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# Eye did this! Sense of agency in eye movements

Julian Gutzeit *, Lisa Weller, Felicitas Muth, Jens Kürten, Lynn Huestegge

*Julius-Maximilians-Universität Würzburg, 11, 97070 Würzburg, Germany*

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ABSTRACT

This study investigates the sense of agency (SoA) for saccades with implicit and explicit agency measures. In two eye tracking experiments, participants moved their eyes towards on-screen stimuli that subsequently changed color. Participants then either reproduced the temporal interval between saccade and color-change (Experiment 1) or reported the time points of these events with an auditory Libet clock (Experiment 2) to measure temporal binding effects as implicit indices of SoA. Participants were either made to believe to exert control over the color change or not (agency manipulation). Explicit ratings indicated that the manipulation of causal beliefs and hence agency was successful. However, temporal binding was only evident for caused effects, and only when a sufficiently sensitive procedure was used (auditory Libet clock). This suggests a feebler connection between temporal binding and SoA than previously proposed. The results also provide evidence for a relatively fast acquisition of sense of agency for previously never experienced types of action-effect associations. This indicates that the underlying processes of action control may be rooted in more intricate and adaptable cognitive models than previously thought. Oculomotor SoA as addressed in the present study presumably represents an important cognitive foundation of gaze-based social interaction (social sense of agency) or gaze-based human-machine interaction scenarios.

*Public significance statement:* In this study, sense of agency for eye movements in the non-social domain is investigated in detail, using both explicit and implicit measures. Therefore, it offers novel and specific insights into comprehending sense of agency concerning effects induced by eye movements, as well as broader insights into agency pertaining to entirely newly acquired types of action-effect associations. Oculomotor sense of agency presumably represents an important cognitive foundation of gaze-based social interaction (social agency) or gaze-based human-machine interaction scenarios. Due to peculiarities of the oculomotor domain such as the varying degree of volitional control, eye movements could provide new information regarding more general theories of sense of agency in future research.

## 1. Introduction

Healthy individuals normally feel control over their actions and the consequences that these actions produce in the environment. This feeling of having control over one's actions and the resulting impact on the environment has often been termed *sense of agency* (Gallagher, 2000; Haggard et al., 2002; Moore & Obhi, 2012), and it has been subject to numerous studies (for review see Haggard, 2017) that have predominantly investigated manual actions. However, it appears especially interesting to study sense of agency in action modalities (motor systems) that usually do not elicit strong environmental changes in most contexts. A prime example would be eye movements, which usually do not affect the world around us and which are regularly triggered either autonomously or on the basis of motor routines as opposed to explicit intentions. Corresponding research would thus shed more light on fundamental issues such as the question of whether sense of agency is a universal, domain-general phenomenon that occurs whenever environmental changes can be elicited with our body movements, and if it can be experienced for entirely newly acquired (instead of well-learned) types of action-effect associations. To address this issue, we here apply the general logic of studying sense of agency in the manual research domain to the oculomotor system.

Eye movements are predominantly used to move information into the center of the visual field, but only rarely to exert an influence on the environment (probably apart from the special case of gaze-based social interaction). In addition, the oculomotor system is special because of its

* Corresponding author at: Julius-Maximilians-Universität, Röntgenring 11, 97070 Würzburg, Germany.
*E-mail addresses:* julian.gutzeit@uni-wuerzburg.de (J. Gutzeit), lisa.weller@uni-wuerzburg.de (L. Weller), felicitas.muth@uni-wuerzburg.de (F. Muth), jens.kuerten@uni-wuerzburg.de (J. Kürten), lynn.huestegge@uni-wuerzburg.de (L. Huestegge).

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high general degree of involuntary or routine-based control (e.g. Huestegge et al., 2019; Leigh & Kennard, 2004) as well as its high movement speed (Büttner & Büttner-Ennever, 2006) and frequency (Schiller, 1998). In particular, the eyes must move after some (usually sub-second) temporal interval, while this is not the case for other parts of the body. In addition, processes underlying motor decisions are considered to differ between different effector systems, for example, between manual and oculomotor actions (Bompas et al., 2017). Taken together, the rather unique characteristics of oculomotor control raise the question of the extent to which sense of agency effects may transfer to the oculomotor domain. In turn, insights into sense of agency in oculomotor control may also inform general sense of agency theories.

Apart from this theoretical motivation, sense of agency in saccades is also highly relevant from an applied perspective. As technology advances, many new forms of action can be used to control our environment. For instance, eye tracking technology can serve as an aid for severely impaired patients and re-enable them to participate and communicate in everyday life (e.g. Ball et al., 2010; Borgestig et al., 2016; Pasqualotto et al., 2015). As eye tracking technology is becoming increasingly widespread, it is also used in everyday applications (Hyrskykari et al., 2005; Majaranta & Bulling, 2014). In some cases, control with eye movements can even exceed the performance of conventional input methods, for instance in pointing (Asai et al., 2000; Tanriverdi & Jacob, 2000). Thus, addressing sense of agency in eye movements would also inform the design of eye tracking devices, as the feeling of being in control is a vital element for usability of devices (Limerick et al., 2014; Shneiderman et al., 2018) and the acceptance and usability of devices are known to be reduced when sense of agency gets disrupted (Berberian, 2019). In the following paragraph, we briefly discuss different explanatory models and measurement methods for sense of agency.

### 1.1. Models and measures of sense of agency

Recent models aimed to explain how sense of agency is generated and which factors determine whether people experience agency for the consequences of their actions. These models assume that the sense of agency is based on information from different sources. In particular, cues from the motor system can inform the sense of agency. For instance, a correspondence between the expected perceptual outcome of a movement and the actually experienced outcome should lead to a more pronounced sense of agency. This was assumed, for example, by internal forward models (Blakemore et al., 2002; Frith et al., 2000; Wolpert et al., 1995) or in the context of ideomotor ideas of action control (for principles of ideomotor action control see e.g., Hommel, 2009; Klaffehn et al., 2019; Kunde, 2001; Pfister, 2019; Waszak et al., 2012). Another source of information for the sense of agency may stem from external cues, such as the belief about an action and its causal effects (Desantis et al., 2011; Wegner, 2003). Recent models have suggested that sense of agency is generated on the basis of these various cues, while differences as to how strongly each cue influences the sense of agency are ascribed to situational/contextual factors (e.g., Moore et al., 2009; Synofzik et al., 2008, 2013).

Sense of agency can be measured explicitly using ratings of subjects' perceived control over their actions and resulting effects. This rather direct, introspective assessment is especially plausible as the term 'sense of agency' itself appears to refer to an experiential concept accessible via introspection. Nevertheless, some authors argued that such introspective judgements can be biased and that they are potentially susceptible to demand characteristics (Dewey & Knoblich, 2014; Sato & Yasuda, 2005; Wegner & Wheatley, 1999). Therefore, many authors resorted to assessing sense of agency with implicit measures, too, such as sensory attenuation effects or temporal binding effects which were shown to correlate to some extent with the presence of agency (Haggard et al., 2002; for a review, see Moore & Obhi, 2012 and Tanaka et al., 2019) and might reflect further aspects of the construct (i.e., the *feeling of agency*, Synofzik et al., 2013).

Temporal binding refers to the subjective compression of the time interval between a voluntary action and its subsequent perceptual consequence, compared to a baseline condition that does not involve a voluntary action causing a perceptual effect. This phenomenon is usually measured by implementing a direct temporal interval estimation, for example, by either asking participants to numerically quantify the length of the interval (e.g., Engbert et al., 2007) or to reproduce the temporal interval between their action and the perceptual effect with a motor action (e.g., Humphreys & Buehner, 2010). A baseline condition within such a temporal interval reproduction approach typically involves the presentation of two sensory events (e.g., the action is replaced by the onset of a stimulus). Alternatively, a clock may be used to judge the time points of the critical events, namely the action and the effect onset (Haggard et al., 2002). In the latter case, a rotating clock is visually displayed (e.g., 2560 ms for a full rotation; Libet et al., 1983), and participants estimate the timing of their action and the subsequent effect by reporting the position of the clock's indicator at the occurrence of the respective event. These temporal judgements of action timing and effect timing are then each compared with a baseline condition, in which either only an action is executed or only an effect is presented in isolation. Typically, actions are perceived later in time when they cause a subsequent effect compared to the baseline condition. In addition, effects are perceived earlier in time when they follow an action compared to the baseline condition (Haggard et al., 2002; for review, see Tanaka et al., 2019). Thus, the event timing approach, in contrast to the temporal interval reproduction approach, allows to distinguish between action binding and effect binding.

While temporal binding has often been interpreted as an implicit measure of agency (Haggard & Tsakiris, 2009), research has suggested that a causal link between two events (i.e., without any agency or intentional action involved) might already suffice to cause temporal binding effects (Borhani et al., 2017; Buehner, 2012, 2015; Hoerl et al., 2020; Kirsch et al., 2019; Ruess et al., 2020), thereby questioning the assumption that temporal binding exclusively indexes agency. However, other studies have reported that temporal binding effects are at least stronger in intentional actions (i.e., between an action and its causal effect) compared to two merely otherwise causally linked events (Borhani et al., 2017; Cravo et al., 2011). Patrick Haggard, the researcher first describing the association of temporal binding and agency (even referring to it as 'intentional binding', Haggard et al., 2002), also addresses these issues in his later work: 'Many factors influence time perception, including attention, causality, pharmacological agents and adaptation. Therefore, shifts in time perception are not diagnostic of a sense of agency. However, a difference in intentional binding between two appropriately chosen conditions can potentially be interpreted as a difference in sense of agency.' (Haggard, 2017, p. 199). These issues therefore call for a study design in which temporal binding can be clearly linked to sense of agency while other confounding factors are ruled out (Gutzeit et al., 2023), which we address below. In sum, research benefits from adding implicit measures to explicit measures of agency. While it is still up to debate whether temporal binding is caused by forward models of sensory motor control (Frith et al., 2000), by mere post-hoc cognitive inference (Wegner, 2003), by an interplay between predictive and postdictive processes (Synofzik et al., 2008), by mere causality (Hoerl et al., 2020), or by multisensory integration mechanisms (Klaffehn et al., 2021), temporal binding is still the most widely used implicit measure for sense of agency (Moore, 2016). Thus, to compensate for biases in explicit ratings, to capture different aspects of the construct Sense of Agency in a meaningful way (Synofzik et al., 2013), to produce results comparable to a large body of agency research, and to potentially shed more light on the phenomenon of temporal binding itself (see General Discussion), we aimed to include temporal binding as an implicit measure for sense of agency in our experiments.

