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Studying Sense of Agency Online: Can intentional binding be observed in uncontrolled online settings?

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Abstract

Intentional binding is often used as an implicit index of the sense of agency. However, intentional binding research has primarily been conducted in controlled lab environments. During the COVID-19 global pandemic, there has been a shift to implementing studies using online platforms and it is an open question whether the intentional binding effect can be found using an online experimental set-up and participant sample. Here, we address this question by asking online participants to complete the Libet clock version of the intentional binding task, which we make freely available to researchers as a jsPsych (De Leeuw, 2014) plugin. Intentional binding was observed in the form of later keypress estimates and earlier auditory tone estimates, when the auditory tone followed the keypress. These findings confirm that intentional binding can be assessed in online contexts. We discuss these findings in relation to the broader intentional binding literature.

Keywords: Sense of Agency, Intentional Binding, Online Sample, Libet Clock, Big Five, Judgement of Agency

1. Introduction

The sense of agency, the fundamental feeling of control over one's actions and the consequences of those actions, is a core part of the human condition (e.g., Obhi & Moore, 2012). In fact, a consistent lack of a sense of agency is associated with brain disorders, such as corticobasal syndrome and schizophrenia (e.g., Moore, 2016; Wolpe et al. 2014). As such, it should come as no surprise that the past two decades has seen a surge of research by cognitive scientists and neuroscientists in this area (for a recent review, see: Haggard, 2017). A key factor in this surge was the discovery of *intentional binding*: the temporal illusion of perceiving the gap between one's actions and the consequences of those actions as closer in time compared to when such actions are not intentional or do not occur at all (e.g., Haggard et al., 2002; Obhi & Moore, 2012; Haggard, 2017).

Initial evidence for intentional binding was provided by Haggard et al. (2002). In their study, they used the Libet clock method wherein participants were tasked with observing a conventional clock face in order to judge the onset time of one of two events: their keypress or an auditory tone. In the *operant* condition, their keypress would always be followed by an auditory tone. In the *baseline* condition, their keypress or the auditory tone would occur alone. As such, participants made four types of perceptual judgements: the onset time of their keypress with no subsequent audio tone (i.e., Baseline-Keypress), the onset time of an audio tone with no preceding keypress (i.e., Baseline-Tone), the onset time of their keypress with the audio tone following (i.e., Operant-Keypress), and the onset time of the audio tone after their keypress (i.e., Operant-Tone). Results showed that participant estimates for the Operant-Keypress were perceived *later* in time relative to the Baseline-Keypress, while the Operant-Tone were perceived as *earlier* in time relative to the

Baseline-Tone. Furthermore, in a critical control experiment, a separate set of participants were induced to make involuntary movements (via TMS) which resulted in perceptual shifts in the opposite direction. Given these results, Haggard et al. (2002) concluded that, "*Our results show that truly operant intentional actions (voluntary action, then tone) elicit perceptual attraction or binding effects. This effect associates or binds together awareness of the voluntary action with awareness of its sensory consequence, bringing them closer in perceived time.*" (pg. 384). They speculate that this *intentional binding* may be related to "normal agency" (i.e., the sense of agency).

Follow up studies have corroborated the intentional binding effect and further clarified its relationship with the sense of agency (for a recent review, see Haggard, 2017). The aim of the current study is to add to this growing field by testing whether the intentional binding effect can be observed using an online sample. There are multiple factors that motivate the need for such a test. First, as far as we are aware, the vast majority of previous work on intentional binding (measured via the Libet clock method) has been conducted in a controlled laboratory setting; this is a problem as a person's sense of agency is not confined to pressing a button in a quiet room with minimal distractions. Exceptions to this is work by Garaizar et al. (2016) and Cubillas et al. (2020) who showed that action-binding effects (perceiving one's action as later in time if it is paired with some outcome) were possible to find using an online sample. However, these studies exclusively looked at action-binding, so it is unclear whether tone-binding (perceiving the outcome of an action as earlier in time if it is preceded by an action) would be successfully replicated using an online sample. Furthermore, these studies compared the delay (e.g., immediate vs. 500ms) between action and outcome as a measure of action-binding; however, it seems to be more common to compare action-outcome pairings with action-alone and outcome-alone baselines. As such, it is still an open question whether intentional binding can be observed in an online sample while using methodological parameters that more closely resemble the original findings (Haggard et al., 2002). Relatedly, with the COVID-19 pandemic disrupting normal lab functions (i.e., no in-person testing), researchers have pivoted to the use of online experiments to continue their work. However, it would be a mistake to assume that intentional binding effects will generalize to this new (noisier) context, as online experiments often mean that the experimenter has little to no control of the environment the participants is completing the experiment in. As such, it is important to test whether the basic intentional binding effect is found using an online sample. Finding that it does opens up future avenues of research using the online medium. Finally, although there are a number of studies showing intentional binding effects, the sample size is often limited due to time constraints and lab space; the use of small sample sizes may be problematic in ascertaining an accurate estimate of effect sizes (especially outside of a well-controlled lab setting). Overall, then, the use of a (relatively) large online sample to test the robustness of the intentional binding effect outside of a lab setting will provide us with novel insights into its generalizability, effect size, and its feasibility as a subject of experimental inquiry outside of well-controlled lab environments.

