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Something from nothing: Agency for deliberate nonactions

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ABSTRACT

Several law systems punish nonactions such as failures to render assistance, although it is unknown if people spontaneously experience a sense of authorship for the consequences of their not acting. Here we provide evidence that events caused by deliberate choices not to act can indeed give rise to a vivid sense of agency. In three experiments, participants reported a sense of agency for events following nonactions and, crucially, temporal binding between nonactions and subsequent consequences suggested a sense of agency for nonactions even at an implicit level. These findings indicate that a sense of agency is not confined to overt body movements. At the same time, agency was more pronounced when the same event resulted from an action rather than being the consequence of a nonaction, highlighting the importance of ascribing different degrees of responsibility for the consequences of acting and not acting.

1. Introduction

Originating from Roman law, the concept of *mens rea* holds that humans are liable for the foreseeable consequences of their doing. This principle is deeply rooted in the vivid phenomenal experience of authorship for one's own actions and their consequences (Haggard, 2017). The sense of agency comprises the feeling of being in control of sensory events through one's own actions, and abnormal experiences of agency are associated with severe mental illnesses, such as passivity symptoms or delusions of control in schizophrenic patients (Blakemore, Smith, Steel, Johnstone, & Frith, 2000; Franck et al., 2001; Moore, 2016). These are also cases in which most law systems deny to lend agents full responsibility.

Sense of agency arises, and has been studied so far, in situations where humans perform motor actions to produce changes in their environment. However, not acting can have severe consequences as well, e.g., when not helping a person in danger. In many continental European countries, but less often in English-speaking countries, a decision not to render assistance to a person in need can even constitute a criminal offense (France: article 223-6, Code pénal; Germany: §323c Strafgesetzbuch; Spain: artículo 195, Código penal). But is it justified to ascribe a person a sense of agency for the consequences of their not acting? And if so, how does agency for the consequences of not acting compare to agency for the consequences of overt acting?

Several conceptual differences arise between actions and nonactions, and some of these differences have been discussed extensively in philosophical discourse. For example, when something is happening because no one intervened, then the question arises: Who among those who did not act is to blame for the consequences of not acting? Key variables that have been highlighted in these philosophical accounts are moral obligation, informational directness (are potential consequences evident and directly perceivable?) and efficaciousness (would my actions be more or less effective than the actions of other potential agents? e.g., Fischer, 2007; McGrath, 2005; Willemsen, 2018).

In addition to such differences of responsibility among several (non) actors, there might be differences between acting and non-acting at the level of cognitive processing of the individual agent. In fact, there is reason to believe that agency mainly emerges for actions and that it is severely decreased and qualitatively different for deliberate nonactions (i.e., conscious decisions not to act). Such a proposal follows from typical two-process accounts of human agency which distinguish between predictive processes and postdictive attributions (Haggard, 2017; Synofzik, Vosgerau, & Newen, 2008). For predictive accounts, a sense of agency emerges if actual action consequences match those that had been predicted prior to the act. Such predictions have been proposed to be tightly linked, both functionally and temporally, to the generation of own efferent activity (Engbert, Wohlschläger, & Haggard, 2008; Haggard, Clark, & Kalogeras, 2002). Obviously, this type of input would not be available for nonactions, thus limiting the potential to induce a sense of agency relative to action-consequence sequences. Yet, recent findings suggest that nonactions are represented and controlled in a fundamentally similar fashion as actions, drawing on stable associations between decisions in favor of not acting and the resulting sensory consequences (Kühn, Elsner, Prinz, & Brass, 2009; Weller, Kunde, &

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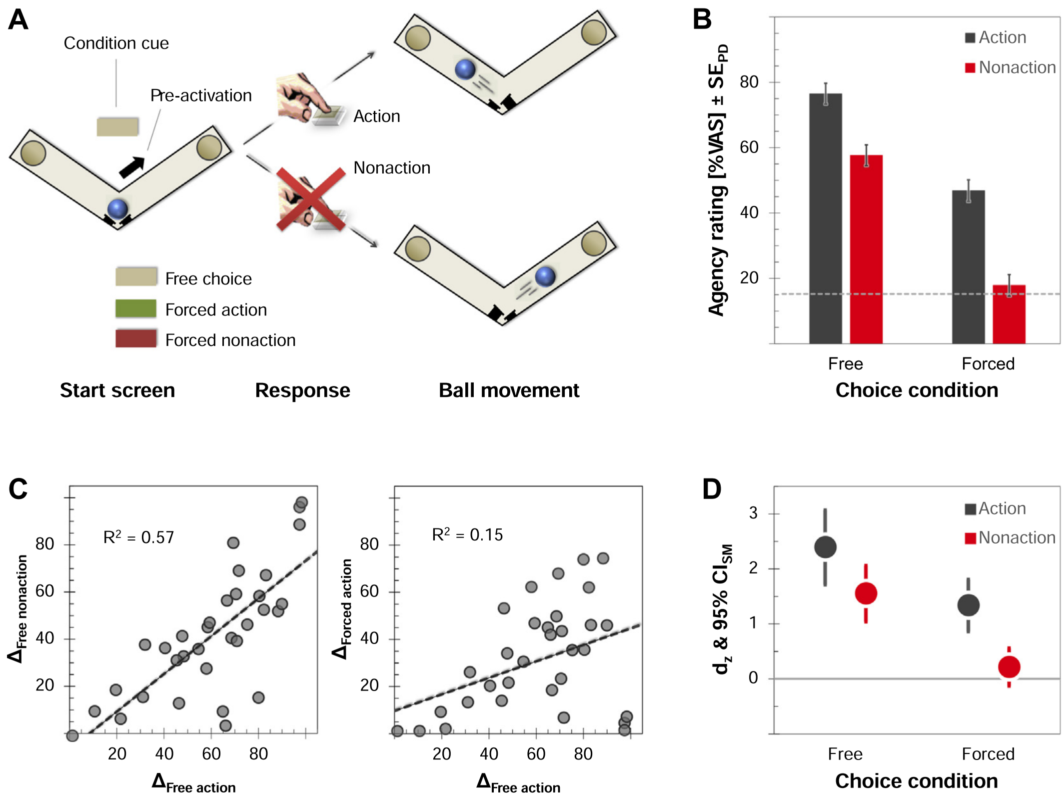

Fig. 1. Setup and results of Experiment 1. (A) Design of the pinball machine that launched a ball either into the left or into the right arm on each trial. Participants were informed about the current default direction by an arrow symbol (pre-activation). Pressing a key changed the direction of the ball whereas not responding caused the ball to shoot in the pre-activated direction. A color cue indicated the current condition, i.e., free choice (action vs. nonaction), forced action, or forced nonaction. In an additional baseline condition, the pinball machine started before onset of the condition cue. (B) Mean agency ratings on a visual analog scale (VAS) as a function of choice condition (free vs. forced) and response type (action vs. nonaction). Agency ratings for the baseline condition are displayed as a dashed line. Error bars indicate standard errors of paired differences (SE_{PD}) for each comparison of action and nonaction (Pfister & Janczyk, 2013). (C) Bivariate correlations for free choice actions and free choice nonactions (left plot) as well as free choice actions and forced choice actions (right plot). Values on the axes indicate difference measures (Δ) of each participant's mean rating to the individual baseline rating. (D) Effect size estimates d_z and corresponding 95% confidence intervals for standardized means (CI_{SM}). (The reader is referred to the web version of this article for colored figures.)