### 1.2. Eye movements

Despite the fact that eye movements are usually considered to be characterized by a lower level of voluntary control overall (see above), research has indicated that basic saccades can also be controlled in terms of the perceptual effects that they produce (i.e., they underlie the same principles of ideomotor action control as manual actions; Herwig & Horstmann, 2011; Huestegge & Kreutzfeldt, 2012; Riechelmann et al., 2017). However, oculomotor actions also come with certain peculiarities which set them apart from manual actions (see above; Bompas et al., 2017; Büttner & Büttner-Ennever, 2006; Huestegge et al., 2019; Leigh & Kennard, 2004; Schiller, 1998). Consequently, it is of particular interest to study the extent to which findings and effects from other motor domains regarding sense of agency and related effects can be transferred to eye movements.

While most everyday movements are a typical way to manipulate all aspects of our external environment in daily life (e.g., hand movements while cleaning up the room or moving a foot to step on the brake pedal while driving), eye movements can impact almost exclusively one's *social* external environment by communicating with a social interaction partner via gaze exchange. Thus, several studies have addressed sense of agency in terms of social gazing. For instance, explicit ratings of the sense of agency for another person's gaze were shown to be affected by the timing, contiguity, and congruency of the other's gaze reaction (Brandi, et al., 2019; Pfeiffer et al., 2012) . Nevertheless, sense of agency for eye movements was also found in some non-social contexts. For instance, 6- to 8-months-old infants were able to learn to control gaze-contingent eye tracking interfaces to alter on-screen stimuli (Deligianni et al., 2011; Wang et al., 2012). In other studies, naïve adult participants were able to discover gaze-contingent stimulus changes and then to actively exert control over these stimuli (Grgič et al., 2016; Grynszpan et al., 2012). Taken together, several studies have indicated that people can principally experience agency when a situation allows them to control effects (both social and inanimate) with their gaze.

Research on temporal binding for eye movements, in contrast, is comparatively scarce and the results less straightforward. Melcher et al. (2020) investigated the influence of intentionality on time perception after saccades using the Libet clock. They found that intentional saccades that caused a sound effect were perceived as occurring later than they actually did, similar to intentional manual actions. This was not observed for saccades when participants had to react to a cue signaling the onset and direction of the saccade. Furthermore, the ensuing effect (the tone) was not perceived earlier than it actually appeared, neither in the case of intentional saccades, nor in the case of cued saccades. Importantly, however, in contrast to typical experiments on temporal binding using the Libet clock these experiments did not involve a baseline condition in which, for example, saccade and effect were each presented in isolation, as is, however, necessary and typical in studies investigating sense of agency (e.g., Beck et al., 2017; Haggard et al., 2002; Schwarz et al., 2019; for review, see Tanaka et al., 2019). Thus, one cannot decide whether the perceived timing of saccades and tone effects in the experiment (i.e., in conditions in which saccades elicited tones) was actually shifted compared to a proper baseline. Additionally, participants were not informed that they could cause effects by moving their eyes in this experiment, thereby making it difficult to judge whether the results can actually be linked to intentions to produce an effect (and thus sense of agency).

In another study, Stephenson et al. (2018) investigated temporal binding in a social setting using an on-screen face stimulus. The participants' task was to fixate the on-screen face and to execute a saccade to an object in the other half of the screen as soon as the object appeared. Shortly afterwards, the on-screen face shifted its gaze to look towards the object. This gaze-following stimulus was triggered 400–2300 ms after the object appeared and was thus independent from the timing of the subjects' eye movements. Nevertheless, participants were told that their gaze would cause the gaze-following behavior. Subsequently, participants were asked to reproduce the interval between the appearance of the object and the gaze-following stimulus. The authors found evidence for temporal binding, that is, shorter interval reproductions for the interval between object appearance and gaze-following compared to conditions in which participants did not perform saccades (but object appearance and gaze-change of the on-screen face were the same). In contrast, no binding was found for a non-social control condition in which the face stimulus and the object were replaced by two fixation crosses, and the fixation cross replacing the face stimulus was at some point enlarged (equivalent to the gaze-following behavior in the experimental condition). These results somewhat surprisingly (as this social specificity was usually not found in corresponding research in other action domains) suggest that temporal binding for eye movements may be limited to social situations. However, it is important to consider that, strictly speaking, this experiment did not involve an actual gaze-contingent paradigm, as the subject's saccades did not cause the face stimulus to change, and therefore they did not exert real agency over the face. This was even reinforced by asking participants to estimate the interval between the appearance of the cue and the gaze-following stimulus, two events that were essentially independent from participants' actions (rather than the interval between the participants' saccade and the gaze-following stimulus). In this case, any temporal binding effect cannot be directly linked to sense of agency but rather to causal relations between the two events.

Furthermore, another problem might arise when studying temporal binding and sense of agency in eye movements that, to our knowledge, has not yet been addressed: Saccadic eye movements per se can lead to specific temporal distortions. For example, the chronostasis effect describes a subjective slowing of perceived time right after a saccade (Yarrow et al., 2001). Typical sense of agency research compares operant agency conditions (with an action triggering a subsequent effect) with baseline conditions. These baseline conditions involve either two externally generated effects (mostly in interval estimation paradigms, as, for example in Humphreys & Buehner, 2009), or isolated effects and isolated actions (mostly in Libet clock paradigms, as, for example, in Haggard et al., 2002). Both variants have in common that the baseline conditions differ from the agency conditions not only in terms of the degree of sense of agency, but also in terms of the presence or absence of movements. This might generally lead to confounds regarding the interpretation of any temporal binding findings (Gutzeit et al., 2023), but especially in the case of eye movements given the well-known temporal distortions following saccades described above. Thus, for a thorough investigation of temporal binding as a function of sense of agency in saccades, it is particularly important that a saccade is performed in both the operant agency condition and in the baseline condition, so that any differences in temporal estimates are exclusively attributable to different degrees of sense of agency.

Taken together, the mixed results of both studies described above as well as the potential procedural confounds regarding saccadic temporal distortions call for a closer inspection of temporal binding for eye movements, while simultaneously controlling for the crucial link of any temporal binding effect to actual sense of agency. In the present study, we thus report two experiments (plus one pilot study) which investigated temporal binding for saccades in a non-social situation.

### 1.3. The present study

In the present study, we aimed to investigate temporal binding for eye movements and its relation to sense of agency. To that end, we established two conditions that only differed with respect to the participants' believe about their sense of agency (i.e., about the causality relation between action and effect) in order to selectively manipulate sense of agency while retaining the presence of both an action and a subsequent stimulus change in all conditions to exclude alternative explanations for temporal binding effects (in line with Haggard's (2017) advice to come up with most “appropriately chosen conditions”, see above). In each condition, participants performed self-paced saccades from a fixation cross at the center of the screen to one of four distinct stimuli presented around the fixation cross. Two of these stimuli retained their white color until they were fixated, while the other two stimuli constantly changed their color after random temporal intervals. In the agency condition, participants were required to fixate the central fixation cross and then to execute a saccade to any of the two unchanging (white) stimuli. Only after fixation onset, the color of the targeted stimulus changed (delayed by a certain time interval). In a control condition, participants were required to fixate any of the color changing stimuli. When the subject fixated one of those stimuli, both stimuli changed their color for a last time (delayed by the same time intervals as in the agency condition), seemingly with no causal relation to the saccade of the participant (non-intentional effect baseline; cf. Gutzeit et al., 2023).[1] We asked subjects to estimate the time interval between their fixation of the stimulus and the gaze contingent (agency condition) or final (non-intentional effect baseline condition) color change by reproducing the temporal interval by pressing and holding the space bar. Note that in each condition all four stimuli were presented on the screen to keep possible confounding factors such as arousal or workload constant between conditions. If sense of agency is reflected in temporal binding effects (as assumed in previous theoretical accounts of sense of agency reviewed above) and if participants are indeed able to experience sense of agency for eye movements (as assessed via explicit agency ratings), one should expect stronger temporal binding, as evident in a relative underestimation of the time interval between action and effect, in the agency condition (which should also be characterized by substantially stronger agency ratings) compared to the baseline condition.

Note that we explicitly instructed participants that they would be able to cause the color change in the agency condition by fixating the stimulus, while all color changes in the baseline condition, including the last color change after their fixation, were occurring independently from their saccades. This way, we actively and deliberately induced strong causal beliefs about their actions and following effects. On the one hand, we wanted to explicitly point out to our subjects that they can influence the environment with their eye movements. As eye movements usually do not affect our environment, we reasoned that a very clear instruction regarding this link is essential. Difficulties in perceiving gaze-based contingencies have already been reported in the literature (Grgič et al., 2016; Wang et al., 2012). On the other hand, by inducing these causal beliefs we hoped to successfully manipulate the sense of agency, in particular cognitive judgements of agency (Buehner, 2012; Hoerl et al., 2020; Synofzik et al., 2013). Because the action-effect association between a saccade and a color change should have never been experienced before by our participants (who were naïve regarding the task), we relied on the strong induction of causal beliefs and associated cognitions (in contrast to previous work on sense of agency that often relies on types of action-effect association that are already well-learned prior to the experiment, e.g., between manual actions and visual effects). In past studies, it has been shown that such instructive inductions could also lead to action-effect associations that have never been experienced beforehand (Dogge et al., 2019; Liefooghe et al., 2012; Meiran et al., 2017). In this way, we created two conditions that differed only in their subjective sense of agency (and not with respect to the occurrence of saccades and perceptual changes, which was constant across conditions). Since conditions only differed in their subjective sense of agency, we believe that potential temporal binding effects in this paradigm can thus safely be linked to actual sense of agency.

In addition, we collected ratings of the stimuli regarding valence and arousal to assess whether manipulable (agency) stimuli were rated more positively than non-manipulable (baseline) stimuli, as control over stimuli is known to increase motivation (Bandura, 1991; Eitam et al., 2013), and whether frequent color changes of the baseline stimuli might be associated with higher levels of arousal.