We created an online version of the Intentional Binding task using the Libet clock method (described above) via JavaScript/jsPsych (available for download at github.com/kinleyid/jspsych-libet; see methods section below for more details) wherein participants completed four conditions: Baseline-Key, Baseline-Tone, Operant-Key, and Operant-Tone. In the Baseline-Key condition, participants simply pressed a key while watching a rotating clock hand on a clockface and estimated the position of the clock hand at the onset of their key press. In the Baseline-Tone condition, they waited for an audio tone to play while watching a rotating clock hand on a clockface and then estimated the position of the clock hand when the onset of the tone occurred. In the Operant-Key and Operant-Tone conditions, participants pressed a key that also triggered an audio tone; in the former condition, participants estimated the timing of the keypress, while in the latter they estimated the timing of the tone. Note that intentional binding has also been studied using the "interval estimation" method (e.g., Engbert et al., 2007; Moore & Obhi, 2012), wherein participants simply estimate (often in milliseconds) the gap between an action they made (i.e., keypress) and a sensory consequence (i.e., an auditory tone). We opted to use the Libet clock method for this study as it has built-in baseline conditions with which to compare the experimental (i.e., operant) conditions. Furthermore, unlike the interval estimation method, the Libet clock allows for the assessment of both action binding effects (i.e., perceptual shift of voluntary keypress towards tone effect) and tone binding effects (i.e., perceptual shift of tone effect towards the voluntary keypress).

Given that this study is a direct replication of previous work on intentional binding, there are a number of directional predictions that we can make. In particular, we predict that estimates during the Operant-Key condition will occur later compared to the Baseline-Key condition while estimates during the Operant-Tone condition will occur earlier in time compared to the Baseline-Tone condition.

As a final point, we also had participants complete the Big 5 inventory as well as a measure of explicit sense of agency (i.e., "On a scale of 1–10, to what extent do you feel like you caused the tone?"). We added the Big 5 inventory to test whether personality traits influence intentional binding effects – a topic that, as far as we are aware, has not been explored using the Libet clock method. Lab experiments may also be limited in studying this topic as correlations often need large sample sizes relative to experimental methods. As such, the current study is in a unique position to shed light on this issue, which may lead to interesting insights for future research. We added the measure of explicit sense of agency (i.e., Judgements of Agency) because questions about the relationship between intentional binding and the sense of agency is currently being debated (e.g., Imaizumi & Tanno, 2019; Kirsch et al., 2019). One method that has been used to help answer this question has been to use explicit measures of the sense agency and to see if such measures correlate with intentional binding (presumably providing convergent validity for the measure). As such, we thought it prudent to add this measure. Overall, however, these measures were added for exploratory purposes and do not constitute the main question in this study.

2. Methods

All methods and data analysis plan have been pre-registered: https://aspredicted.org/blind.php?x=c8pq7w. Please see the supplementary materials for a copy of the processed and raw Libet clock data. The jsPsych plugin used to obtain the results here is available at github.com/kinleyid/jspsych-libet.

2.1 Deviation from Pre-Registration

Please note that we did not pre-register the use of the Judgement of Agency scale as an exploratory measure. Overall, however, we thought it interesting enough to include late in the experimental design pipeline, although we caution strong interpretations due to it being a last minute addition to this study. Also note that although we explicitly pre-registered conducting two *t*-tests, we did not specify in our pre-registration how we would account for the inflation of type 1 error as a result of multiple comparisons. To account this inflation, we decided to lower our alpha to 0.025 for this study.