Pfister, 2017). Based on these observations, a sense of agency for the consequences of nonactions might emerge similarly to the case of deliberate actions. A decision between both hypotheses can be established experimentally; this is what we aim at here with a set of three complementary studies.

2. Experiment 1: subjective ratings

To capture agency for nonactions, we set up an experiment where participants could control the function of a virtual pinball machine (Fig. 1A). In each trial, the pinball machine launched a ball either to the left or the right depending on the participants' choosing, and the participants' task was to launch the ball about equally often in each direction. The machine came with a default direction that resulted when participants refrained from acting and which changed from trial to trial. Participants could change direction by performing a keypress action or stay with the pre-activated direction by deciding not to press the key. Shortly after an action or nonaction, the ball was launched in the corresponding direction. Across trials, we manipulated whether participants could freely choose between acting and not acting, or whether they were forced to perform either an action or a nonaction in a full within-subjects design.

The influence of free as compared to forced choice on human action control has been discussed extensively in previous research (Hommel, Lippelt, Gurbuz, & Pfister, 2017; Janczyk, Nolden, & Jolicoeur, 2015; Keller et al., 2006; Pfister, 2019). For explicit judgments of agency, most studies found that the opportunity to freely select an action among multiple possibilities increased agency, presumably mediated by participants' control believes or low-level influences of action-selection processes (see e.g., Liesner, Kirsch, & Kunde, in press; Schwarz, Weller, Klaffehn, & Pfister, 2019; Sebanz & Lackner, 2007; Sidarus & Haggard, 2016). Arguably, the distinction of free and forced choices has even stronger implications in the case of nonactions as freely chosen nonactions are likely based on a more explicit decision not to act and seem to be associated more readily with the subsequent consequences (Kühn et al., 2009). This finding suggests that agency should be especially pronounced for freely chosen nonactions.

To assess agency for participants' action and nonactions, we asked participants how strongly they felt as causal agent for the ball movement. Agency ratings for free and forced actions and nonactions were compared to a baseline condition, in which participants could neither control nor foresee the ball movement. The baseline condition therefore assessed the level of agency in the absence of a decision (not) to act, as compared to decisions in favor or against acting. To validate the results of the agency question, we further asked participants for their perceived responsibility for the outcome of each trial (see the Supplementary material for details).

2.1. Methods

Stimulus materials, computer programs, raw data and analysis scripts are available on the Open Science Framework (https://osf.io/kmwfq/).

2.1.1. Participants

We tested 34 participants (mean age: 21.0, *SD* = 3.9; 2 male; 4 left-handed). This sample size was based on an a priori power analysis with a medium effect size ($d_z = 0.5$) and a power of 1-β = 0.8. All participants gave informed consent prior to the experiment and received either course credit or monetary compensation for participation. One participant was excluded from the analyses because he or she misunderstood the instructions.

2.1.2. Apparatus

Participants sat in front of a 22″ flat screen and used the C key of a standard German QWERTZ keyboard to respond. Stimuli were presented on a black background and a V-shaped pinball machine appeared in the screen center (see Fig. 1A). Participants controlled a ball and could launch the ball into the left or the right arm of the machine by deciding whether to press the response key or not. Each arm had a hole where the ball could fall into after being shot in the corresponding direction. Each ball movement took 375 ms and was continuously animated. Red arrows were presented directly above the arms to indicate the pre-activated direction in the current trial. Imperative stimuli (colored rectangles in green, yellow, and red) as well as the agency and responsibility questions were presented in the horizontal center of the screen above the pinball machine. The agency question was "How strongly did you feel as causal agent for the ball movement to the left/right?" (German original: "Wie sehr hast du dich als Verursacher der Ballbewegung nach links/rechts gefühlt?"). The responsibility question was "How strongly did you feel responsible for your own (non)action?" (German original: "Wie sehr hast du dich gerade verantwortlich für deine (Nicht)Handlung gefühlt?"). Participants could respond with the mouse on a visual analog scale (VAS) ranging from 0 ("a little") to 100 ("a lot").

2.1.3. Procedure

Each trial started with the ball in rest. In all trials, except for the baseline trials, an arrow indicating the pre-activated direction was then shown for 500 ms. If participants decided not to press a key, the ball would be launched in that direction. Then, a colored rectangle appeared (either red, green, or yellow). In case of a green rectangle, participants were to press the response key (forced action). In case of a red rectangle, participants were to not press the response key (forced nonaction). A yellow rectangle indicated that participants could freely decide whether to press the response key (free action) or not (free nonaction). If participants pressed the response key, the ball was launched in the opposite direction of the pre-activation 50 ms after the keypress. To determine when participants decided not to press the response key, a nonaction interval was calculated using participants' response time (RT) history. At the beginning of the experiment this interval was set to 1500 ms because no RT history was available yet, while it was calculated as interval length = (mean RT + mean RT + last RT)/3 + 300 ms for the remainder of the experiment (following previous nonaction studies; Kühn et al., 2009; Weller, Kunde, & Pfister, 2017). A nonaction was registered if the key had not been pressed in this time window and 50 ms later the ball was launched in the pre-activated direction. In baseline trials, no pre-activation arrows and imperative stimuli were presented. Instead, at the beginning of the trial the ball was immediately launched in a randomly determined direction (see Fig. S1 in the Supplementary material for a full timeline of each trial type).

Participants were instructed to try to keep the number of ball movements to the left and right about equal within one block, whenever they had the chance to freely choose the direction of the ball. To that end, the number of launches to the left and right was displayed above the left and right arm of the pinball machine.

In case of errors of the participant, an error message was displayed for 1000 ms and the trial was aborted. This included errors of commission (if the participant responded incorrectly in the forced choice trials) and trials in which participants pressed a key during the ball movement. At the end of each block, participants were informed about the number of errors, as well as the number of ball movements to the left and the right in the previous block.

The experiment consisted of 14 blocks with 36 trials each with an inter-trial interval of 2000 ms. Within a block, eight trials were forced action trials, eight trials forced nonaction trials, two trials were baseline trials, and eighteen trials were free choice trials (the number of actions and nonactions in the free choice trials depended on participants' choices). Most trials did not contain a question and ended directly after the ball had vanished into one of the holes. In the remaining trials, the agency or the responsibility question was presented 500 ms after the ball movement had ended and remained on the screen until participants responded. Only one type of question was presented within one block (the agency question in eight blocks and the responsibility question in six blocks), and the order of blocks was determined randomly. Participants were informed about the current question type before a block started. The questions were presented in two randomly selected trials of the forced action and the forced nonaction trials and in six randomly selected free choice trials (equally often for ball movements to the left and the right if possible). Furthermore, the agency question was presented in the two baseline trials. The responsibility question was not presented in baseline trials because there was no action or nonaction.

2.1.4. Statistical analysis

Errors trials were excluded (4.5%), comprising trials with the wrong response in the forced choice condition (5.7% of the forced choice trials) and trials in which keypresses occurred after the ball movement had already been initiated (2.0% of all trials).

