On the influence of Libet clock parameters on intentional binding and intention timing

PhD thesis

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Declaration of authorship

I, Bianca Elena Ivanof, hereby declare that this thesis and the work presented in it are my own. Where I have consulted the work of others, this is always clearly stated.

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Abstract

The Libet clock method for subjective timing of events is a widely used implicit agency and intention timing measure. However, to date, it has been unclear how the properties of the Libet clock stimulus, as well as the participant instructions, affect these two cognitive phenomena and, by extension, the comparability of results across the volition literature. This is something this PhD thesis assessed for the very first time.

In a series of experiments, I first investigated how manipulations of the Libet clock influenced intentional binding, a paradigm based on the Libet clock that provides an implicit measure of sense of agency. I found that certain manipulations of the clock, namely the rotation speed and radius, significantly impact on binding. I then moved beyond the clock stimulus per se and found that manipulations of Libet-style action initiation instructions influence binding. Finally, I turned the lens on intention timing itself, something the Libet clock was originally designed to measure. I found that manipulations of the Libet clock speed and number of clock markings have a significant effect on intention timing judgments.

These findings are discussed in the context of their practical implications for the inter-study variability in results in volition research, and of their theoretical implications for theories of sense of agency and intention timing.

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CHAPTER 1

General introduction

Sense of agency refers to the experience of being in control of one's actions and their consequences. It governs everyday life and is closely tied to self-awareness (Jeannerod, 2003), social interaction among individuals (Obhi & Hall, 2011), their health and well-being (Marmot et al., 1991; Welzel & Inglehart, 2010) and the sense of responsibility they have over their deeds (Caspar, Christensen, Cleeremans & Haggard, 2016).

Although we take our agentic capabilities for granted, there is increasing evidence that the experience of agency can be quite separate from the facts of agency (Moore, 2016). This point is most visible in scenarios in which we feel an exaggerated sense of agency over our actions and their outcomes. Examples of this include the so called "placebo" buttons, which are buttons that we encounter in our everyday lives, such as "close door" buttons in lifts, that we think do things, but are often redundant (McRaney, 2013).

The discrepancy between the experience and facts of agency is also apparent in neurological or psychiatric disease. For instance, people who suffer from anosognosia for hemiplegia (and are thus unaware of their limb paralysis) have been shown to report a sense of control over movements that they did not, in fact, make (Fotopoulou et al., 2008). As far as psychopathology is concerned, certain lines of evidence suggest, for example, that clinically depressed individuals exhibit a decreased sense of agency over their action consequences (Alloy & Abramson, 1979), which leaves them feeling profoundly incapacitated (Slaby, Paskaleva & Stephan, 2013). By contrast, schizophrenia patients have been noted to exhibit an increased sense of agency over their actions and associated outcomes (e.g. Haggard, Martin, Taylor-Clarke, Jeannerod & Franck, 2003; Voss et al., 2010), despite self-reports indicating that they feel less in control than neurotypical individuals (Moore & Obhi, 2012). Such findings highlight how our experience of agency is fallible. This leads us to the question as to what mechanisms and processes aid the construction of sense of agency, which forms the focus of the next section.