2.2 Participants

We collected data from 80 right-handed participants (female = 59; mean age = 18.1 [SD = 1, Range = 17-24])[1] from the McMaster University Psychology participant pool for course credit. N = 80 was based on an effect size of d = 0.45 (based on the average effect size in social psychology (Richard et al., 2003); however, please note that a recent meta-analysis by Tanaka et al. (2019) showed that the meta-analytic effect size for action-binding is d = 0.451 and the meta-analytic effect size for tone-binding (or outcome-binding) is d = 0.726 – as such, our current power analysis seems to appropriately estimate the size of these effects (with the tone-binding effect perhaps being overpowered). As we expected directional effects, we based our power analysis on one-tailed dependent *t*-tests (see pre-registration). However, note that we pre-registered two tests – to account for possible inflation of type 1 error, we opted to lower our alpha to 0.025 for our analysis. Note that 5 participants were removed due to not following instructions during the experiment and 1 participant was removed due to having >30% outliers – as these participants were all replaced, our sample size remained at 80 (see pre-registration for these exclusion criteria). Prior to participation,

[1] Note that participants were free not to answer questions regarding their gender and age. As such, 9 participants did not indicate their gender and 7 participants did not indicate their age.

participants provided online informed consent. The study was approved by the McMaster Research Ethics Board (MREB).

2.3 Intentional Binding Task via the Libet Clock Method

The experiment was programmed in JavaScript using the jsPsych library (De Leeuw, 2014) and administered over the web. The intentional binding task was built as a jsPsych plugin (available for download at github.com/kinleyid/jspsych-libet) and follows previous work on intentional binding (e.g., Haggard et al., 2002; Kirsch et al., 2019). The main display contained a clock face with a fixation cross at its center and numbers around its perimeter. The number 5 was at the 1 o'clock position, 10 at the 2 o'clock position, etc., up to 60 at the 12 o'clock position. 60 major tick marks were placed around the perimeter of the clock face, corresponding to the positions of minutes in an hour on an analog clock, and each pair of major tick marks was separated by a minor tick mark half their length.

At the beginning of a trial, the fixation cross appeared in yellow for 400ms (Fig. 1A). The screen then became black, and the clock hand appeared at a random initial location and began spinning clockwise with a period of 2560ms (Fig. 1B). This animation used JavaScript's requestAnimationFrame() function, which allows modern web browsers to synchronize their display with the screen refresh rate. After the "critical event" (a key press or auditory tone, depending on the condition), the clock hand continued to spin for either 1, 1.5 or 2 s (selected at random with equal weight) before disappearing for 1 s (Fig. 1C). It then reappeared in green at a random location between 45 and 60 degrees away from where it was at the time of the "target event," whose latency was to be estimated (a key press or auditory tone, depending on the condition; Fig. 1D). Simultaneously, instructions appeared above the clock face indicating that the participant could move the clock hand with the left and right arrow keys and finalize their selection with the enter key. The right and left arrow keys moved the clock hand 3 degrees clockwise or counter-clockwise, respectively (i.e., 30s worth of movement on the minute hand of an analog clock). Upon making a selection, the instructions and clock hand immediately disappeared, and the fixation cross again became yellow, signaling the beginning of another trial. Regarding possible latency issues due to variability in participant hardware – there will certainly be differences between the true time of the physical keypress and the timestamp assigned to it (which depends on, among other things, the keyboard's scan rate), and between the true time that the audio begins to play, and the timestamp assigned to the onset of the audio stimulus (which is roughly the audio latency). However, the same code records keypress timestamps in the baseline and operant key conditions, and the same code records audio timestamps in the baseline and operant tone conditions. Thus, these differences between the true and reported times of different events are consistent between conditions and will (on average) cancel out when measures of intentional binding are computed on a within-participants basis by subtracting one condition's timestamps from another's. Nevertheless, note that recent work has shown that factors such the participant's operating system and browser, software implementation, etc. can introduce temporal variability for studies implemented online – especially regarding perception-action coupling (Bridges et al., 2020). As such, readers should be cautious when interpreting these results as the current study cannot fully account for all of these extraneous factors (see discussion section).