As a first analysis, we compared agency ratings for each of the four action and nonaction conditions to the baseline via two-tailed, paired *t*-tests. Corresponding effect sizes were calculated as $d_z = t/\sqrt{n}$. We then compared mean agency and responsibility ratings by repeated-measures analyses of variance (ANOVAs) with the factors choice (free choice vs. forced choice) and action type (action vs. nonaction). As follow-up analyses, we calculated bivariate correlations across participants and tested these correlations against zero with a two-tailed *t*-test.

2.2. Results

Agency ratings for free nonactions were higher than in the baseline condition, as were ratings for free and forced actions, *ts* > 7.70, *ps* < .001 (see Fig. 1B–D). In contrast, agency ratings for forced nonactions did not differ from baseline, $t(32) = 1.25$, $p = .220$, $d_z = 0.22$. Agency ratings were generally higher for free compared to forced (non) actions, $F(1,32) = 65.86$, $p < .001$, $\eta_p^2 = 0.67$, and higher for actions compared to nonactions, $F(1,32) = 63.59$, $p < .001$, $\eta_p^2 = 0.67$. These main effects interacted, $F(1,32) = 16.81$, $p < .001$, $\eta_p^2 = 0.34$, and separate pairwise comparisons showed that agency ratings for forced nonactions (17.9%VAS ± 2.2) were especially low compared to all other conditions (all *ts* > 8.79, *ps* < .001; see Table S1 in the Supplementary material). In contrast, agency ratings for free nonactions (57.7%VAS ± 3.7) were lower compared to free actions (76.5% VAS ± 2.5), $t(32) = 5.92$, $p < .001$, $d_z = 1.03$, but not compared to forced actions (46.8%VAS ± 3.8), $t(32) = 1.73$, $p = .093$, $d_z = 0.30$.

A strong correlation of the baseline-corrected agency ratings for free nonactions and those for free actions emerged, $r = 0.754$, $t(31) = 6.40$, $p < .001$. In contrast, baseline-corrected agency ratings for free and forced actions were correlated less strongly, $r = 0.384$, $t(31) = 2.32$, $p = .027$ (see Table S2).

Responsibility ratings revealed a converging pattern of results. That is, participants felt more responsible for free choice compared to forced choice actions and nonactions, $F(1, 32) = 81.52$, $p < .001$, $\eta_p^2 = 0.72$, and more responsible for actions compared to nonactions, $F(1, 32) = 36.83$, $p < .001$, $\eta_p^2 = 0.54$ (see Supplementary Text, Fig. S2, and Tables S1 and S3 in the Supplementary material, for descriptive statistics and detailed analyses). Responsibility and agency ratings were further correlated with each other for free actions, $r = 0.844$, $t(31) = 8.76$, $p < .001$, free nonactions, $r = 0.679$, $t(31) = 5.14$, $p < .001$, forced actions, $r = 0.587$, $t(31) = 4.03$, $p < .001$, and forced nonactions alike, $r = 0.735$, $t(31) = 6.03$, $p < .001$.

2.3. Discussion

The results of Experiment 1 suggest that a sense of agency indeed emerges for the consequences of not acting if nonactions can be freely chosen whereas agency for forced nonactions did not differ notably from baseline. This finding supports accounts that highlight a prominent role of freedom of choice for agency (Borhani, Beck, & Haggard, 2017) and suggests that the distinction of free versus forced choice conditions is especially relevant in the case of nonactions. These findings further address potential limitations of the present experimental design by indicating that participants did indeed form deliberate decisions not to act at least in the free choice condition. Moreover, agency for freely chosen nonactions seems to be related to agency for actions, as suggested by the strong correlation of the corresponding agency ratings. Responsibility ratings mirrored the results of the agency questions and correlation analyses further indicated a strong relation of these two ratings.

Directly asking participants about their sense of agency could, however, introduce specific confounds, e.g., due to beliefs about the purpose of the question (Haggard & Tsakiris, 2009). The sense of agency is therefore often investigated using implicit markers of agency, such as the temporal binding effect (Haggard et al., 2002). This measure makes use of the fact that a voluntary action and its subsequent effect are temporally drawn towards each other, which results in a perceived compression of the temporal interval between an action and the resulting effects. This compression effect is evident for intentional actions, but not for passive or involuntary actions (Engbert, Wohlschläger, & Haggard, 2017; Haggard & Clark, 2003). Temporal binding is thus assumed to be an implicit marker of the sense of agency (Borhani et al., 2017; Moore & Obhi, 2012), and Experiment 2 targeted whether a decision not to act would indeed yield temporal binding.

3. Experiment 2: temporal binding

To assess an implicit proxy for subjective agency, we measured temporal binding for actions and nonactions in a pre-registered experiment that employed a classical temporal judgment task (Fig. 2A; cf. Haggard et al., 2002; Moore & Haggard, 2008; Ruess, Thomaschke, & Kiesel, 2017). Participants thus observed a rotating clock hand to judge the timing of events, and they could choose between acting and not acting. Each choice would result in a distinct and consistent tone effect after a short action-effect interval. We assessed the participant's perceived time of tone onset and compared these estimates to a baseline condition, in which the tones could not be controlled. Temporal binding should be evident in an earlier tone perception if tones result from an action or a nonaction compared to the baseline condition. We further assessed agency ratings for these tone effects to replicate the observations of Experiment 1.

3.1. Methods

The experimental setup and data analysis of Experiment 2 were preregistered on the Open Science Framework (https://osf.io/nzhrk) and materials are provided in the same repository as for Experiment 1.

3.1.1. Participants

We recruited 34 participants (mean age = 21.2, $SD = 2.2$; 6 male; 4 left-handed). As there was no prior indicator of the effect size of temporal binding for nonactions, a medium effect size ($d_z = 0.5$) was assumed. As for Experiment 1, an a priori power analysis suggested 34 participants to detect such an effect with a power of $1\text{-}\beta = 0.80$. This also ensured a high power of $1\text{-}\beta > 0.99$ to replicate the comparison of agency ratings for free nonactions to baseline according to the effect size observed in Experiment 1.

3.1.2. Apparatus

Participants sat in front of a 17″ monitor of a standard desktop computer and watched a white clock face presented centrally on a black background (6 cm diameter). The clock hand took 2560 ms for a full rotation and participants were instructed to use this clock for estimating the time of tone presentation. One full rotation was labeled as 60 “minutes” and every five “minutes” (5, 10, 15…) were tick-marked on the clock face. The sound stimuli in the experiment were a high (600 Hz) and a low (300 Hz) sinusoidal tone of 100 ms duration, which were presented via headphones.

Participants used the V key of a standard German QWERTZ keyboard with the index finger of the left hand to produce the sound effects and entered their estimated time of tone presentation using the number keys of the keyboard. The high and low tones were used as action and nonaction effects, respectively. The mapping of tone and response was constant for each participant but was counterbalanced across participants. The agency question for (non)action tones read “How strongly did you feel as causal agent for the tone in the current trial?” (German original: “Wie sehr hast du dich gerade als Verursacher des Tones gefühlt?”). Participants responded with the mouse on a VAS as in Experiment 1.