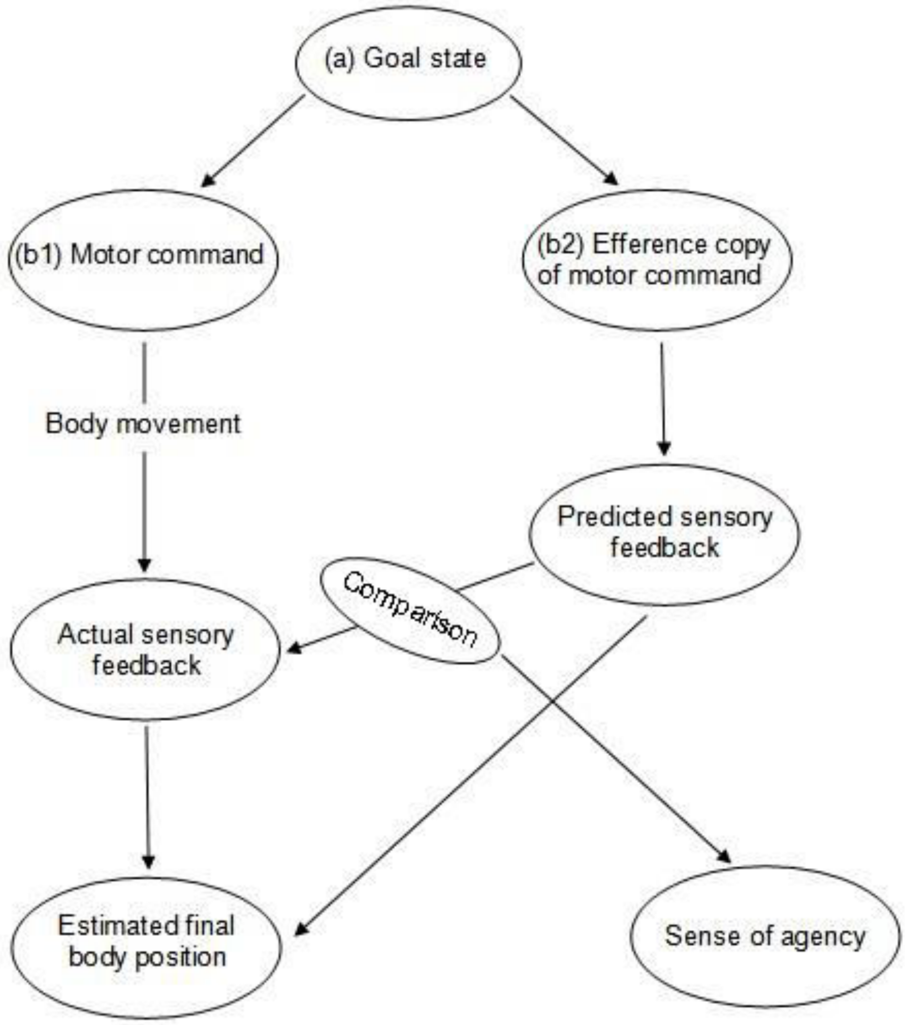
SENSE OF AGENCY

Underlying mechanisms

From a theoretical standpoint, sense of agency has been explained in terms of three broad approaches. The first of these approaches centres on motor prediction (the so called "comparator model"; Frith, Blakemore & Wolpert, 2000, see **Figure 1.1**) and has its origins in work conducted on the computational processes underpinning motor learning and control (for a review, see Wolpert, 1997). The comparator model posits that any voluntary action starts with a goal state (a) that goes on to produce a corresponding motor command (b1) and a concurrent efference copy of this motor command (b2). Motor command (b1) gives rise to a body movement. This changes the state of the motor system and produces sensory feedback. On the basis of these changes, the actual state of the system can be estimated. In parallel with this process, the efference copy of the motor command (b2) is used to produce an estimate of the predicted state of the system. This predicted state is useful for motor control, allowing rapid adjustment to motor commands so as to minimise error. According to Frith et al. (2000), the predicted state is also useful for sense of agency – the agent can compare the predicted state of the system with the actual state of the system and use this computation to determine its own causal role in bringing about these changes. If the predicted and actual states of the system match, then the agent treats these events as self-caused. If they do not match, then they treat these events as externally caused.