FIGURE 1 ABOUT HERE

The task consisted of 4 conditions presented in a random order, each condition corresponding to a block of trials. Each block began with at least 5 practice trials and contained 40 experimental trials. In the Baseline-Key condition, a single key press from the participant was both the “critical event” and the “target event.” Participants were instructed to wait at least one full rotation of the clock hand before pressing the space bar with their right index finger. They were also instructed not to make their key press at a “pre-decided and/or stereotyped” time point. During the practice trials, if participants made a response before one full rotation had elapsed, they were informed of this and repeated that trial. In the Baseline-Tone condition, an auditory tone (440 Hz sine wave lasting 200ms) was both the “critical event” and the “target event.” The time at which the tone played was selected from a uniform distribution between 2.5 s and 8 s after the clock hand began to spin. The Operant-Key condition was identical to the baseline key condition, except that the “critical event” was the playing of the auditory tone 250ms after the key press. Participants were asked to estimate the time of their key press. Finally, the Operant-Tone condition was identical to the operant key condition except that the “target event” was the playing of the auditory tone rather than the key press.

2.4 Judgement of Agency and Big 5 Scales

Participants also completed an explicit measure of agency (sometimes referred to as a Judgement of Agency) wherein they answered the question: On a scale of 1–10, to what extent do you feel like you caused the tone (with 0 labelled as “Not at all” and 10 as “Fully”)? They answered this question every 3 trials during the Operant conditions. Participants also completed the 44-item Big

5 Inventory (John & Srivastava, 1999) wherein Openness, Conscientiousness, Extraversion, Agreeableness, Neuroticism were the major subscales.

2.5 Procedure

Participants were recruited via the McMaster Psychology participant pool. After signing up for the study, they were given a link to the experiment website. Participants first saw a welcome page with information about the study. They were informed that continuing with the study after this page would be indicative of their consent to participate in the study. Afterwards participants completed the intentional binding task (described above). During the Operant conditions, every 3 trials, participants completed the Judgement of Agency scale. At the end of the intentional binding task participants completed the Big 5 scale. At the completion of the Big 5 scale, participants were shown a debrief page explaining the purpose of the experiment and then re-directed back to the McMaster Psychology participant pool website to obtain their credit.

2.6 Data Processing and Analysis Plan

As per our pre-registration, estimates that were larger than 90 degrees from the actual time point of the critical event (e.g., if the critical event occurred at the clock hand 3, then any estimates before 12 or after 6 are larger than 90 degrees) or estimates that were made 300ms or faster after trial onset were removed (~1.5%). Valid estimates were used in two directed *t*-tests: Baseline-Key < Operant-Key and Baseline-Tone > Operant-Tone. Note that as participants used the clock hand to estimate when they made their action or when the tone occurred, we used the distance (measured in milliseconds) between the actual clock hand position and participant estimates as the dependent variable. As such, a 0 ms value would indicate no error between the actual and estimated clock hand position, while negative and positive values would indicate estimates earlier and later in time, respectively. All analyses were conducted via Jamovi (ver. 1.6.23).

3. Results

3.1 Intentional Binding Effects

Results showed that estimates in the Operant-Key condition were significantly *later* in time [M = 29.2 ms, SD = 92.6 ms, SE = 10.35 ms] relative to estimates in the Baseline-Key condition [M = -17.4 ms, SD = 78.6 ms, SE = 8.8 ms] [$t(79) = 4.15$, $p < 0.001$, $d = 0.46$, 95% CI [0.23, 0.69]]. See Fig 2A. Results also showed that estimates in the Operant-Tone condition were significantly *earlier* in time [M = -15.1 ms, SD = 142 ms, SE = 15.8 ms] relative to estimates in the Baseline-Tone condition [M = 78.2 ms, SD = 108 ms, SE = 12.1 ms][$t(79) = 8.58$, $p < 0.001$, $d = 0.96$, 95% CI [0.69, 1.22]]. See Fig 2B.