3.1.3. Procedure

The experiment consisted of two conditions, a baseline and an operant condition, presented in different blocks. Each trial started with the display of the clock face with the hand in upright position, and the clock hand immediately started rotating. On operant trials, participants could freely choose whether to press or not to press a response key (see Fig. 2A), and actions as well as nonactions predictably triggered a distinct effect tone. If participants pressed a key, the action tone was played 300 ms after the keypress. Participants were instructed to wait for at least half a rotation of the clock hand before pressing a key and they were instructed to refrain from using strategies such as pressing the key at a predetermined position of the clock hand. To compute the time of the nonaction effect presentation, the participant's RTs for actions (time between presentation of the clock face and a keypress) were used when they exceeded 1200 ms and the time of nonaction effect presentation was computed for each trial as (mean RT + mean RT + last RT)/3 + 600 ms. This approach was similar to Experiment 1 with slight adjustments to prevent a presentation of the nonaction effect directly after trial start. In baseline trials, participants could not control the tones and one of the two tones was presented at a randomly chosen time between 750 and 5120 ms after trial start, sampled from a uniform distribution. After tone onset the clock hand kept rotating for another 2000 to 3000 ms. Then, the clock face disappeared, and participants were asked to enter the time of tone presentation in minutes or to answer the agency question.

Baseline and operant trials were presented in different blocks. The experiment consisted of eight blocks in total, four blocks of each condition. Blocks of different conditions alternated (ABABABAB) and the order of conditions was counterbalanced across participants. The baseline blocks consisted of 26 trials each. In two of these trials, the time estimation was replaced and participants had to rate their sense of agency as in Experiment 1. The experimental blocks consisted of 30 trials, of which six trials featured the agency question instead of the time estimation task. Before the actual experiment, participants were familiarized with the clock hand and practiced time estimation for six baseline trials and six operant trials.

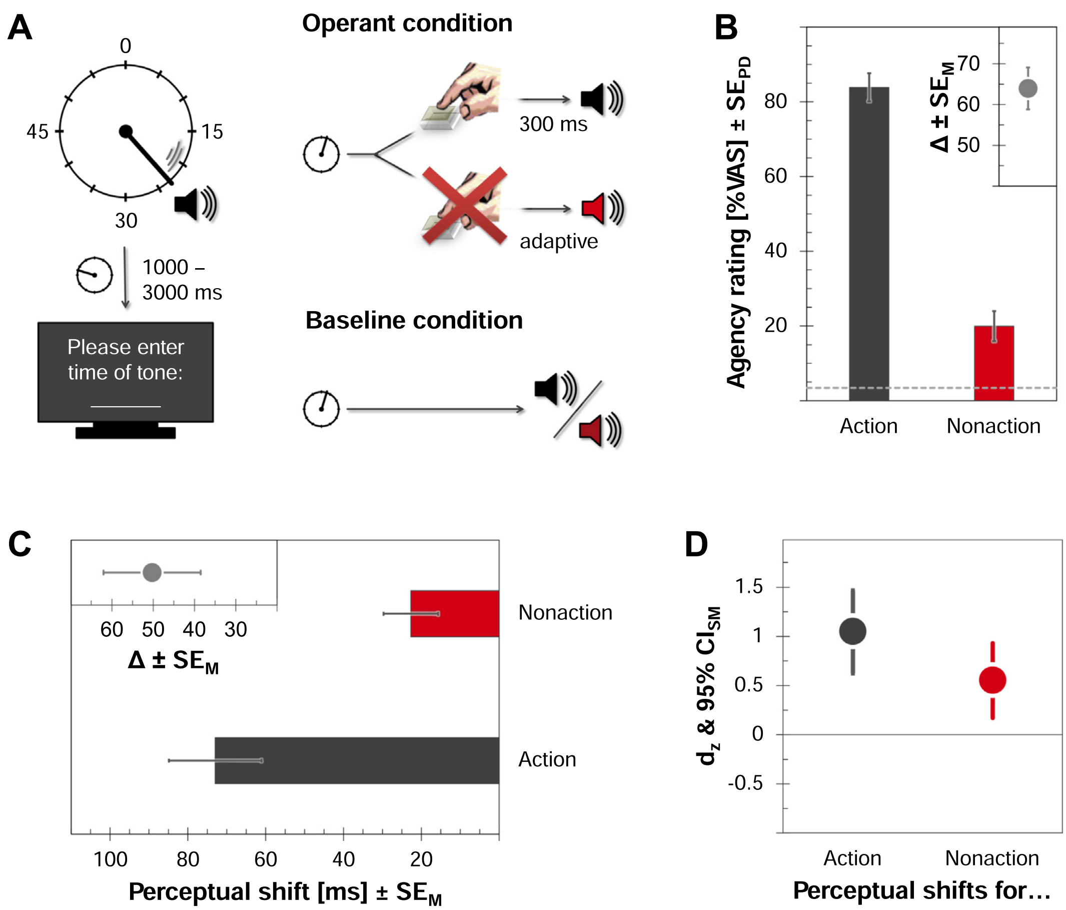

Fig. 2. Experimental procedure and main results of Experiment 2. (A) Participants observed a rotating clock hand on the computer screen (angular velocity: one turn every 2560 ms). In the operant condition, they could freely choose between acting (pressing a key) or not acting in a given time interval. Actions and non-actions produced distinct tone effects (see Methods as well as the Supplementary material for details regarding effect timing). The rotation stopped after a random time (1000–3000 ms) after tone onset and the participants were to estimate the time of tone representation. In the baseline condition, one of the two tones was played at a random point in time during the trial. (B) Mean agency ratings on a visual analog scale (VAS) for actions and nonactions. Agency ratings for the baseline condition are displayed as a dashed line. Error bars indicate standard errors of paired differences (SE_{PD}) for the comparison of action and nonaction with the baseline condition (Pfister & Janczyk, 2013). The inset shows the mean pairwise difference between the ratings of both conditions (Δ) accompanied by its standard error. (C) Temporal binding as assessed via perceptual shifts between the estimates of the operant condition and the baseline condition. Error bars indicate standard errors of the mean. The inset shows the mean pairwise difference between the perceptual shifts of both conditions (Δ) accompanied by its standard error. (D) Effect size estimates d_z for temporal binding and corresponding 95% confidence intervals for standardized means (CI_{SM}).

3.1.4. Statistical analysis

Trials with errors were excluded from all analyses (i.e., keypresses after a nonaction was recorded; 1.1% of all trials). For each participant and condition, the mean time estimation error was computed as participant's estimation of tone presentation minus the actual time of tone presentation. For the analysis of temporal binding, trials with estimation errors that deviated > 2.5 standard deviations from the cell mean were excluded, calculated separately for each participant and condition (1.9%). Mean estimation errors were compared with two-tailed, paired *t*-tests.

3.2. Results

Tones triggered by an action were judged to occur earlier as compared with externally triggered tones of the baseline condition (73 ms ± 11.9), $t(33) = 6.13$, $p < .001$, $d_z = 1.0$, as were tones triggered by a choice to not act (23 ms ± 7.0), $t(33) = 3.24$, $p = .003$, $d_z = 0.56$ (Fig. 2B–D, Table S4). The binding effect for actions, however, was more pronounced than for nonactions, $t(33) = 4.27$, $p < .001$, $d_z = 0.73$.

Ratings differed from baseline for nonactions (Δ16.5%VAS ± 4.0, see Table S4), $t(33) = 4.13$, $p < .001$, $d_z = 0.71$, and actions alike (Δ80.5%VAS ± 3.8), $t(33) = 21.34$, $p < .001$, $d_z = 3.66$. Agency ratings for actions were also higher than for nonactions, $t(33) = 12.49$, $p < .001$, $d_z = 2.14$.