In both conditions, we systematically varied the delays for all effects to investigate a potential attenuation of sense of agency effects with longer delays. Delayed action effects were previously reported to have a negative impact on the experienced sense of agency (Blakemore et al., 1999). However, findings are ambiguous with respect to the influence of specific delay durations. Several studies showed diminished explicit agency ratings for delays above 200–400 ms (David et al., 2016; Ebert & Wegner, 2009; Farrer et al., 2013; Haering & Kiesel, 2015; Kühn et al., 2011; Sato & Yasuda, 2005; Wen et al., 2015), and a similar effect on temporal binding (Haggard et al., 2002; Ruess et al., 2017). Other studies, however, did not replicate these findings for agency ratings (Farrer et al., 2008; Weller et al., 2020) or temporal binding (Dewey & Knoblich, 2014; Humphreys & Buehner, 2009; Kühn et al., 2013; Ruess et al., 2017; Wen et al., 2015), for a comprehensive review see (Wen, 2019). Despite this mixed evidence, one might still expect that larger delays would (if anything) reduce temporal binding and agency ratings in the agency condition. Since we anticipated that the feeling of being in control over effects caused by saccades may not be experienced as very natural (see above), we were also interested in usability aspects of the saccade-based agency task, especially the perspicuity and the dependability of the eye tracking task. Therefore, we collected user experience ratings at the end of the experiment to compare them with benchmark data, which might indicate whether participants experienced problems with learning and performing the eye tracking task.

Prior to the following experiment, we conducted a pilot study that also included a condition in which participants could freely choose which stimulus they wanted to fixate. However, in this pilot study we encountered technical difficulties with the eye tracker, so numerous trials had to be excluded and, as a result, data might not be reliable. Hence, the methods, results, and discussion of this pilot study will not be presented in this manuscript but can be found in the supplementary material.

[1] Note that there were two reasons for why we decided that in the baseline condition *both* stimuli changed color after one of them was fixated. First, it was necessary that during the trial both baseline stimuli changed their color randomly and continuously, as participants could freely choose which of the two stimuli they wanted to fixate. Thus, it had to be established, that *both* stimuli changed their color independent of the behavior of the participant in each trial, as both of them could be the potential fixation target in the baseline condition. To keep the illusion of having no environmental impact on these stimuli, which was crucial for the baseline condition, the last color change (after the saccade was performed) occurred for both stimuli as well, with the aim of preventing a perceived disruption of the color-changing pattern in any way following the saccade. We included two stimuli for each condition to give participants some freedom of choice in both conditions, as this has led to increased agency ratings (Barlas et al., 2017; Schwarz et al., 2019; Sidarus et al., 2017; Wenke et al., 2010) and temporal binding (Barlas et al., 2017; Barlas & Obhi, 2013; but see Antusch et al., 2021) in some previous studies. Second, by also changing the color of the stimulus that was not fixated, we hoped to further diminish (subjectively) any causal link between the saccade and the last color change in the baseline condition, as the color change of the non-fixated stimulus was spatially distant to the landing point of the saccade. This should have further reduced any impression of causal connectedness between the saccade and the visual effect. If only one of the baseline stimuli had changed its color a last time, this might have induced perceived agency over the color change of the fixated stimulus. In contrast, the agency condition only involved a color change of the stimulus that was fixated. We reasoned that this should increase the differences in causal beliefs between conditions. However, this difference between conditions (two color changes after the saccade in the baseline condition vs. one color change in the agency condition) might, at least theoretically, also impact on time estimations or sense of agency. We address this potential confound in the General Discussion in more depth.

## 2. Experiment 1

### 2.1. Methods

#### 2.1.1. Transparency and openness

All experiments' sample sizes, variables, hypotheses, data treatments, and analyses were preregistered on Open Science Framework (https://osf.io/t8pnx/) prior to data collection. Raw data, analysis scripts, and stimulus materials are also available at https://osf.io/t8pnx/. This study was conducted in accordance with German Psychological Society (DGPs) ethical guidelines (2004, CIII) which do not require Institutional Review Board approval for the experiments reported in this article. We report how we determined our sample size, all data exclusions, all manipulations, and all measures in the study, and we follow JARS (Kazak, 2018). Data were analyzed using R, version 4.0.0 (Core R Team, 2021). This publication was supported by the Open Access Publication Fund of the University of Wuerzburg.

#### 2.1.2. Participants

From previous data based on a student's thesis we calculated an effect size for temporal binding in the agency condition of $d_z = 0.47$. A power analysis computed with R-package *pwr* (Champely, 2020) based on these results showed that a sample size of $n \approx 36$ would yield a power of $1 - \beta = 0.8$ to detect this medium-sized effect.

To fully counterbalance our participants across the eight different counterbalancing conditions, we recruited 40 participants in total. We excluded two participants who did not understand the time reproduction task (see Results section) and recruited two new participants to include n = 40 participants in our final data analysis (mean age: 27.27, SD: 8.44, range: 20–58, 67.50 % female, 92.50 % right-handed). All participants gave informed consent, reported normal or corrected to normal vision, and were compensated monetarily. Data were collected in the year 2021.

#### 2.1.3. Setup and stimuli

The experiment took place in a dimly lit room. Participants were seated in front of a 19″ cathode tube monitor (resolution: 1024 × 768 pixels, refresh rate 100 Hz). They placed their heads in a chin rest at a distance of approximately 70 cm from the screen to ensure stable head posture during the experiment. For manual responses, a standard German QWERTZ keyboard and mouse were used. Movements of the right eye were recorded with an eye tracker at a sampling rate of 1000 Hz (EyeLink 1000, SR Research Mississauga, Ontario, Canada). Stimulus presentation and logging of eye movements and manual responses was controlled with PsychoPy 3.0 (Peirce et al., 2019). The four stimulus icons (moon, sun, rocket, house) were each 30 × 30 pixels (1.17 cm × 1.17 cm, visual angle ≈ 1.35°) in size. The stimuli were presented simultaneously 252 pixels above and below the central fixation cross, and 448 pixels to the left and to the right of the central fixation cross. The colors of all stimuli (yellow, blue, orange, green, purple) had the same intensity in terms of their RGB and HSL values to achieve isoluminance. To ensure a reliable detection of stimulus fixations (accounting for imprecisions in the eye tracking) we implemented interest areas around each stimulus of 1° visual angle. A fixation was recorded when the gaze position was detected inside one of these interest areas.

#### 2.1.4. Task

In each trial, all four stimuli were presented simultaneously around the central fixation cross. Two opposite stimuli always changed their color (blue, orange, green, or purple) at random intervals (500, 700, 900, or 1100 ms) in a fixed order that was randomized across all subjects. The other two opposite stimuli retained their white color. Which stimulus pair changed its color, and which did not (rocket and house/sun and moon) as well as the position of the two opposite stimulus pairs (above/below vs. left/right of central fixation cross) were counterbalanced between participants.

Participants should either fixate one of the two agency stimuli or one of the two baseline stimuli. These tasks were presented block-wise in alternation within four blocks in total (ABAB design). The starting task (agency vs. baseline) was counterbalanced between subjects, resulting in eight balancing conditions in total (2 stimulus positions × 2 stimulus identities × 2 starting blocks). Each trial started with a fixation cross in the center of the screen. After 750–1250 ms of fixating the cross all four stimuli appeared. This was to prevent participants from fixating the position of the target stimulus before the start of the trial. Participants were instructed to wait a short time prior to fixating any of the stimuli. This way, we ensured they would see at least one color change of the baseline stimuli before they fixated any stimulus. After this initial waiting period, they were free to move their eyes to the required stimulus at any time. As soon as the gaze position of the participant was located inside the one of the interest areas a fixation was recorded. When one of the white stimuli was fixated (agency condition), both baseline stimuli stopped changing their colors and the fixated agency stimulus changed its color from white to yellow after a delay of 300, 500, or 700 ms. When one of the randomly color changing stimuli was fixated (non-intentional effect baseline or free choice condition), they both changed their color one last time after the same delays (300, 500, or 700 ms). Participants were told that they could cause the color change of the white stimuli, whereas the color changes of the baseline stimuli would occur completely independently of their gaze (even though the latter was technically not the case, see above). We deliberately instructed participants that they could change the color of the white stimuli to induce corresponding causal beliefs (see above), which should then propagate onto reported sense of agency accordingly. Note that the latter instruction should be very plausible for the participants as these particular stimuli already changed their color previously (see rating described below for a manipulation check and a validation of this assumption). In both conditions, the changed color was presented for 1000 ms. Although all stimuli were displayed in each condition, subjects could only produce effects on those stimuli that they were instructed to choose from as a fixation target. For instance, when an agency stimulus was fixated in the non-intentional baseline condition, it did not change its color.

After each trial, subjects were asked to estimate the time that elapsed between their fixation and the (final) color change of the stimulus by reproducing this interval via holding down the space bar. For each of the three different delays in every condition, participants had to rate their sensed agency for the color change after two randomly chosen corresponding trials in each block on a visual analogue scale ranging from "not at all" (0) to "a lot" (100),[2] resulting in a total of 24 agency ratings (4 per interval and condition).[3] For an overview of the trial structure see Fig. 1.

In each block, each delay interval (300, 500, 700 ms) was repeated twelve times, resulting in 36 trials in each block and 144 trials in total. Before the start of the experiment, participants were presented with six training trials for each condition to familiarize themselves with the task.

After the participants concluded the experiment, they were asked to rate the presented stimuli regarding their valence and associated arousal on a visual analogue scale to assess if stimuli that could be influenced (agency stimuli) were rated as more positive than those that could not be

[2] While the estimation was recorded as an integer value between 0 and 100, participants only saw the verbal poles on the scale, no numeric labels.

[3] We did not ask participants to report agency ratings in every trial to avoid disruption of the task and to prevent the task of becoming too difficult. Sidarus and Haggard (2016) compared trial-wise with block-wise agency ratings and found no difference in rating scores, but an impaired task performance for the group of participants that was asked for their rating after each trial. Measuring agency ratings only after certain trials rather than after every trial is well in line with previous research (e.g., Caspar et al., 2021; Pfister et al., 2021; Stephenson et al., 2018; Weller et al., 2017)

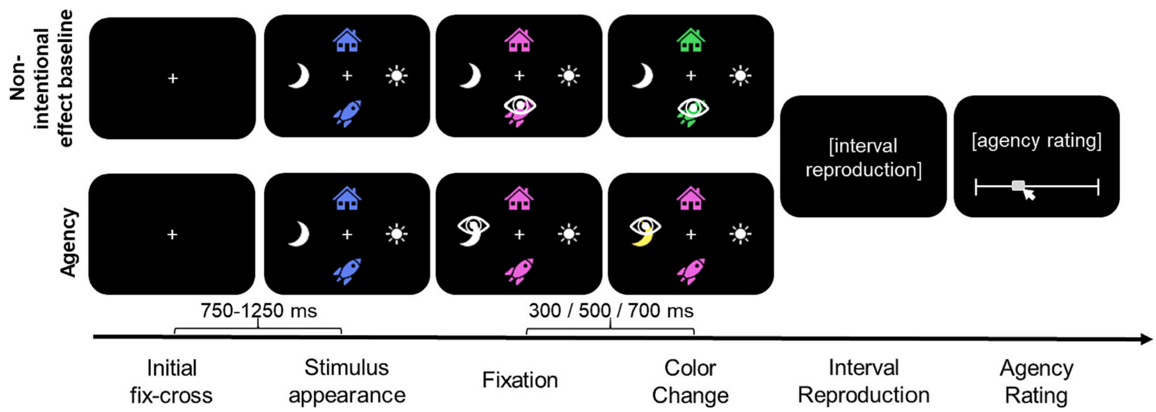


**Fig. 1.** Trial structure of the non-intentional effect baseline condition and the agency condition. All stimuli were presented simultaneously in every trial. At the beginning of each trial, the initial fixation cross was presented at the center of the screen. After 750–1250 ms of stable fixation of the cross, the four stimuli appeared. Each correctly fixated target stimulus then changed its color after a delay of 300, 500, 700 ms (for a last time). After each trial, participants estimated the time interval between their saccade and the (final) color change. Agency was (unpredictably) rated four times for each delay condition in each of the two task conditions.

influenced (baseline stimuli), and if the frequent color changes of the baseline stimuli might be associated with higher levels of arousal. Furthermore, they were asked if they had used a special strategy to reproduce the time intervals.