FIGURE 2 ABOUT HERE

3.2 Big 5 Personality Traits

To correlate the intentional binding effects with the Big 5 subscales, we opted to take the difference score between each Baseline and Operant conditions for both stimulus types (i.e., Operant-Key -

Baseline-Key; Operant-Tone - Baseline-Tone)[2]. This gave us raw effect sizes for both the Key and Tone intentional binding effects. For all of the Big 5 subscales (i.e., Openness, Conscientiousness, Extraversion, Agreeableness, Neuroticism) we found no significant correlations with the Key nor Tone intentional binding effects [Key Effects: Openness [r = 0.034, 95% CI [-0.19, 0.26], p = 0.77], Conscientiousness [r = 0.096, 95% CI [-0.13, 0.31], p = 0.39], Extraversion [r = 0.089, 95% CI [-0.14, 0.31], p = 0.31], Agreeableness [r = 0.023, 95% CI [-0.20, 0.25], p = 0.85], Neuroticism [r = 0.16, 95% CI [-0.07, 0.37], p = 0.16]; Tone Effects: Openness [r = 0.13, 95% CI [-0.35, 0.09], p = 0.25], Conscientiousness [r = 0.05, 95% CI [0.26, 0.67], p = 0.67], Extraversion [r = - 0.003, 95% CI [-0.22, 0.22], p = 0.98], Agreeableness [r = 0.028, 95% CI [-0.19, 0.25], p = 0.81], Neuroticism [r = 0.079, 95% CI [-0.15, 0.29], p = 0.49].

3.3 Judgement of Agency

Based on recent work by Imaizumi & Tanno (2019), we opted to conduct trial-by-trial intracorrelations to explore the relationship between participants' judgements of agency and the intentional binding effect. Data from blocks with zero variance in judgments of agency (e.g., in which participants only responded with the same number) were treated as missing, which meant that we did not have usable data for some participants. For the Keypress binding effect, we had n = 68 valid participants, while for the Tone binding effect, we had n = 74 valid participants. We

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[2] Note that participants were free to choose not to answer any question they did not feel comfortable answering. As such, some subscales had missing values and could not be computed. The breakdown of missing participant information is as follows: Extraversion: 3 missing, Agreeableness: 4 missing, Conscientiousness: 1 missing, Neuroticism: 4 missing, Openness: 3 missing.

correlated participant estimates (via Spearman's rho to account for the skewed distribution of values obtained from the Judgements of Agency scale) during the Operant Key and Operant Tone blocks with their score on the Judgement of Agency scale. Positive and negative correlation coefficients indicate that higher scores on the judgement of agency is associated with later or earlier estimates (relative to the actual hand clock position). One-sample t-tests were used to explore whether correlation coefficients were significantly different from 0 for both the Operant Key and Tone conditions. For the Operant Tone condition, the one-sample t-test was significant [rho = -0.14, 95% CI [-0.220, -0.068], $p < 0.001$. For the Operant Key condition, the one-sample t-test was not significant [rho = 0.05, 95% CI = [-0.017, 0.127], $p = 0.13$].

4. Discussion

The aim of the current study was to test whether the intentional binding effect could be observed in an online context. Consistent with laboratory studies, results showed robust evidence for both the action and tone binding effects, such that participants estimated their key responses later in time when they were paired with a tone and tones estimated as earlier in time when they were preceded by a keypress (relative to when the keypress and tones occurred on their own). This result nicely corroborates previous work on the intentional binding effect and (in addition to work by Garaizar et al., 2016 and Cubillas et al., 2020) further extends it by showing that it is possible to study intentional binding using an online testing set-up and participant sample. This suggests that the intentional binding effect is not limited to well-controlled lab environments. It is important to highlight that a recent meta-analysis by Tanaka et al. (2019) showed that the meta-analytic effect size for action-binding is d = 0.451 and the meta-analytic effect size for tone-binding (or outcome-binding) is d = 0.726. As such, our results seem to be quite consistent with the action-binding meta-analytic effect size while our tone-binding effect seems to be larger than the meta-analytic effect size; nevertheless, there are previous lab experiments that have reported similarly high effect sizes (e.g., Obhi & Hall, 2011 report tone-binding effects at d = 1.16). All in all, our reported effect sizes obtained from our online sample seems to be within the expected range relative to laboratory studies.

We also report that we failed to reject the null hypotheses for the correlational analyses between action/tone binding effects and the Big 5 personality traits. As far as we are aware no other study has explicitly explored the relationship between the intentional binding effect (measured via the Libet clock method) and the Big 5 personality traits (although see Caspar et al., 2016; 2018). However, as it is not possible to accept the null hypothesis using frequentist statistics, we simply state the fact that null results were found for these tests. Our exploratory analysis also showed that tone binding was significantly correlated with an explicit measure of the sense of agency. This finding corroborates recent work by Imaizumi & Tanno (2019) showing that intentional binding (assessed via the interval estimation method in their study) correlates significantly with explicit judgements of agency. The current study thus adds to our growing understanding of this relationship by showing that intentional binding measured via the Libet clock method also correlates with explicit judgements of agency. It is unclear exactly why we found a significant correlation for tone-binding but not for action-binding. Again, however, note that we cannot conclude that there is no effect simply due to a non-significant result.