As a follow-up analysis, we further calculated the correlation between agency ratings and binding (i.e., the perceptual shifts) for actions and nonactions, respectively. Correlations between these two measures were small and unreliable for actions and nonactions alike, $r = 0.220$, $t(32) = 1.27$, $p = .212$ and $r = -0.020$, $t(31) = -0.11$, $p < .912$, respectively, though visual inspection of the data suggested that this pattern was driven by ceiling effects in the case of free actions (for similar findings in settings without such ceiling effects, see Dewey & Knoblich, 2014; Saito, Takahata, Murai, & Takahashi, 2015; Schwarz et al., 2019).

3.3. Discussion

Tones resulting from a nonaction were temporally drawn towards the triggering nonactions as compared with the baseline condition, even though the physical stimulation was identical in these conditions. This finding indicates that participants spontaneously perceived the nonaction tone to be a sensory consequence of the nonaction also at an implicit level, while explicit agency ratings replicated the pattern observed in Experiment 1.

In line with previous findings, action effects were also drawn towards the triggering action, and temporal binding was stronger for actions as compared to nonactions. Notably, however, tone effects for actions were perfectly predictable in terms of identity and timing, whereas temporal predictability was reduced for tone effects following nonactions. This methodological difference may bias the experimental design to favor the present observation of less temporal binding for nonactions as compared with actions (Hughes, Desantis, & Waszak, 2013; Kirsch, Kunde, & Herbort, 2019). To address this potential confound we conducted Experiment 3, in which we paralleled the predictability in time and identity for effects of actions, nonactions, and the respective baseline condition as closely as possible.

4. Experiment 3: predictability

Experiment 3 used a modified version of the pinball setup of Experiment 1 to control the timing of action and nonaction effects. Temporal binding was assessed with direct interval estimates, i.e., participants were directly asked to estimate the delay of the (non)action effect instead of relying on the clock procedure of Experiment 2 (Engbert et al., 2008; Engbert, Wohlschläger, Thomas, & Haggard, 2007; Pfister, Obhi, Rieger, & Wenke, 2014; Wenke & Haggard, 2009). Experiment 3b was a close replication of Experiment 3a with minor adjustments to counter participant drop-out.

In each trial, the pinball machine launched a ball either to the left or the right depending on the participants' choosing. Participants could choose between a nonaction, which would launch the ball into a pre-activated direction, and an action, which would launch the ball into the opposite direction. Importantly, participants had to indicate their choice at the beginning of each trial. Following this decision, participants had to wait a certain time which was indicated by a progress bar. If participants had chosen an action, they were allowed to press the action key as soon as the progress bar was filled completely. Shortly after participants' keypress, the ball launched and participants had to indicate the interval between keypress and launch. If participants had chosen a nonaction, they heard a clicking sound when the progress bar was filled completely and shortly afterward the ball was launched into the pre-activated direction. Participants had to indicate the interval between clicking sound and ball launch. Interval estimates in these conditions were compared to a baseline condition, in which participants could not choose the direction of the ball movement; instead, the ball was launched into the pre-cued direction without participants' involvement. The timing of the movement was again indicated by a progress bar as for nonaction trials in the experimental conditions. Importantly, the delays between the participants' keypress action and ball launch were identical to the delays between clicking sound and ball launch for nonactions and the baseline condition. Thus, the ball movement could be predicted equally precisely in all conditions. Temporal binding should be evident in terms of shorter interval estimates for action and nonactions compared to baseline, whereas increased agency for actions should be evident in shorter estimates for actions than for nonactions. Agency ratings were assessed as in Experiment 1.

4.1. Methods

The experimental setup and data analysis of Experiment 3 were pre-registered on the Open Science Framework (Exp. 3a: https://osf.io/y9mn8; Exp. 3b: https://osf.io/ucwpq). Corresponding materials are provided in the same repository as for Experiment 1 and 2.

4.1.1. Participants

To determine the sample size of Experiment 3a, we assumed a medium effect size ($d_z = 0.5$) as a slightly more conservative criterion than the effect size of $d_z = 0.56$ observed in Experiment 2. An a priori power analysis suggested 34 participants to detect such an effect with a power of 1-β = 0.80. Thus, we recruited 34 participants for Experiment 3a (mean age = 28.4; *SD* = 11.2; 28 female, 2 left-handed). Following the pre-registered analysis plan, the data of three participants was replaced because of a negative correlation between estimated and actual time interval in the baseline trials, suggesting that participants had difficulty with the interval estimation task.

Experiment 3b was a close replication of Experiment 3a with an increased sample size of 40 participants (mean age = 26.8; *SD* = 7.6; 29 female, 2 left-handed). This sample size was based on the effect size for temporal binding found in Experiment 3a. As pre-registered, participants were replaced when there were only five or less observations per cell available (because of an uneven distribution action and nonaction choices; this applied to one participant) and when the correlation between estimated delay and actual delay in the baseline trials was negative (this applied to six participants). All participants gave informed consent prior to the experiments and received either course credit or monetary compensation for participation.

4.1.2. Apparatus

Participants sat in front of a 24″ flat screen and used the C key of a standard German QWERTZ keyboard with the left index finger and the mouse with the right hand to give responses. All visual stimuli were presented on a black background. The pinball machine and the animated ball movement were identical to Experiment 1. Participants wore headphones and heard a launching sound of 650 ms duration whenever the ball was launched to make the event more distinct for the interval estimation task. Likewise, a clicking sound of 200 ms duration was played in trials without keypresses to mark the start of a to-be-estimated interval.

To enter their time estimation, participants saw the question "How long was the interval?" (German original: "Wie lang was das Intervall?"), displayed in the upper part of the display. They responded on a VAS ranging from 0 to 1000 ms with markers in steps of 100 ms by moving the mouse to the left and right. The agency question for (non) action effects was "How strongly did you feel as causal agent for the ball movement to the left/right?" (German original: "Wie sehr hast du dich als Verursacher der Ballbewegung nach links/rechts gefühlt?"). Participants could respond on a VAS ranging from 0 ("a little") to 100 ("a lot") by moving the mouse to the left and right.

4.1.3. Procedure

Each trial started with the display of the pinball machine and the ball in rest (see Fig. S3 in the Supplementary material). A red arrow appeared for 1000 ms above one arm indicating the pre-activated direction. The current number of ball movements to the left and right within one block was displayed above the left and right arm of the pinball machine, respectively.

In operant blocks, participants then saw the words "keypress" and "no keypress", presented above the pinball machine (German original: "Tastendruck" or "Kein Tastendruck"). The mouse cursor was presented between these words and participants could select a keypress or no keypress by moving the mouse cursor onto the corresponding words, thus, selecting the direction in which the ball would be launched. When participants had chosen one option, the words were replaced by a progress bar, i.e., a white framed rectangle which was continuously filled for 1000 to 1500 ms. If participants had chosen the option "no keypress", they heard a clicking sound as soon as the progress bar was filled completely and, following that, the ball was launched into the pre-activated direction. At the same time, participants heard a sound representing the launch of the ball. If participants had chosen the option "keypress", they were instructed to press the response key after the progress bar had been filled completely. Following the keypress, the ball was launched into the opposite direction of the pre-activation and participants heard the launch sound. In both cases, fixed delays were introduced between clicking sound and ball launch or between keypress and ball launch, respectively (Exp. 3a: 100 ms, 400 ms, and 700 ms; Exp. 3b: 300 ms, 500 ms, and 700 ms). Participants were instructed to estimate the delay between the clicking sound and the ball launch sound or their own keypress and the ball launch sound and to indicate their time estimation on a VAS ranging from 0 to 1000 ms. In one out of four randomly selected trials, time estimation was replaced by the agency question. If participants pressed a key during the filling of the progress bar or if they had chosen the option “no keypress” but pressed a key, an error message was presented for 1000 ms and the trial was aborted.