#### 2.1.5. Statistical analyses

We analyzed all data with repeated measures ANOVAs in R (Core R Team, 2021) based on $\alpha = 0.05$ for all reported results. If Mauchly's test of sphericity yielded $p < .05$, we corrected the degrees of freedom for every analysis with more than two factor levels. In these cases, we report uncorrected degrees of freedom with Greenhouse-Geisser adjusted *p*-values and the adjustment parameter ε. In case of significant effects, we conducted Bonferroni-adjusted post-hoc pairwise comparisons serving as post-hoc tests.

To assess temporal binding, we computed time estimation errors by subtracting the true interval between fixation onset and color change from the reproduced interval. Thus, negative (positive) values correspond to an under-(over-)estimation of the temporal interval. We calculated one ANOVA for time estimation errors and another ANOVA for explicit agency ratings with the two-level within-subject factor condition (agency vs. non-intentional baseline). In additional analyses we investigated the main effect of the time interval (one three-level factor: 300, 500, 700 ms) and the interaction of task condition and time interval on both agency ratings and time estimation errors. We anticipated shorter intervals to lead to higher sense of agency (stronger temporal binding and higher agency ratings).

For the valence and arousal ratings of the stimuli we computed two-sided paired *t*-tests (white agency stimuli vs. color-changing baseline stimuli). Ratings from the UEQ will be reported descriptively and compared with a benchmark (Schrepp et al., 2017).

### 2.2. Results

#### 2.2.1. Data treatment

To ensure that participants understood the time estimation task and reproduced the temporal intervals properly, we checked whether the duration of reproduced temporal intervals increased in the same order as the true temporal intervals (i.e., shorter interval estimations for 100 ms intervals compared to 500 ms intervals, 500 ms intervals compared to 900 ms intervals, and so on). Two of the participants deviated notably from this pattern and were thus excluded. These two participants were replaced with two newly recruited participants, yielding 40 participants that were included in all following analyses (see methods). All trials in which the gaze position of the remaining participants at the time of the color change was more than one degree of visual angle away from the stimulus were excluded (4.70 %). All trials in which the eye tracker was recalibrated and all trials immediately prior to these trials were excluded (0.09 %). Since the calibration of the eye tracker was sometimes inaccurate, there was a small number of trials in which the gaze position was not recorded precisely enough to detect a fixation inside the interest area around the stimulus. These trials were characterized by a long duration between trial onset and stimulus fixation, as participants fixated one of the target stimuli, but the recorded gaze position was not located inside the interest area (representing a precondition for triggering the color change). This led to relatively long intervals, as subjects believed to have already fixated the stimulus, while the eye tracker did not record the fixation correctly and the color change was not triggered. Thus, we excluded all trials with a delay of the target saccade of >10 s (0.53 %). We then computed z-scores for every duration until target fixation of every participant in each condition for each interval and excluded trials with durations with $|z| > 3$ (2.23 %) to exclude remaining trials with a failed fixation detection. Note that this issue was unforeseen, which is why we deviated from our pre-registered exclusion criteria here. Following these exclusions, trials with stimulus fixations before the required waiting time of 1100 ms were excluded (6.23 %). Further, we excluded trials in which the wrong type of stimulus was fixated first (0.92 %). We then excluded all trials with implausibly long time estimations (>2.8 s, four times the longest actual time interval; 0.06 %). Based on the remaining trials we computed z-scores for every time estimation error of every participant in each condition for each interval, and excluded trials with estimation errors of $|z| > 3$ (0.68 %). In total, 14.62 % of all trials were excluded.

#### 2.2.2. Estimation errors

The ANOVA of estimation errors revealed no significant main effect of condition, $F(1, 38) = 1.23$, $p = .275$, $\eta^2_G = 0.002$ (baseline: $M = -0.159$ s, $SE = 0.021$; agency: $M = -0.148$ s, $SE = 0.022$; see Fig. 2A). However, we found a significant main effect of interval, $F(2, 76) = 205.30$, $p < .001$, $\eta^2_G = 0.311$ ($\varepsilon = 0.58$), with weakest underestimation errors for the 300 ms interval ($M = -0.038$ s, $SE = 0.019$), medium underestimation errors for the 500 ms interval ($M = -0.150$ s, $SE = 0.022$), and strongest underestimation errors for the 700 ms interval ($M = -0.271$ s, $SE = 0.024$). Each pairwise comparison showed that the longer the interval the larger the value of the time estimation error (all differences $\geq$0.111 s, all $t(38) \geq 12.18$, all $p < .001$, all $p_{adj.} < .001$, see Table 1). Again, the interaction of condition and interval was not significant, $F(2, 76) = 0.14$, $p = .809$, $\eta^2_G < 0.001$ ($\varepsilon = 0.74$).

#### 2.2.3. Agency ratings

We excluded one participant from this analysis because of missing

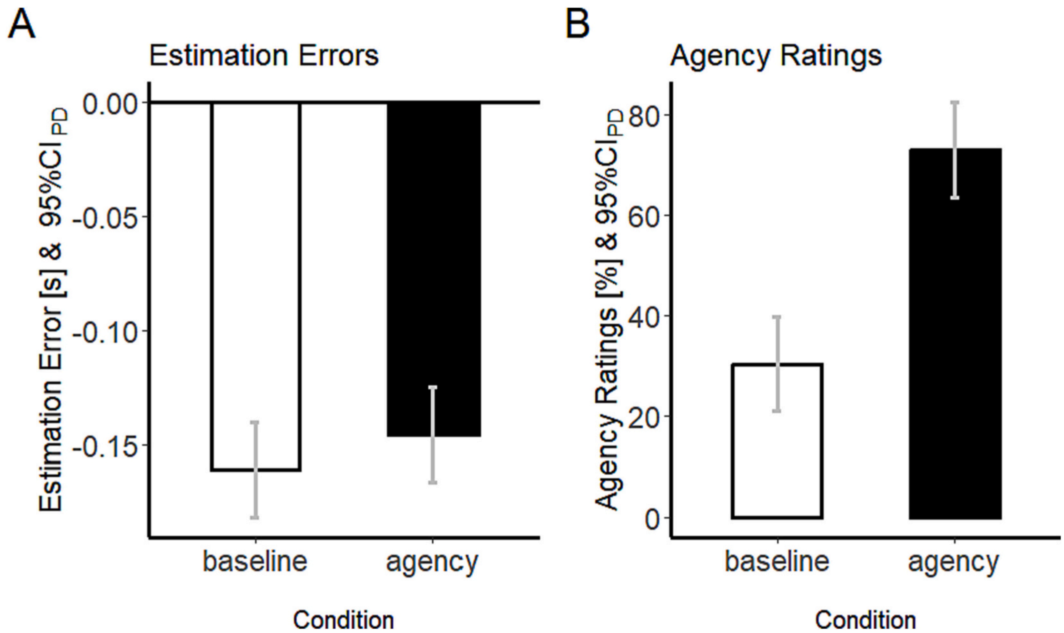


**Fig. 2.** Results of Experiment 1. Mean estimation errors (reproduced inter-event interval subtracted from true inter-event interval) as a function of condition (baseline vs. agency) with the non-intentional effect baseline (A). Agency ratings as a function of condition (B). All error bars indicate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

**Table 1**
Pairwise comparisons of Estimated Marginal Means for time estimation errors between all three interval conditions. Bonferroni-adjustments were applied based on 3 tests (p*3).

| Contrast | Difference | SE | df | t | *p* | *p* (adj.) |
|---|---|---|---|---|---|---|
| 300–500 ms | 0.111 | 0.009 | 38 | 12.18 | <.001 | <.001 |
| 300–700 ms | 0.232 | 0.016 | 38 | 14.9 | <.001 | <.001 |
| 500–700 ms | 0.121 | 0.008 | 38 | 14.69 | <.001 | <.001 |

values in at least one design cell. The ANOVA of agency ratings revealed a significant main effect of condition, $F(1, 37) = 77.02$, $p < .001$, $\eta^2_G = 0.434$, with higher ratings for agency stimuli (mean = 71.78 %, SE = 4.04 %) vs. baseline stimuli (mean = 29.48 %, SE = 3.69 %, see Fig. 2B). The main effect of interval was not significant, $F(2, 74) = 2.99$, $p = .068$, $\eta^2_G = 0.002$ ($\varepsilon = 0.81$), with agency ratings numerically tending to be lower at longer intervals (300 ms interval: $M = 52.00$ %, $SE = 3.27$ %; 500 ms interval: $M = 50.77$ %, $SE = 3.18$ %; 700 ms interval: $M = 49.11$ %, $SE = 2.85$ %). We found no significant interaction between condition and interval, $F(2, 74) = 1.54$, $p = .221$, $\eta^2_G = 0.001$.

#### 2.2.4. Stimulus ratings, reported task strategies, and UEQ ratings

While participants numerically rated the valence of agency stimuli more positively compared to the baseline stimuli (67.06 % [SE = 3.21 %] vs. 61.67 % [SE = 2.80 %]), this difference was not statistically significant, $t(39) = 1.36$, $p = .183$, $d_z = 0.215$. The difference between arousal ratings of the different stimuli types was even smaller (agency: 46.86 % [SE = 3.89 %] vs. baseline stimuli: 44.59 % [SE = 3.67 %]; $t(39) = 0.44$, $p = .659$, $d_z = 0.070$). 28.57 % of the participants reported that they counted numbers in their head to estimate the length of the time interval, 21.43 % stated that they relied on their gut-feeling, 7.14 % hummed a melody or rhythm in their head, 4.76 % visually reimagined the interval, 33.33 % reported no specific strategy, and 2.38 % reported other interval reproduction strategies. Participants rated the agency task on six subscales of the UEQ on a scale from −3 to +3. Four participants did not answer the questionnaire and thus produced missing values. For mean ratings of all subscales and for a comparison to benchmark data see Table 2.