Figure 1.1

Frith and colleagues' (2000) comparator model of sense of agency

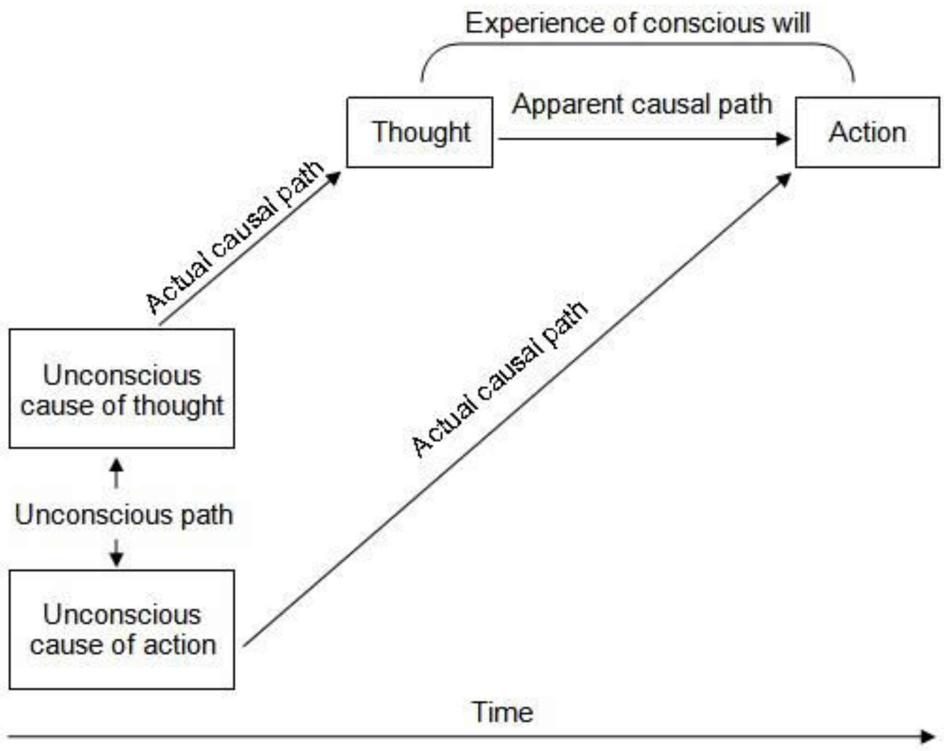


Note. The predicted sensory feedback is compared with the actual sensory feedback. If there is a match, sense of agency gets elicited.

The second approach (the so called "theory of apparent mental causation"; Wegner & Wheatley, 1999, see **Figure 1.2**) sits in contrast to the comparator model and focusses on contributions to sense of agency beyond the motor system. The theory of apparent mental causation suggests that we are susceptible to illusions of agency (such as the above-mentioned "placebo" control buttons) precisely because we *do not* have direct access to the workings of the motor control system. The only aspects of intentional action that we are aware of are the thoughts (intentions) prior to action, as well as the actions and outcomes themselves. According to this theory, it is the precise relationship between our intentions and actions that determines our sense of agency. Thus, we believe we caused an action to happen as long as our intention to act happens before the action, is consistent with the action and is the only apparent cause of the action. Wegner (2002) refers to these as *the principles of priority*, *consistency* and *exclusivity*.

Figure 1.2

Wegner and Wheatley's (1999) theory of apparent mental causation



Note. Will is experienced to the degree that an apparent causal path is inferred from thought to action.

The third approach that seeks to explain the mechanisms underlying human agency combines the comparator model with the theory of apparent mental causation (the so called "cue integration" approach; Moore & Haggard, 2008; Moore, Wegner & Haggard, 2009;