In baseline blocks, participants could not choose the direction of the ball movement. The trial structure was identical to nonactions, except that the words to choose an action or a nonaction were not presented, but only the red arrow to indicate the pre-activated direction. After 1000 ms, the progress bar appeared automatically. When the bar was filled completely (1000–1500 ms), participants heard a clicking sound. Shortly afterward the ball was launched into the pre-activated direction and participants heard the launch sound. The delays between clicking sound and ball launch sound were identical to the delays for actions and nonactions. Participants indicated their time estimation of the delay between the clicking sound and the ball launch sound on the visual analog scale. As for operant blocks, the time estimation question was replaced by the agency question in one out of four randomly determined trials.

At the beginning of the experiment, participants were familiarized with the instructions and completed ten baseline and ten operant practice trials. In the practice trials, all delays from 100 ms to 1000 ms in steps of 100 ms were used and participants received feedback about the accuracy of their estimation. In experimental trials, participants received no feedback and only three different delays were used (Exp. 3a: 100 ms, 400 ms, and 700 ms; Exp. 3b: 300 ms, 500 ms, and 700 ms). The experiment consisted of four baseline blocks and four action/nonaction blocks presented in alternation. The order of block type was counterbalanced across participants. Baseline blocks consisted of 12 trials (18 trials in Exp. 3b), action/nonaction blocks consisted of 24 trials (36 trials in Exp. 3b).

4.1.4. Statistical analysis

All error trials were excluded (i.e., keypresses during the filling of the progress bar or when the option “no keypress” had been chosen; Exp. 3a: 1.7%, Exp. 3b: 1.2%). Temporal binding and agency judgments were analyzed via 3 × 3 repeated-measures ANOVAs with the factors response type (baseline vs. action vs. nonaction) and delay (Exp. 3a: 100 vs. 400 vs. 700 ms; Exp. 3b: 300 vs. 500 vs. 700 ms). For violations of the sphericity assumption, Greenhouse-Geisser corrected *p*-values are reported along with the corresponding ε estimate for correcting degrees of freedom. Pairwise comparisons were analyzed with two-tailed, paired *t*-tests. As for Experiment 2, we further calculated bivariate correlations between binding and agency ratings across participants and tested these correlations against zero with a two-tailed *t*-test. For analysis of the interval estimation data, all trials with estimates that deviated > 2.5 standard deviations from the cell mean were excluded, calculated separately for each participant, response type (baseline, action, nonaction) and delay (Exp. 3a: 0.4%, Exp. 3b: 1.0%). The number of observations per cell was low (≤2) for several participants, due to an uneven distribution of action and nonaction choices in Experiment 3a. To allow for proper, outlier-corrected data, these participants ($n = 7$) were excluded from all analyses.

4.2. Results

Fig. 3 shows mean temporal binding scores and agency ratings for actions and nonactions (see Supplementary Tables S5 and S6 for detailed descriptive statistics).

4.2.1. Exp. 3a

The ANOVA of mean interval estimates revealed a main effect of delay, $F(2,52) = 56.10$, $p < .001$, $\eta_p^2 = 0.68$ ($\varepsilon = 0.57$), and an interaction of delay and response type, $F(4,104) = 5.52$, $p = .009$, $\eta_p^2 = 0.18$ ($\varepsilon = 0.45$). For the delay of 700 ms, actions yielded shorter intervals than baseline (action: 540 ms ± 29.4; baseline: 644 ms ± 24.0), $t(26) = 3.27$, $p = .003$, $d_z = 0.63$. This was also the case descriptively for the delay of 400 ms, but the *t*-test did not reach significance (action: 437 ms ± 29.6; baseline: 498 ms ± 20.8), $t(26) = 1.97$, $p = .059$, $d_z = 0.38$. The delay of 100 ms was not perceived shorter for actions compared to the baseline condition (action: 330 ms ± 37.3; baseline: 311 ms ± 28.0), $t(26) = -0.74$, $p = .468$, $d_z = -0.14$.

For nonactions, the delay of 400 ms was perceived shorter compared to the baseline condition (nonaction: 465 ms ± 20.4; baseline: 498 ms ± 20.8), $t(26) = 2.62$, $p = .014$, $d_z = 0.50$. This was also the case descriptively for the delay of 700 ms (nonaction: 627 ms ± 26.4; baseline: 644 ms ± 24.0), but the *t*-test was not significant, $t(26) = 1.06$, $p = .299$, $d_z = 0.20$. The delay of 100 ms was not perceived shorter compared to the baseline condition (nonaction: 315 ms ± 28.0; baseline: 311 ms ± 28.0), $t(26) = -0.56$, $p = .579$, $d_z = -0.11$.

For analysis of the agency judgements, one additional participant had to be excluded because of empty cells. The ANOVA revealed a main effect of response type, $F(2,50) = 39.03$, $p < .001$, $\eta_p^2 = 0.61$ ($\varepsilon = 0.79$). Neither the main effect of delay, $F(2,50) = 1.88$, $p = .175$, $\eta_p^2 = 0.07$ ($\varepsilon = 0.74$), nor the interaction of response type and delay, $F(4,100) = 0.76$, $p = .526$, $\eta_p^2 = 0.03$ ($\varepsilon = 0.79$), were significant. Planned *t*-tests showed that agency ratings were higher for actions (72.4%VAS ± 4.5) and nonactions (48.2%VAS ± 3.9) compared to the baseline (14.4%VAS ± 3.8), $t(25) = 7.38$, $p < .001$, $d_z = 1.45$ and $t(25) = 6.93$, $p < .001$, $d_z = 1.36$.

Correlations between agency ratings and interval estimates were small and unreliable in most conditions, $|rs| < 0.047$, $ps = .820$, except in the case of actions followed by an action-effect interval of 100 ms, $r = -0.513$, $t(24) = -2.93$, $p = .007$, and 400 ms, $r = -0.384$, $t(24) = -2.04$, $p = .053$.

4.2.2. Exp. 3b

The ANOVA of mean interval estimates revealed a main effect of delay, $F(2,78) = 99.40$, $p < .001$, $\eta_p^2 = 0.72$ ($\varepsilon = 0.56$), and an interaction of delay and response type, $F(4,156) = 12.64$, $p < .001$, $\eta_p^2 = 0.25$ ($\varepsilon = 0.71$). The main effect of response type did not reach significance, $F(2,78) = 3.48$, $p = .056$, $\eta_p^2 = 0.08$ ($\varepsilon = 0.66$). For actions, the delay of 700 ms was perceived shorter compared to the baseline condition (action: 484 ms ± 26.0; baseline: 608 ms ± 24.4), $t(39) = 4.22$, $p < .001$, $d_z = 0.67$. This was also the case descriptively for the delay of 500 ms (action: 428 ms ± 24.1; baseline: 474 ms ± 13.1), $t(39) = 1.81$, $p = .079$, $d_z = 0.29$. The delay of 300 ms was not perceived shorter compared to the baseline (action: 344 ms ± 22.7; baseline: 329 ms ± 13.8), $t(39) = -0.69$, $p = .494$, $d_z = -0.11$.