**Table 2**
Mean UEQ ratings and standard errors. Results are compared to the benchmark data of Schrepp et al. (2017).

| Subscale | Mean | SE | Comparison to benchmark | Interpretation |
|---|---|---|---|---|
| Attractiveness | 0.98 | 0.19 | Below average | 50 % of results better, 25 % of results worse |
| Perspicuity | 1.83 | 0.21 | Good | 10 % of results better, 75 % of results worse |
| Efficiency | 0.93 | 0.22 | Below average | 50 % of results better, 25 % of results worse |
| Dependability | 0.92 | 0.20 | Below average | 50 % of results better, 25 % of results worse |
| Stimulation | −0.03 | 0.25 | Bad | In the range of the 25 % worst results |
| Novelty | 0.35 | 0.22 | Below average | 50 % of results better, 25 % of results worse |

### 2.3. Discussion

In Experiment 1, we exclusively manipulated sense of agency between conditions while keeping other factors such as workload, stimulus characteristics, and, in particular, the requirement of an eye movement followed by a subsequent stimulus change constant between conditions. As we found significantly higher agency ratings for agency trials compared to baseline trials, we conclude that our manipulation has been successful. That is, despite the fact that the participants' eye movements triggered the respective stimulus change in both conditions, it was only in the agency condition that they reported a pronounced sense that their action (the eye movement) produced the subsequent color change effect. While these results confirm the success of our induction of causal beliefs, they have to be interpreted cautiously. We explicitly told the participants that they were only able to trigger the color change in the agency condition and that all color changes in the baseline condition were occurring completely independently from their behavior. With these instructions, we aimed to deliberately induce strong causal beliefs to support the eye movement task (Grgič et al., 2016) and to design two conditions that exclusively differ in causal beliefs and thus judgements of agency (Gutzeit et al., 2023; Haggard, 2017). This instruction was thus 'stronger' than some instructions from previous studies investigating sense of agency (e.g., instructing subjects that sometimes they and sometimes the experimenter produces an effect, Sato & Yasuda, 2005). Based on the rating data we conclude that we were successful in manipulating sense of agency by inducing different causal beliefs (Desantis et al., 2011; Hoerl et al., 2020). On a basic level, this shows that participants can indeed experience agency with respect to eye movements. However, as mentioned before, this is hardly surprising given that the instructions clearly informed participants about their ability to affect their environment. This is also supported by the fact that although longer intervals tended to be associated with lower ratings in Experiment 1, this effect was not significant. This could indicate that the induced causal beliefs (via instruction) were so strong that they could not be attenuated by delaying the effect in our study, contrary to our hypotheses. However, it should also be noted that previous studies have not always found a reduction in sense of agency ratings due to effect delays (for review, see Wen, 2019). In sum, we can conclude that our ratings represented a good manipulation check for the intended induction of causal beliefs.

Unexpectedly, however, this successful manipulation of causal beliefs was not associated with corresponding significant differences in temporal reproductions between conditions. Looking at the data more closely, it appears unlikely that this was merely due to a lack of statistical power, as there was not even a strong numerical tendency in this regard in our data. Given that temporal binding effects have been quite reliably observed in previous studies (for review, see Tanaka et al., 2019), our data are rather indicative of the absence of such an effect.

Considering these mixed findings, we will speculate about several possible explanations for the absence of evidence for temporal binding effects. First, the present findings appear to be different from those previously reported with respect to other action domains (where a successful manipulation of sense of agency is usually associated with a corresponding temporal binding effect). This difference between effector systems might therefore be due to specific characteristics of the oculomotor system. For example, it is well known that saccades can distort time perception to some extent (Hunt & Cavanagh, 2009; Morrone et al., 2005; Yarrow et al., 2001). However, such distortions should have occurred in both the agency and the baseline condition as participants performed saccades in both, which speaks against saccade-based time distortion effects as an alternative explanation of the present results.

Second, the absence of evidence for temporal binding might indicate that participants did in fact not experience sense of agency for the gaze-contingent color changes. While in the explicit ratings they reported that they were the causal agent of these effects in the agency condition and not in the baseline condition, they might just have done so due to strong demand characteristics associated with the instruction. While there has been previous research reporting temporal binding and sense of agency for eye movements (Stephenson et al., 2018), it could be possible that eye movement tasks are not well learned and that it thus might take longer to establish a profound understanding of one's impact on external stimuli (Dogge et al., 2019; Wang et al., 2012). However, we made sure that participants could familiarize themselves with the task in training trials prior to the main experiment. However, we made sure that participants could familiarize themselves with the task in training trials before the main experiment. Further, UEQ ratings for the agency task on the Perspicuity subscale, a scale describing how easy it is to become familiar with a product (or task), were descriptively high (1.83 on a scale from −3 to +3) and equivalent to "good" compared with a benchmark (10 % of results better, 75 % of results worse) of several products and interfaces (Schrepp et al., 2017).[4] We thus conclude that the absence of significant differences in temporal binding between conditions in this experiment are therefore unlikely due to difficulties associated with learning the eye tracking task per se, but probably due to other factors.

Third, in the unintended effect baseline condition, participants observed the last color change in two stimuli after their saccade. We deliberately decided to implement two color changing stimuli to give participants some freedom of choice and to (locally) disassociate (part of) the effects from their action to further diminish subjective agency (see Footnote in "The present study" section). At first sight, one might thus argue that this might have introduced a confound in our paradigm, as the 'magnitude' (or saliency) of the effect following the saccade differed between conditions. It could be possible that, while there was no agency-driven temporal binding in the baseline condition, there might have been other factors at work that could have biased time perception of participants in a similar order of magnitude, thus overshadowing any differences between conditions. For example, one could assume that this might have led to a stronger redirection of attention towards the stimuli than in the agency condition, or it might have induced more workload. Both attention and workload have been shown to impact on time perception (Block et al., 2010; Block & Gruber, 2014; Schwarz & Weller, 2022; Zakay & Block, 1995). In addition, the color change of two stimuli might have redirected the attention of participants towards the effect more strongly. In a recent study by Schwarz and Weller (2022), the authors showed that (task-irrelevant) stimuli that capture the attention of participants could severely affect time perception and could even completely shift temporal binding effects into the opposite direction (leading to an expansion of the subjective temporal interval between action and effect). However, if the baseline color changes had truly attracted more attention, this should have shifted the end of the subjective action-effect interval more closely towards the veridical effect. This mechanism should therefore be in contrast to time estimations in the agency condition, where effect binding should induce a subjective temporal shift of the effect towards an earlier time. Thus, if there had been effects of attention redirection in the baseline condition, we would have expected *over*estimations compared to the agency condition. However, the potential attentional redirection towards the effect might have also caused a subjective temporal shift of the beginning of the interval towards a later time, possibly resulting in an underestimation of the interval. To test this possibility, one has to capture action and effect binding separately from each other. We did this in Experiment 2 using an auditory Libet clock, but did not find any differences in the temporal action shift between conditions (see below), rendering this possibility highly unlikely.

Further, higher cognitive load tends to lead to an underestimation of intervals in prospective durational judgements (Block et al., 2010). Thus, it might be possible that participants experienced temporal compression in the baseline condition due to workload effects that were of the same magnitude as temporal binding effects in the agency condition, cancelling out any differences between conditions. This, however, seems unlikely for two reasons. First, it is debatable whether the two color changes indeed lead to heightened workload compared to one color change. While participants were theoretically able to perceive one additional color changing stimulus after a saccade in the baseline condition than in the agency condition, the color changing peripheral non-target stimulus was not relevant for their task as they were asked to estimate the interval between their fixation and the last color change of the fixated stimulus. Thus, the task demands were practically identical between conditions. Second, the effects of temporal compression due to higher cognitive load are usually more subtle than the effects of temporal binding (for an overview of workload effects, see Block et al., 2010; for an overview of temporal binding effects, see Tanaka et al., 2019). Thus, even if the workload was higher in the baseline condition, any associated temporal compression should have been considerably smaller compared to the temporal binding in the agency condition, which should still have led to noticeable time estimation differences between conditions. In summary, we deem it highly unlikely that the

[4] Note that this benchmark consists of rating data regarding fully developed products. Comparisons with an experimental task must be interpreted cautiously, as our task was rather simple and basic in nature.

two color changing stimuli of the baseline condition (vs. one in the agency condition) meaningfully compromised the interpretation of the time estimation of the participants.

Lastly, the utilized interval reproduction task might have been too imprecise to find subtle differences in time estimation. This is to some extent in line with some previous research that reported smaller effect sizes and lower sensitivity in the context of interval estimation procedures (as the one employed here) compared to Libet clock procedures (Tanaka et al., 2019). It is worth considering that the lack of evidence for temporal binding effects in our study may be due to the limitations of the measurement technique rather than the absence of such effects in the context of eye movements. For example, Melcher et al. (2020) used a visual Libet clock paradigm to measure time distortions between saccades and auditory effects (although they did not focus on the important linkage between participants' saccades and their subsequent effects for addressing sense of agency proper, which renders their results not directly comparable to ours). Thus, to examine whether we would find temporal binding effects by using a potentially more sensitive time point estimation procedure, we conducted a third experiment using a Libet clock. In our study, however, participants cannot use their visual system to monitor time, since the oculomotor system is engaged in producing the color changes in the agency condition. For this reason, we chose to implement a novel Libet clock procedure which relies on *auditory* cues involving spoken letters (Cornelio Martinez et al., 2018; Muth et al., 2020) instead of a visual clock presentation in our experiment.

## 3. Experiment 2

Since we found no temporal binding effects with an interval estimation procedure in the previous experiment, we implemented a potentially more sensitive auditory Libet clock procedure in our second experiment to assess the experienced time points related to either the action (i.e., the saccade) or the environmental (color) change. The task conditions and temporal intervals in this experiment were the same as in Experiment 1.