As a final note, there are a number of limitations in this study that are worth outlining. First, although using an online sample arguably should lead to more potential distractors for the participant while completing the task, we cannot know for sure what distractors existed for each participant. It is possible that every single participant was in a quiet room with minimal distractions while completing the task, thus replicating a lab environment. Nevertheless, our main objective was to test whether an intentional binding effect would still be found in an online context; in this regard, the results do seem to suggest that such effects are robust. More work will be needed to explore how different types of distractors might influence intentional binding. Second, as discussed in the introduction, the current study used the Libet clock method rather than the interval estimation method to index intentional binding. As such, it is unclear whether the current results will generalize to the interval estimation method. Indeed, the relationship between intentional binding as measured by the Libet clock method and intentional binding as measured by the interval estimation method has not yet been fully explored. Future work will be needed to elucidate this issue. It is also important to note that we specifically used a *student* online sample in this study. As such, it is unclear whether these results will generalize to a more general online population (with a wider range of ages, occupation, etc.). Future online studies on the sense of agency will be needed to explore this issue.

It is also important to acknowledge that many extraneous factors (hardware, operating system, browser, and software implementation) introduce variability to web-based measures of audio and action latency; in particular, Bridges et al. (2020) note that estimates of absolute latency are not reliable in this setting. In keeping with their recommendations, we have used a fully within-subjects design with an explicit control condition, which cancels out any absolute lags in latency measurements (Bridges et al., 2020). Thus, our main goal of testing whether an intentional binding effect would still be found in an online context is still met. Nevertheless, it is important to note that such latency variability could potentially explain the large group variability we found relative to off-line studies; thus, the results of this study can be taken as a first step in validating the use of intentional binding in an online setting. Future studies will be needed to directly explore the differences between online and offline studies of intentional binding, ideally with both formats conducted in the same experiment. These issues are especially critical for future studies involving one or more between-subjects variables, as extraneous factors involving participants' hardware, software, browser, etc. may systematically influence between-subject comparisons.

Lastly, while the current study follows previous work in interpreting intentional binding as an implicit marker of the sense of agency, it is important to note that there is evidence to suggest that this relationship may not be so clear. In particular, recent work by Kirsch et al. (2019) using the Libet clock method showed that intentional binding effects can be observed without action intentions being present. This suggests that alternative explanations may be more appropriate – for example, it is possible that intentional binding is due to the role of causal inferences between an action and effect (e.g., Buehner, 2012) and/or due to the sensory integration of multimodal cues (e.g., Moore & Fletcher, 2012). Nevertheless, recent work by Imaizumi & Tanno (2019) showed that intentional binding (assessed via the interval estimation method in their study) correlates significantly with explicit judgements of agency, providing some convergent validity for intentional binding as a measure of the sense of agency. The current study corroborates this work by showing that intentional binding (assessed via the Libet clock method), or at least tone-binding, significantly correlates with explicit judgements of agency. However, note that it is an open question whether explicit judgements of agency are itself a sound measure of the sense of agency, as factors such as causal beliefs may also be influencing one's self-reported judgements. Future work (both empirical and theoretical) will ultimately be needed to full resolve this issue; nevertheless, the use of an online format in conducting this work may prove useful in solving this issue.

In conclusion, the current study found robust evidence for the intentional binding effect using the Libet clock method: participants estimate their keypresses as later in time and the audio tone as earlier in time when the keypresses were subsequently followed by an audio tone (relative to when the keypress and audio tone were presented on their own). We also report an exploratory analysis showing no significant correlations between intentional binding effects and personality traits; however, we found that explicit judgements of agency significantly correlate with tone binding but did not find significant correlations with action binding. Overall, the current study furthers our understanding of intentional binding by directly replicating the original effects in a novel sample/context, which may lay the foundation for future research using online samples.

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Conflicts of Interest

The authors declare no conflicts of interest.

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