For nonactions, the delay of 700 ms was perceived shorter compared to the baseline condition (nonaction: 565 ms ± 22.3; baseline: 608 ms ± 24.4), $t(39) = 2.52$, $p = .016$, $d_z = 0.40$. This was also the case descriptively for the delay of 500 ms (nonaction: 455 ms ± 19.1; baseline: 474 ms ± 13.1), but the test did not reach significance, $t(39) = 1.52$, $p = .137$ $d_z = 0.24$. The delay of 300 ms was not perceived shorter compared to the baseline condition (nonaction: 331 ms ± 16.1; baseline: 329 ms ± 13.8), $t(39) = -0.16$, $p = .870$, $d_z = -0.03$.

For analysis of the agency judgements, three participants were excluded because of empty cells. The ANOVA revealed a main effect of delay, $F(2,72) = 3.18$, $p = .047$, $\eta_p^2 = 0.08$, hinting at higher agency ratings for shorter delays. Furthermore, there was a main effect of response type, $F(2,72) = 83.83$, $p < .001$, $\eta_p^2 = 0.70$. The interaction of response type and delay was not significant, $F(4,144) = 1.53$, $p = .208$, $\eta_p^2 = 0.04$ ($\varepsilon = 0.79$). Planned *t*-tests showed that agency ratings were

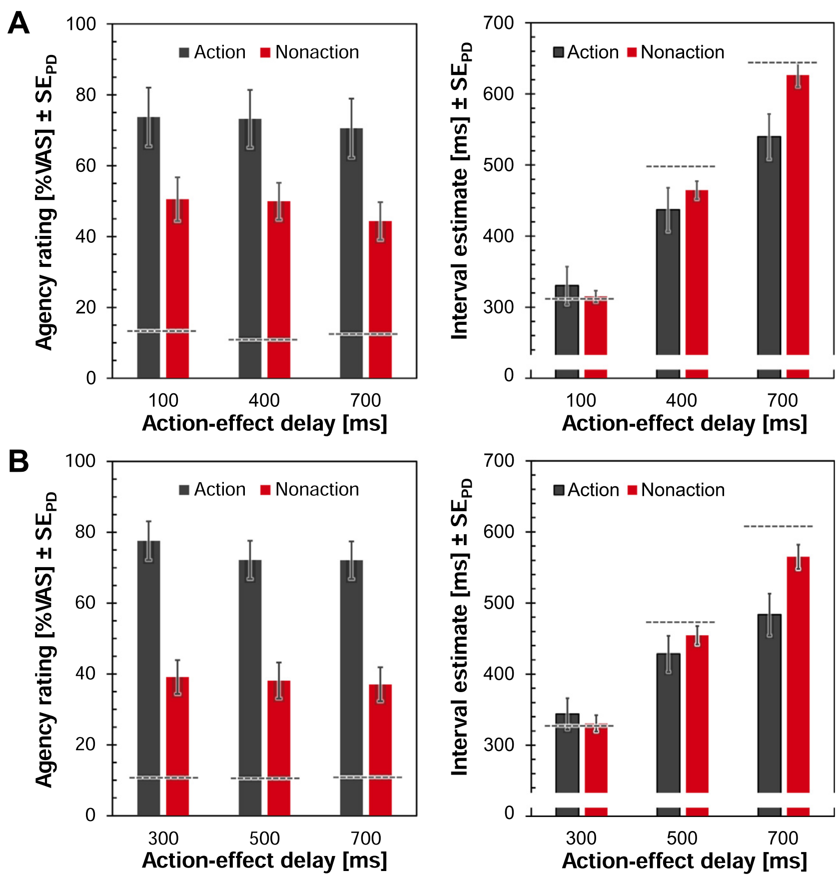

Fig. 3. Results of Experiments 3a (A) and 3b (B). Left panels show mean agency ratings on a visual analog scale (VAS) for actions and nonactions. Agency ratings for the baseline condition are displayed as a dashed line. Right panels show temporal binding as assessed via direct interval estimates. Error bars indicate standard errors of paired differences (SE_{PD}) for the comparison of actions and nonactions with baseline at each action-effect delay.

higher for actions (73.8%VAS ± 3.0) and nonactions (37.9% VAS ± 4.2) compared to the baseline (13.1%VAS ± 2.9); $t(36) = 11.88$, $p < .001$, $d_z = 1.95$ and $t(36) = 5.32$, $p < .001$, $d_z = 0.87$.

Correlations between agency ratings and interval estimates were small and unreliable in almost all conditions, $|rs| < 0.087$, $ps > .609$, except in the case of actions followed by an action-effect delay of 700 ms, $r = 0.329$, $t(35) = 2.06$, $p = .047$.

4.3. Discussion

Experiment 3 replicated the observation of temporal binding for nonactions while carefully controlling for potential confounds in terms of timing uncertainties: Effect delays were judged to be shorter for nonactions compared to baseline trials, even though these conditions were identical in terms of physical stimulation and differed only by the explicit intention not to act in the case of nonactions. The data further suggests a moderating role of the overall delay between action and effect with only longer delays yielding temporal binding for actions and nonactions alike (Ruess et al., 2017).

Temporal binding thus emerged for nonactions, even though the participants' intention not to act was the only difference between a nonaction trial and a baseline trial. This observation qualifies recent proposals that questioned the roles of intentions for temporal binding but instead attribute such binding to multisensory integration of body-related (e.g., tactile) feedback and body-external auditory or visual effects (e.g., Kirsch, Kunde, & Herbort, 2019). Without motor output no distinct body-related feedback is generated, and hence temporal binding is unlikely to ensue from this perspective. The observation of non-action-effect binding in the present study thus suggests that multisensory integration alone cannot account for temporal binding. Rather, this observation suggests that intentions can fulfill a generative role in the process.

In line with Experiment 2, analyses regarding the relation of implicit and explicit measures revealed mostly small and unreliable correlations between these two measures. Even though significant correlations emerged for actions in Experiment 3a at least at shorter delays, these correlations were absent or reversed in Experiment 3b. Together, these findings corroborate the assumption that implicit and explicit measures tap into different processes of the sense of agency (Dewey & Knoblich, 2014; Saito et al., 2015; Schwarz et al., 2019).

5. General discussion

These present results indicate that people readily feel a sense of agency for sensory consequences that follow a decision not to act, and this was not only true for subjective ratings but also for the implicit measure of temporal binding. It thus seems as if the internal event of forming a deliberate intention not to act is sufficient to induce perceptual illusions such as temporal binding which has been discussed as an implicit precursor of subjective agency (Haggard et al., 2002; for critical remarks, see Gozli, 2019). This observation is striking because nonactions, by definition, do not include any overt motor event that would enable an actual causal connection between a nonaction and its resulting effects.