### *3.1. Methods*

#### *3.1.1. Participants*

Although we implemented substantial changes to our paradigm, we still anticipated effect sizes for temporal binding of $d_z = 0.47$. We thus aimed for a sample size of n = 36 to establish sufficient power (see Methods of Experiment 1). To achieve full counterbalancing across 8 different counterbalancing conditions, we recruited n = 40 participants in total. One of the participants was not a native German speaker and struggled to understand the task. We excluded this person and recruited a new participant, thereby ensuring a complete data set of n = 40 subjects (mean age: 23, SD: 2.77, range = 18–29, 80.00 % female, 82.50 % right-handed). Data were collected in the year 2022.

#### *3.1.2. Setup and stimuli*

The setup of hardware and software and the stimulus design were the same as in Experiment 2. For the auditory Libet clock participants heard the German letters “A”, “F”, “I”, “O”, and “T” via headphones. Each letter was presented for 250 ms followed by a 250 ms interval of silence, resulting in a total sequence length of 2500 ms. After the last interval of silence following the letter “T”, the total sequence was repeated again starting with the letter “A” (Muth et al., 2020). Note that Muth et al. (2020) used their clock with a typical effect baseline condition, in which no action was performed, and participants only observed an effect. This differs from our paradigm, as we used the non-intended effect baseline, in which participants performed a saccade *and* observed an effect to come up with a more comparable baseline.

#### *3.1.3. Task*

The task was the same as in Experiment 1: Participants were instructed to either fixate a white (agency) stimulus or a color-changing (baseline) stimulus. Other than in the previous experiments, participants were not asked to reproduce the time interval between fixation and color change after each trial. Instead, they heard a (2500 ms) repeating loop consisting of the German letters (“A F I O T”) presented during each trial. They were instructed to pay attention to the time point of either their saccade to the target stimulus or the time point of the observed subsequent color change. After each trial, participants used a slider on a continuous visual analogue scale, depicting one full loop of the previously heard letters and pauses, always starting and ending with the first letter of each sequence (“A F I O T A”), to report the exact time point of either their fixation or the observed color change with a precision of 1 ms. Again, as in the previous experiments, participants were required to explicitly rate the experienced agency for the observed color changes after a (randomly chosen) sixth of all trials.

Each of the two conditions (agency and non-intentional effect baseline) was presented block-wise in an alternating manner, resulting in four blocks in total (ABAB design). Participants estimated only the timing of their fixation (or of the color change) in the first two blocks and only the timing of the color change (or of the saccade) in the last two blocks. We counterbalanced which condition was presented first (agency vs. baseline), which event participants had to estimate in the first two blocks (action vs. effect), and the position of the agency and baseline stimuli (left/right vs. top/bottom), resulting in 8 counterbalancing conditions. Prior to the main experiment, participants underwent twelve training trials to familiarize themselves with the task. The main task consisted of 4 blocks, each containing 36 trials (144 trials in total).

After completing the experiment, participants rated the stimuli regarding valence and arousal. They also rated the task of actively changing the color with their saccades (agency condition) using the UEQ (Laugwitz et al., 2008).

#### *3.1.4. Statistical analysis*

We analyzed all data with repeated-measures ANOVAs in the same manner as in Experiment 1, but separately for time estimations of actions and effects. Thus, we computed ANOVAs with the two-level within-subject factor condition (agency vs. baseline) for effect and action time estimation errors. We also computed one repeated measure ANOVA with the factor condition for all trials in which explicit agency ratings were given (without distinguishing between trials with effect and action estimations). As an additional analysis, we added the factor time interval (three levels: 300, 500, 700 ms) to the analyses.

We calculated a two-sided paired *t*-test for each of the valence and arousal ratings of the stimuli (agency stimuli vs. baseline stimuli). Ratings from the UEQ will again be reported descriptively and compared with a benchmark (Schrepp et al., 2017).

### *3.2. Results*

#### *3.2.1. Data treatment*

We treated our data in the same manner as in Experiment 1. We excluded all trials with a visual angle between gaze position at the time of the color change and the targeted stimulus of $>1^\circ$ (9.29 %), all trials in which the eye tracker was recalibrated and all trials immediately prior to this (0.78 %), and all trials with a delay to target fixation of >10 s (0.35 %). We then computed z-scores for every duration to target fixation of every participant in each condition for each interval and excluded trials with durations of $|z| > 3$ (1.92 %). After that, we excluded trials with early stimulus fixations (<1100 ms; 11.23 %) and all trials in which the wrong type of stimulus was fixated first (3.78 %). In this study, we had no exclusion criterion for implausibly long time estimations, since the estimations could only vary between 0 and 2500 ms (see Task section). As in the previous experiments, we computed z-scores for every time estimation error of every participant in each condition for each interval and excluded trials with estimation errors of $|z|$

> 3 (0.02 %). In total, 24.88 % of all trials were excluded.

### 3.2.2. Estimation errors

*3.2.2.1. Actions.* To analyze time estimation errors of participants' saccades, we included only trials in which participants estimated the time point of their saccade to the target stimulus. We found no significant main effect of condition, $F(1, 39) = 0.22$, $p = .645$, $\eta^2_G = 0.001$ (baseline: $M = .017$ s, $SE = 0.020$; agency: $M = 0.024$ s, $SE = 0.013$; see Fig. 3A). We also found no significant main effect of interval, $F(2, 78) = 2.38$, $p = .100$, $\eta^2_G = 0.007$ (300 ms: $M = 0.007$ s, $SE = 0.017$; 500 ms: $M = 0.023$, $SE = 0.017$; 700 ms: $M = 0.031$ s, $SE = 0.015$). Finally, we found no significant interaction of condition and interval, $F(2, 78) = 0.11$, $p = .897$, $\eta^2_G < 0.001$.

*3.2.2.2. Effects.* To investigate time estimation errors of the perceived color changes following participants' saccades, we only included trials in which participants estimated the time points of the observed color change of the fixated stimulus. Contrary to the previous analysis, we found a significant main effect of condition on effect time estimation errors, $F(1, 39) = 31.42$, $p < .001$, $\eta^2_G = 0.167$, indicating a significantly stronger underestimation of the effect time in the agency condition (mean = −0.240 s, SE = 0.021) compared to the baseline condition (mean = −0.103 s, SE = 0.021, see Fig. 3A). Additionally, we found a significant main effect of interval, $F(2, 78) = 25.25$, $p < .001$, $\eta^2_G = 0.087$ ($\varepsilon = 0.86$), with less pronounced time estimation error values (weaker underestimation) for shorter intervals compared to longer intervals (300 ms: $M = -0.118$ s, $SE = 0.016$; 500 ms: $M = -0.165$ s, $SE = 0.021$; 700 ms: $M = -0.233$ s, $SE = 0.021$; all differences ≥ 0.047 s, all $t(39) \geq 2.91$, all $p \leq .006$, all $p_{adj.} \leq .018$, for all pairwise comparisons, see Table 3).

**Table 3**
Pairwise comparisons of Estimated Marginal Means for effect time estimation errors between all three interval conditions. Bonferroni-adjustments were applied based on 3 tests (p*3).

| Contrast | Difference | SE | df | t | *p* | *p* (adj.) |
|---|---|---|---|---|---|---|
| 300–500 ms | 0.047 | 0.016 | 39 | 2.91 | .006 | .018 |
| 300–700 ms | 0.115 | 0.019 | 39 | 6.06 | <.001 | <.001 |
| 500–700 ms | 0.068 | 0.013 | 39 | 5.15 | <.001 | <.001 |

### 3.2.3. Agency ratings

For the following analysis, only trials in which an agency rating was assessed were included. Nine participants produced at least one empty cell and were thus excluded. As in the previous experiment, we found a significant main effect of condition, $F(1, 30) = 50.44$, $p < .001$, $\eta^2_G = 0.342$, indicating substantially higher ratings in the agency condition (mean = 68.33 %, SE = 3.66 %) compared to the baseline condition (mean = 34.32 %, SE = 4.52 %, see Fig. 3B). Contrary to the previous experiment, we also found a significant main effect of interval, $F(2, 60) = 12.06$, $p < .001$, $\eta^2_G = 0.017$, with higher agency ratings for shorter intervals (300 ms interval: $M = 55.10$ %, $SE = 3.64$ %; 500 ms interval: $M = 51.37$ %, $SE = 3.32$ %; 700 ms interval: $M = 47.50$ %, $SE = 3.41$ %; all differences ≥ 3.73 %, $t(30) \geq 2.00$), all $p \leq .014$, all $p_{adj.} \leq .041$ (for all comparisons see Table 4). We found no significant interaction of condition and interval, $F(2, 60) = 1.78$, $p = .177$, $\eta^2_G = 0.003$.

### 3.2.4. Stimulus ratings, reported task strategies, and UEQ ratings

Stimuli of the agency condition were rated more positively regarding valence compared to the color-changing baseline stimuli (70.51 % [SE = 2.62 %] vs. 56.45 % [SE = 2.63 %]). In contrast to Experiment 1 where only a corresponding numerical tendency was found, this difference was statistically significant, $t(39) = 4.14$, $p < .001$, $d_z = 0.654$. As in the previous experiment, the difference between arousal ratings of the different stimulus types was not significant (agency: 40.02 % [SE = 3.62 %] vs. baseline stimuli: 43.91 % [SE = 3.73 %]; $t(39) = -0.96$, $p = .345$, $d_z = -0.151$). 41.46 % of the participants reported that they timed their saccade to a specific letter for estimating time points, 19.51 % internally counted the letters, 2.44 % internally hummed a melody, 2.44 % relied on their gut-feeling, 26.83 % reported no specific strategy, and 7.32 % reported other time estimation strategies. Participants rated the agency task on six subscales of the UEQ on a scale from −3 to +3. For

**Table 4**
Pairwise comparisons of Estimated Marginal Means for agency ratings between all three interval conditions. Bonferroni-adjustments were applied based on 3 tests (p*3).

| Contrast | Difference | SE | df | t | *p* | *p* (adj.) |
|---|---|---|---|---|---|---|
| 300–500 ms | 3.73 | 1.331 | 30 | 2.8 | .009 | .026 |
| 300–700 ms | 7.595 | 1.797 | 30 | 4.23 | <.001 | .001 |
| 500–700 ms | 3.866 | 1.475 | 30 | 2.62 | .014 | .041 |

