org/10.1016/j.concog.2015.06.004>
- Wiener, M., Zhou, W., Bader, F., & Joiner, W. M. (2019). Movement Improves the Quality of Temporal Perception and Decision-Making. *Eneuro*, 6(4), ENEURO.0042-19.2019. <https://doi.org/10.1523/ENEURO.0042-19.2019>
- Wolpe, N., Haggard, P., Siebner, H. R., & Rowe, J. B. (2013). Cue integration and the perception of action in intentional binding. *Experimental Brain Research*, 229(3), 467–474. <https://doi.org/10.1007/s00221-013-3419-2>
- Yokosaka, T., Kuroki, S., Nishida, S., & Watanabe, J. (2015). Apparent Time Interval of Visual Stimuli Is Compressed during Fast Hand Movement. *PLOS ONE*, 10(4), e0124901. <https://doi.org/10.1371/journal.pone.0124901>
- Yon, D., Edey, R., Ivry, R. B., & Press, C. (2017). Time on your hands: Perceived duration of sensory events is biased toward concurrent actions. *Journal of Experimental Psychology: General*, 146(2), 182–193. <https://doi.org/10.1037/xge0000254>

- Zacks, J. M., & Swallow, K. M. (2007). Event Segmentation. *Current Directions in Psychological Science*, 16(2), 80–84. <https://doi.org/10.1111/j.1467-8721.2007.00480.x>
- Zakay, D., & Block, R. A. (1995). An attentional-gate model of prospective time estimation. *Time and the Dynamic Control of Behavior*, 5, 167–178.
- Zakay, D., & Block, R. A. (1997). Temporal Cognition. *Current Directions in Psychological Science*, 6(1), 12–16. <https://doi.org/10.1111/1467-8721.ep11512604>
- Zhao, K., Chen, Y.-H., Yan, W.-J., & Fu, X. (2013). To Bind or Not to Bind? Different Temporal Binding Effects from Voluntary Pressing and Releasing Actions. *PLoS ONE*, 8(5), e64819. <https://doi.org/10.1371/journal.pone.0064819>