Based on these observations we propose that agency for nonactions is similar to agency for overt actions in that they both draw on a combination of predictive and retrospective, inferential processes. The latter processes rest on top-down beliefs, thoughts, and contextual information which can be used to construct a sense of agency after observing an event in the environment (Synofzik et al., 2008; Wegner, 2003; Wegner, Sparrow, & Winerman, 2004; Weller, Schwarz, et al., 2017). An involvement of such retrospective inferences in case of nonactions is certainly plausible as it mirrors everyday experiences in the assessment of own choice behavior (Kahneman & Tversky, 1982). Crucially, the observation of effects for the implicit measure of temporal binding tentatively suggest that humans also recruit predictive processes in the case of a nonaction (Moore & Obhi, 2012; Moore & Haggard, 2008; Tanaka, Matsumoto, Hayashi, Takagi, & Kawabata, 2019; for a potential interplay of predictive and retrospective processes, see Desantis, Roussel, & Waszak, 2011; Haering & Kiesel, 2012; Makwana & Srinivasan, 2019). This conclusion resonates with recent findings indicating that nonactions are based on an active anticipation of upcoming sensory consequences, i.e., a representation of nonactions in terms of their sensory effects (Kühn et al., 2009; Weller, Kunde & Pfister, 2017).

The joint operation of predictive and inferential processes in the sense of agency has been proposed to operate by means of a weighted integration of different internal and external cues (informing inferential and predictive processes), such as sensorimotor and proprioceptive cues, as well as beliefs about action-effect relations (Moore & Fletcher, 2012; Moore, Wegner, & Haggard, 2009). Cue integration approaches assume that internal and external cues are combined and weighted based on their availability and reliability (Synofzik et al., 2008). At first sight, it seems that a major difference between actions and nonactions is the availability of certain agency cues. That is, the motor event of an action allows for specific motor predictions based on efference copies, which are assumed pivotal for predictive processes by motor-based forward models (Haggard et al., 2002). In contrast, these motor-related predictions are not available for nonactions. However, recent theoretical arguments have highlighted that many experimental effects that are explained with reference to such forward models, including temporal binding, may be equally captured by ideomotor models which typically assume predictions (or: anticipations) to precede rather than follow motor activity (Dogge, Custers, & Aarts, 2019; Horváth, 2015; Hughes et al., 2013; Klaffehn, Baess, Kunde, & Pfister, 2019). Conversely, findings that have been discussed as decisive evidence for the involvement of motor predictions have not been replicated (Schwarz, Pfister, Kluge, Weller, & Kunde, 2018). Adopting an ideomotor perspective therefore suggests that the cognitive system might draw on fundamentally similar cues to determine agency for actions and nonactions alike.

We assumed that participants in our study ‘decided not to act’, when they did not act. Alternatively, one may argue that they ‘did not decide to act’. It is notoriously difficult to distinguish between these two alternatives from a third-person perspective, but we still lean towards assuming that in our experiments, not acting was preceded by a decision. First, technically speaking, participants were forced to decide because acting and not acting were mutually exclusive alternatives. So not deciding inevitably ended in one of the two options with the associated consequences. Second, in forced-choice trials participants complied with task instructions, indicating that they processed the task stimuli and made their behavior depended on these stimuli. Even if participants may not decide to act in case on nonactions, there must have been a stimulus-contingent decision to not decide to act. In any case, this would be a less parsimonious description of the matter of affairs, as compared to assuming that acting and not acting are both based on a decision for the option that was chosen eventually. At a more general level this discussion points to the question whether it is possible to not act all. Watzlawick et al. once noted that it was impossible to not communicate (Watzlawick, Bavelas, & Jackson, 1967). Thus, even if a person does or says “nothing”, this causes effects in the social environment, though the person may not always have these effects in mind (but should do so to avoid misunderstanding). To say nothing does become an intentional communicative act also from the perspective of the actor, if she aims at certain effects at a partner, for example, to express disagreement. The same is true, we suggest, for not acting. Not acting can produce effects in the environment. And in this sense it is not possible to not act, as it is not possible to not communicate. Not acting becomes an intentional act also from the perspective of the actor, if the actor does not act with a specific effect in mind, so as it was likely the case in our studies.

In addition to showing that a vivid sense of agency emerges for deliberate nonactions, our findings also indicate that agency for actions is substantially stronger than agency for nonactions. From a cue-integration perspective, this pattern is conceivable as the onset of an action and thus the duration of the action-effect interval can be assessed more reliably than the corresponding interval in case of nonactions. This difference might have been particularly pronounced in Experiment 1 and 2 in which the visual or auditory effects followed actions in all cases with a defined, constant delay, whereas this delay was highly variable in the case of nonactions. In Experiment 3, the (non)action-effect delay was fixed, but this methodological extension came at the expense of separating nonaction decision and execution. We believe that the limitations inherent in each of these experiments reflect a characteristic that necessarily accompanies most or even all nonactions. The observation of reduced agency in the present paradigm is therefore likely to capture the subjective side of nonactions in general, including those that have been discussed in moral and philosophical approaches (Cushman, Young, & Hauser, 2006; Willemsen, 2018).

The setup of the present experiments differs from previous moral and philosophical approaches in one critical aspect, however: While most previous approaches focused on nonaction decisions for motivationally or morally charged scenarios, the present experiments explicitly focused on highly controlled and motivationally neutral settings. We believe that this methodological choice allowed investigating agency for nonactions in a strict and conservative manner. The results obtained in the present setup should therefore be seen as lower bound for agency in the case of nonactions, while both explicit and implicit markers of agency are likely to yield even more pronounced effects in situations in which (non)action decisions come with a moral connotation (Moretto, Walsh, & Haggard, 2011). A second potential moderator might be the emotional valence of (positive or negative) outcomes in such scenarios. The database on the impact of valence on agency is currently mixed, however, with studies finding more agency for positive outcomes, more agency for negative outcomes, or no modulation of agency, depending on the task and agency measure (Christensen, Yoshie, Di Costa, & Haggard, 2016; Moreton, Callan, & Hughes, 2017; Tanaka & Kawabata, 2019; Yoshie & Haggard, 2013).

Setting aside such methodological considerations, we believe that investigating the sense of agency for nonactions in scenarios with higher motivational value and moral implications is a promising avenue for future research. In a similar vein, it might be informative to assess how related classic findings – such as the diffusion of responsibility in the bystander effect (Latané & Darley, 1970), or the observation of limited regret over unfavorable outcomes of omissions relative to actions (Kahneman & Tversky, 1982) – may derive from inherent differences in subjective agency for actions and nonactions. Finally, the potential of spontaneous agency and responsibility for the consequences of deliberately not acting is of increasing relevance in view of technical systems, such as autonomous vehicles, which can cause far-reaching consequences if human operators do not intervene (Bonnefon, Shariff, & Rahwan, 2016). Our findings foster a psychologically informed debate about the responsibilities that law and society ascribe in such pervasive cases.

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Author contributions

Conceived study and methodology: L.W., W.K., R.P.; programmed experiments and collected data: L.W.; analyzed data: L.W., K.S., R.P.; curated data: L.W.; visualized results: L.W., R.P.; wrote the paper: L.W., K.S., W.K., R.P.

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Declaration of competing interest

Authors declare that they have no competing interests.

Supplementary data

Experiment files, raw data, and analysis scripts are available via the Open Science Framework (https://osf.io/kmwfq; doi: 10.17605/OSF.IO/KMWFQ). Supplementary material to this article can be found online at https://doi.org/10.1016/j.cognition.2019.104136.

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