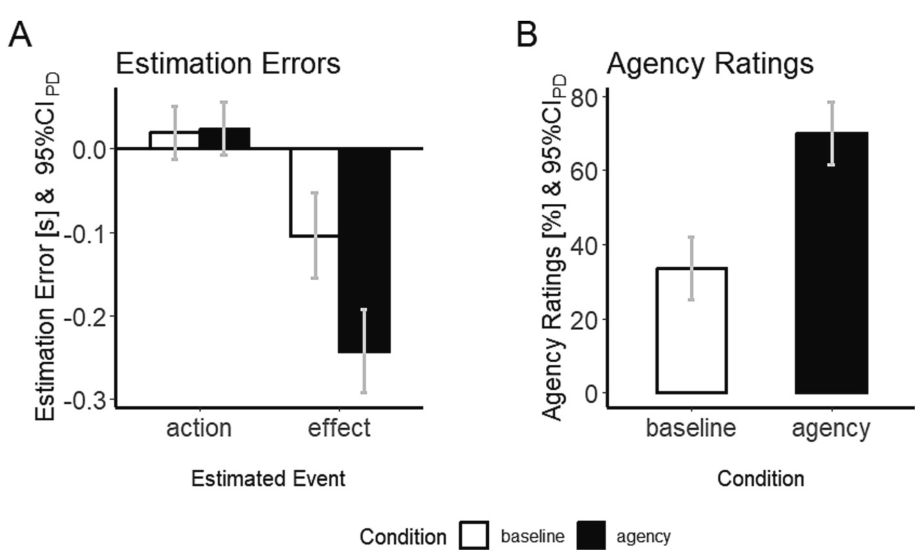


**Fig. 3.** Results of Experiment 2. Mean estimation errors (estimated time point subtracted from true time point) as a function of condition (baseline vs. agency) for action and effect estimation (A). Agency ratings as a function of condition (B). All error bars indicate the 95 % confidence intervals of paired differences (95%CI$_{PD}$) for each comparison of baseline and agency condition (Pfister & Janczyk, 2013).

mean ratings of all subscales and comparison to benchmark data see Table 5.

### 3.3. Discussion

While the explicit sense of agency ratings mirrored the results of the previous experiment, the novel procedure here – using an auditory Libet clock – revealed first evidence for a significant temporal binding effect with respect to the time point estimation of the environmental change (i.e., evidence for effect binding). However, there was no significant indication of temporal binding for the action (i.e., the saccade). This pattern of results is to some degree in line with previous research from other action control domains, where effect binding was reported to be more pronounced than action binding using Libet-like procedures, especially when presenting visual action effects (Tanaka et al., 2019). The fact that we did not find corresponding temporal binding effects in the previous experiment may thus potentially be due to the overall lower sensitivity of temporal interval estimation/reproduction procedures compared to Libet clock procedures. In the General Discussion, we will provide several potential explanations for why we found a significant effect binding effect but no action binding effect in this experiment.

## 4. General discussion

The aim of the present study was to test whether sense of agency can be ascribed to eye movements by transferring typical experimental setups of the sense of agency literature to the oculomotor domain. Participants in each experiment were exposed to an experimental condition, in which they knew that their saccade would elicit a visual perceptual (color) change, and to a baseline condition, in which the visual change seemingly occurred without any relation to a previously executed saccade. In both experiments, we assessed explicit sense of agency via ratings in conjunction with well-established implicit measures of sense of agency, namely temporal binding effects. In Experiment 1, temporal binding was assessed using a temporal reproduction procedure, while in Experiment 2, we utilized an auditory Libet clock procedure.

The main results were as follows: We found a significant indication of sense of agency in the explicit agency ratings in both experiments, demonstrating the effectiveness and validity of our procedure. However, we found no indication of temporal binding, presumably an implicit measure of sense of agency, in the first experiment using an interval reproduction procedure. However, with the Libet clock procedure implemented in Experiment 2 there was evidence for effect binding, albeit not for action binding. In the following, we will discuss potential reasons of why we found no significant temporal binding effect in the first experiment, and why there was only evidence for effect (but not for action) binding in Experiment 2.

One of the most significant differences between Experiment 1 and Experiment 2 is the procedure used for time estimation. In the first experiment, participants reproduced the interval between fixation onset and the observed color change, while in the second experiment they estimated the time points of their fixation onset or the time point of the observed color change with an auditory Libet clock (Libet et al., 1983; Muth et al., 2020). The Libet clock procedure has previously been found to produce significantly larger effect sizes than interval estimation procedures (Tanaka et al., 2019). One might thus conclude that the auditory clock in our study might have been more sensitive to detect subtle temporal binding effects. Furthermore, in a recent study, Siebertz and Jansen (2022) suggested that interval estimations (or reproductions) and Libet clock procedures can produce unrelated effects and might therefore in fact be driven by different underlying cognitive processes.

The absence of evidence for temporal binding effects in the first experiment might also be due to a general methodological problem associated with previous research on temporal binding as an implicit sense of agency measure. Specifically, many occurrences of temporal binding effects in previous research and in particular their presumable relation to sense of agency might have been due to procedural peculiarities that were not sufficiently controlled for: For example, some previous studies on temporal binding compared performance in an agency condition with a baseline condition that did not only differ in the presence/absence (or degree of) agency, but also with respect to the presence or absence of an action or an effect, or more generally with respect to the number of events occurring within a trial. However, all these factors might potentially affect time perception and thus the temporal binding effects in these studies cannot clearly be ascribed to the agency manipulation. In a manual agency study we recently conducted with a similar paradigm as in Experiment 1, we showed that these procedural confounds indeed had a significant influence on typical temporal binding findings, and that these findings can disappear if one controls for these confounds (at least for experiments using the interval estimation procedure, Gutzeit et al., 2023). In our present study, we utilized a more controlled baseline condition by keeping the number of saccades participants had to perform and the number of events occurring in each trial constant across the agency and baseline conditions. As in most of the previous studies regarding temporal binding this was not the case, temporal binding effects in agency conditions might thus have been overestimated. The absence of evidence for temporal binding effects in our experiments might therefore stem from the elimination of such confounds described above. The questionable association between explicit sense of agency and temporal binding also seems less reliable due to the fact, that only some of the previous studies looking into the association between explicit agency ratings and temporal binding have found positive correlations (Imaizumi & Tanno, 2019; Pyasik et al., 2018), while many other studies have found only extremely small or no correlations at all (Dewey & Knoblich, 2014; Saito et al., 2015; Schwarz et al., 2019).

However, this reasoning does apparently not hold for the occurrence of effect binding found in Experiment 2. In the following, we propose several explanations for this presence of effect binding in the concomitant absence of evidence for action binding.

According to one recently proposed mechanism underlying temporal binding effects, events with low temporal certainty might be perceived as being temporally shifted towards causally linked events with higher temporal certainty (Klaffehn et al., 2021). Transferring this idea to the setup in Experiment 2, one might assume that fixation onset is temporally more predictable (certain) than the color change, as the latter occurred after an unpredictable temporal interval and the former was performed by the participants at a time of their choice. This assumption is supported by the fact that a substantial proportion of subjects (41.46 %) stated (after the experiment) that they had timed their saccade execution precisely on the basis of a specific auditory letter. As a consequence, fixation onset might serve as an 'anchor' for time perception, while the (temporally comparably uncertain) color change is

**Table 5**
Mean UEQ ratings and standard errors. Results are compared to benchmark data of Schrepp et al. (2017).

| Subscale | Mean | SE | Comparison to benchmark | Interpretation |
|---|---|---|---|---|
| Attractiveness | 1.07 | 0.18 | Below average | 50 % of results better, 25 % of results worse |
| Perspicuity | 1.89 | 0.19 | Good | 10 % of results better, 75 % of results worse |
| Efficiency | 0.99 | 0.21 | Below average | 50 % of results better, 25 % of results worse |
| Dependability | 0.99 | 0.20 | Below average | 50 % of results better, 25 % of results worse |
| Stimulation | −0.10 | 0.24 | Bad | In the range of the 25 % worst results |
| Novelty | 0.30 | 0.21 | Below average | 50 % of results better, 25 % of results worse |

then perceived as being shifted in time towards the (temporally more certain) action, eventually resulting in effect binding but not in action binding. However, despite the overall plausibility of this mechanism, it should be kept in mind that there are also systematic temporal distortions following saccades (chronostasis effect, Yarrow et al., 2001) that may to some extent undermine the assumption of relative temporal certainty of the saccades, which in turn might (to some degree) compromise the effectiveness of such a mechanism. Therefore, more in-depth research is needed to specifically address these issues.

Sense of agency in general depends on causal beliefs, as one has to identify one's action as the cause of an observed effect in the first place prior to ascribing agency to the action. In our paradigm, we explicitly induced causal beliefs regarding saccades and subsequent effects by telling our participants that they were able to change the color of a stimulus by looking at it. Of course, exerting an influence on the environment by merely looking at objects should be considered as a rather unnatural phenomenon that usually does not occur in daily life. As a result, this renders our induction of causal beliefs *the* critical factor of the sense of agency experienced by the participants given that they did not have experienced this degree of oculomotor agency before (unlike in social situations, where our own eye movements might indeed affect our social environment, see Riechelmann et al., 2021). The explicit agency ratings in our study showed that our manipulation of beliefs was quite successful. Such causal beliefs, however, showed varying degrees of impact on action and effect binding effects in previous research. For example, Desantis et al. (2011) manipulated the extent to which participants believed that they were responsible for a tone following a keypress by making them believe that the tone was either triggered by themselves or by another person. Similar to the results of our present study, they found no differences between time estimations of actions preceding a tone that was experienced as being caused by their own action (compared to tones that were not causally linked to their own behavior), whereas they still found evidence of effect binding.

Another interesting result from our experiments is the impact of the temporal interval between the saccade and the effect on implicit and explicit agency measures. In Experiment 1, we found a significant and strong effect of temporal interval on time estimation errors. Longer temporal effect delays resulted in more pronounced under estimations of the interval (Experiment 1) or of the occurrence of the effect (Experiment 2), which is in line with numerous previous studies (e.g., Dewey & Knoblich, 2014; Humphreys & Buehner, 2009; Ruess et al., 2017; but see Haggard et al., 2002; Ruess et al., 2018; for review, see Wen, 2019). This can be explained by assuming a general slowing down of the internal clock by experiencing agency, compared to a fixed temporal shift through binding that would lead to a stronger total temporal compression of longer intervals (Wen et al., 2015). Further, a longer interval might induce more noise and uncertainty to the subject's perception, thus leading to stronger multisensory integration (Klaffehn et al., 2021).

Contrary to the effects of temporal binding, the results regarding the effect of temporal interval on explicit agency ratings differed across our experiments. In Experiment 1, we found a trend of decreasing explicit ratings as the temporal interval increased, but this effect was not significant. In contrast, we found a significant effect of the temporal interval on the ratings in Experiment 2. The reason why the longer effect delays in Experiment 1 did not reduce the self-reported agency ratings significantly might be related to the fact that the contiguities between saccades and causally linked effects are not well-learned for participants prior to such specific eye movement tasks. In contrast, human beings might have acquired rather narrow action-effect time windows for manual actions leading to visual effects by, for example, interacting with a keyboard to write a text. When this well-learned time window is exceeded it might lead to lower explicit agency (Wen, 2019). Since it is unlikely that humans have a comparable, strongly learned time window for eye movements and resulting effects, it seems plausible that they still ascribe the visual effect to their action more tolerantly, even if it is delayed by up to 700 ms (especially given our explicit instructions). A similar rationale, however, applies to Experiment 2. Participants would also not have acquired well-established action-effect timeframes that would lead to diminished agency ratings in extended intervals due to the expectation of immediate contiguity. However, we here found significantly decreased ratings with longer intervals. Since we used the auditory Libet clock paradigm in Experiment 2, participants might have paid more attention to the elapsing time after their saccade due to the stream of letters between their action and the subsequent effect. In fact, temporal intervals may have subjectively become elongated for participants due to the interval segmentation through the auditory letters (Kurby & Zacks, 2008). Consequently, the elongation should be most pronounced for the 700 ms interval, as participants always heard two letters between their saccade and the color change, while they heard zero to one letter in the 300 ms interval and always one letter in the 500 ms interval. Thus, the sense of agency ratings might have been affected more strongly in trials with the longest effect delays. In line with this, in the first experiment of the original study establishing the auditory clock, Muth et al. (2020) found lower ratings of authorship when they presented more letters compared to fewer letters within the action-effect interval. In conclusion, while we used the same length of temporal effect delays in both studies, the experience of the interval length of longer delays might have been prolonged in Experiment 2 compared to Experiment 1 due to more pronounced event segmentation. This in turn might have caused agency ratings to diminish more strongly for those longer intervals. Further, the presented letters might have generally drawn more attention to the temporal effect delays. This increased attention to temporal aspects may in turn have led to ratings being more strongly biased by delays in Experiment 2 (Farrer et al., 2008).

Considering the present results in the context of recent research, it appears safe to conclude that temporal action binding and effect binding might (at least partially) be driven by different processes (Wolpe et al., 2013), an assumption that is also supported by a previously reported absence of substantial inter-individual correlations between action binding and effect binding effects (Tonn et al., 2021). Specifically, research suggests that action binding strongly depends on specific, previously learned action-effect associations, whereas more general associations (between two events) appear to be sufficient for effect binding (Tanaka et al., 2019).

It is important, however, to consider that in our study we have not asked participants to estimate the exact time point of the onset of their saccade, but rather the time point of their fixation onset ("at which time did you fixate the target?"), as we reasoned that saccades are too fast to expect participants to be able to report the exact time point of their response initiation. As a consequence, this procedure slightly differs from typical sense of agency research regarding manual actions (e.g., involving keypresses), in which participants usually estimate action onset time (instead of the time of action termination, which is equivalent to fixation onset). The more typical approach in previous (manual) studies might result in participants reporting the time of their action intention or action initiation rather than the time of their action completion (e.g., down-pressing of the button). Any action binding effects in such previous studies might thus have partially also been caused by temporal distortions driven by their action intentions (rather than by the actions themselves). Here, by asking them specifically for the time of their fixation onset (i.e., equivalent to the completion of the action) such effects related to ambiguous instructions should be substantially reduced.

In sum, our results indicate that it is indeed possible to experience sense of agency over effects caused by saccades, at least when using highly suggestive instructions as done here. It is interesting to note that in the present study, the explicit agency results are not directly mirrored by the implicit agency measure. Since we induced strong causal beliefs through our instruction, it is not surprising that the explicit ratings reflect these manipulations. Hence, these findings primarily serve as a manipulation check of the induced causal beliefs. However, since we also found clear effect binding in Experiment 2, we believe these ratings not to be solely driven by demand characteristics induced by our instruction, but to reflect true differences in sense of agency between conditions. To further validate this interpretation, future research might use more subtle instructions or abstain from instructing any causal beliefs at all but rather let participants infer causal relations completely based on their experience during the eye tracking task. Probably, explicit (e.g., rating-based) agency measures should generally be considered as a more valid measure, as the construct "sense of agency" itself (via its terminology) appears to refer to a rather subjective phenomenon that should be directly accessible via introspection. Explicit measures should therefore provide sufficient information about whether persons experience a sense of agency at all (and to what extent), which is relevant for applied questions such as the design of an eye tracking application. Implicit measures, on the other hand, may provide information about different underlying processes for the emergence of sense of agency (Synofzik et al., 2013).

Our present findings have several implications for different areas of application and future research. First, our results can form the basis for deeper insights into sense of agency for eye movements, which in turn can be helpful in the development of eye-tracking applications. This technology plays a vital role for severely disabled patients in communication and participation in everyday life (e.g., Ball et al., 2010; Borgestig et al., 2016; Pasqualotto et al., 2015) and is being integrated into an increasing number of everyday life applications (e.g., Hyrskykari et al., 2005; Majaranta & Bulling, 2014). However, since the ability to influence the environment with eye movements is an unnatural and poorly learned phenomenon, everyday eye-tracking faces several problems with respect to its application. For example, lower controllability has been reported (e.g., the "Midas Touch" Problem, e.g., Jacob, 1991; Majaranta & Bulling, 2014), which in turn might yield lower usability and acceptance (Berberian, 2019). Our results (specifically our usability ratings) indicate a fast learning process regarding the sense of agency for eye movements when causal beliefs are induced through explicit instructions. Note that without explicit instructions, it is difficult to detect contingencies between one's oculomotor behavior and their effects (e.g., Grgič et al., 2016; Wang et al., 2012).

Second, by (partially) replicating typical findings regarding sense of agency in the manual domain, further research is now able to utilize the oculomotor domain and its peculiarities to gather new information regarding more general theories of sense of agency. For instance, the high overall degree of involuntary or routine-based control (e.g. Huestegge et al., 2019; Leigh & Kennard, 2004) of eye movements might be leveraged in future experimental paradigms to further investigate the role of voluntary vs. involuntary actions for sense of agency (similar to studies that induced involuntary manual actions externally, e.g., Haggard et al., 2002; Kirsch et al., 2019). For this purpose, for example, the sense of agency for effect-producing saccades with a high degree of automaticity (e.g., saccades based on the gap-effect, Munoz & Wurtz, 1993; Saslow, 1967) could be compared to the sense of agency for effect-producing anti-saccades that are characterized by a high degree of endogeneous control (e.g., anti-saccades, Munoz & Everling, 2004).

Third, the investigation of sense of agency for visual environment-related effects caused by saccades raises a profound question: can individuals attribute a sense of agency to actions that have not historically yielded observable outcomes (as eye movements, unlike other action domains, are not experienced to elicit effects in the inanimate environment)?Therefore, in our experiments sense of agency could only have been elicited by either a rapid learning process of the action-effect associations or by our explicit instruction and associated causal beliefs (or both). According to basic motor-based forward models such as the comparator-model (Carruthers, 2012; Frith et al., 2000; Wolpert et al., 1995) the sense of agency is elicited by a match between the predict-ed sensory feedback through motor commands and the actual sensory feedback. However, this process of fast modulation – taking only a few minutes in Experiment 2 - is not compatible with relatively slow Hebbian learning processes of action-effect outcomes (Horváth, 2015). The significant temporal binding effect in Experiment 2 thus indicates a more complex and flexible cognitive model underlying these effects, incorporating fast adaptations driven by higher cognitive processes (Dogge et al., 2019). This is even more relevant in our novel paradigm in which we incorporated a baseline condition that yielded exactly the same basic action-effect associations: a saccade followed by a color change after a certain delay. Purely associative models could not explain the temporal binding differences, so the differences between conditions must be caused by higher cognitive processes (i.e., causal beliefs). This reasoning can also be extended to postdictive models (Wegner, 2003), as these models also require some previously acquired action-effect cues for experiencing agency (Linser & Goschke, 2007). Our present results regarding temporal effect binding for entirely new acquired action-effect associations thus further emphasize the adaptability of cognitive models that exceed purely associative motor-based forward models. Note that this assumption of flexibility and adaptability of the underlying processes is well in line with recent literature (Dogge et al., 2019; Synofzik et al., 2008).

Another interesting topic for future research could be the relation between sense of agency for saccades and other implicit agency measures. For example, there are a large number of studies on sense of agency for manual actions that use sensory attenuation as an implicit agency measure (Blakemore et al., 1999; Brown et al., 2013; Hughes et al., 2013; Klaffehn et al., 2019; Weller et al., 2017; etc.). A paradigm utilizing sensory attenuation to implicitly investigate sense of agency for saccades might deliver further insight into the specificities of sense of agency for saccades. In addition, it might also be possible to assess implicit sense of agency behaviorally via prediction errors (Perrykkad et al., 2021). However, at this point one should again be aware of the possibility that explicit agency measures might generally be considered as more valid, as the term *sense* of agency by itself implies a rather subjective, introspectively accessible phenomenon (see above).

## 5. Conclusion

In the present study, we successfully induced different causal beliefs between two experimental conditions (triggered by respective instructions), hence manipulating sense of agency. We found clear evidence for experienced sense of agency in the domain of oculomotor control that is (at least partially) comparable to effects reported in other action domains. This indicates similar basic mechanisms underlying the experience of oculomotor action and action in other, more commonly addressed domains (e.g., manual). However, temporal binding results (that are typically discussed as a potential implicit measure of sense of agency) were somewhat divergent, as we only found evidence for effect binding under specific conditions. This indicates a less reliable connection between temporal binding and sense of agency than previously thought, which is also in line with more recent research. Probably, explicit agency ratings might be the more valid measure for assessing the presence and the degree of sense of agency in general. The present results may have implications for other fields in which oculomotor behavior more directly affects the environment, such as in corresponding human-machine interaction scenarios (Majaranta & Bulling, 2014) or in the context of social interactions (social agency), where we may utilize our own eye movements for the sake of controlling the others' social attention (e.g., Brandi et al., 2019; Riechelmann et al., 2021). Moreover, the results of this paper provide strong evidence for the idea that sense of agency can emerge for type of action-effect associations that were never previously experienced. This challenges more rigid, purely associative agency models and suggests more complex and flexible cognitive models underlying the emergence of sense of agency.

**Declaration of competing interest**

We have no known conflict of interest to disclose.

## Data availability

Raw data, analysis scripts, PsychoPy scripts, and stimuli materials are available on https://osf.io/t8pnx/.

## Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.actpsy.2023.104121.

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