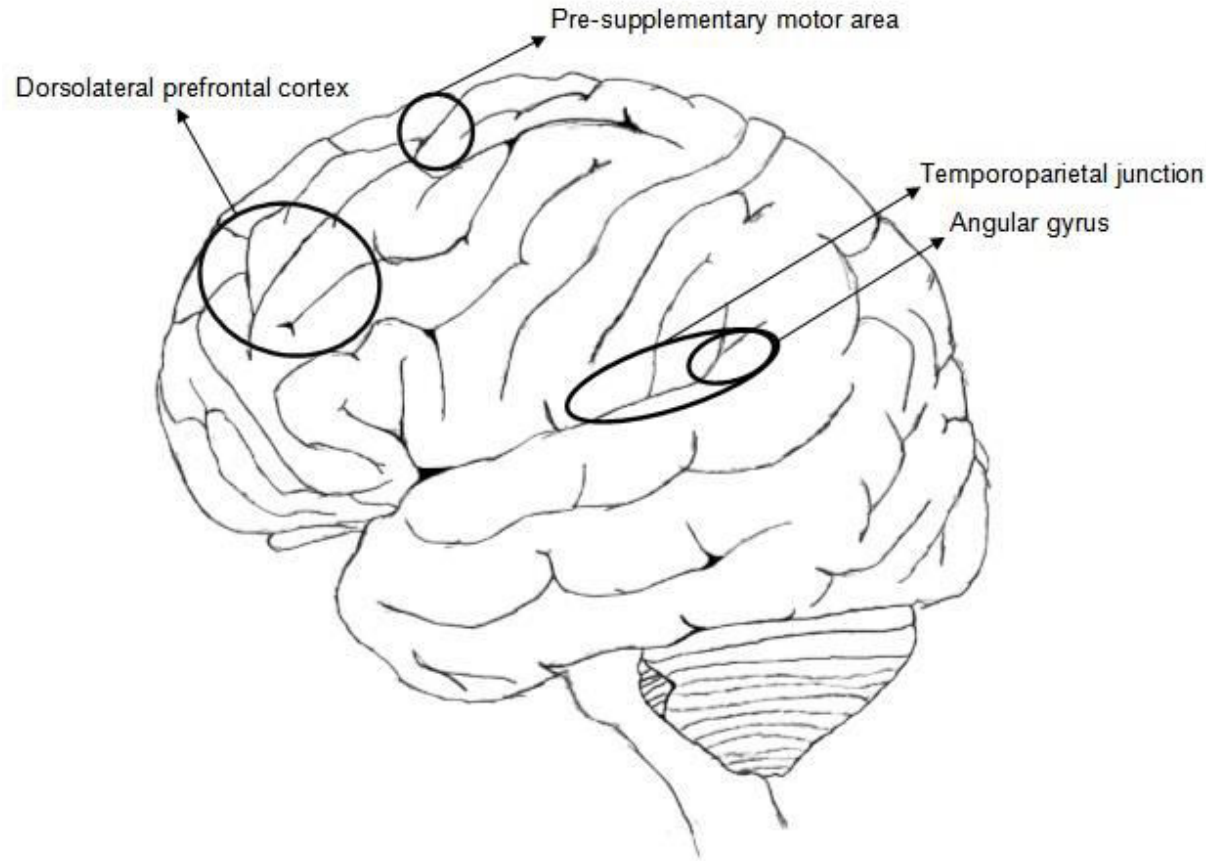
Synofzik, Vosgerau & Lindner, 2009; Moore & Fletcher, 2012). On this view, there are various sources of information that can contribute to sense of agency; these include motoric (e.g. sensorimotor prediction) and non-motoric sources of information (e.g. sensory feedback). These sources of information are combined in order to determine whether or not one experiences certain events as self-caused. Furthermore, it has been suggested that these sources of information are optimally combined in that the more reliable source of information exerts a stronger influence on the experience of agency. This cue integration framework has been successfully evoked by certain investigators to explain how the interplay between predictive and retrospective signals affects the experience of agency in neuro-typical (e.g. Moore & Haggard, 2008; Moore et al., 2009) and neuro-atypical individuals (e.g. Synofzik, Thier, Leube, Schlotterbeck & Lindner, 2010; Voss et al., 2010). Based on this, it is often argued that this approach offers a better explanation of the mechanisms underpinning human agency.

Neural basis

As can be seen in the section above, our sense of agency is a complex experience that consists of multiple components. We shall now turn our attention to some of the brain areas that appear to subserve some of these components (see **Figure 1.3**).

Figure 1.3

Some of the classic volition centres in the human brain



Note. The dorsolateral prefrontal cortex, pre-supplementary motor area, temporoparietal junction and angular gyrus are implicated, among other things, in action selection, the conscious experience of the intention to act, and conflicts that signal non- or spurious agency, respectively.

Haggard (2017) notes how a host of neuroimaging studies have identified certain brain areas that implement the various cognitive and computational processes underlying human agency. The aggregated findings from some of these studies point towards a division of labour between *the (pre)frontal* and *parietal cortices*, whereby the (pre)frontal cortex specialises in planning and initiating voluntary actions (Marmot et al., 1991; see also the role of *the dorsolateral prefrontal cortex* in action-selection, Khalighinejad, DiCosta & Haggard, 2016) and the parietal cortex is implicated in the management of conflicts that might arise during action-selection processes associated with frontal activity (Eimer & Schlaghecken, 2003; Chambon, Wenke, Fleming, Prinz & Haggard, 2013). These studies have shown, among other things, that there is an increased functional connectivity between *the angular gyrus* in the inferior parietal lobe and *the lateral prefrontal cortex* in scenarios in which participants' fluency of action selection is altered by means of subliminal priming paradigms.

In a similar vein, a meta-analysis has identified *the temporoparietal junction* (including the angular gyrus) as the neural correlate of "non-agency" (Sperduti, Delaveau, Fossati & Nadel, 2011), while other investigators have shown that it also responds to unexpected external sensory events in the absence of voluntary action (Kincade, Abrams, Astafiev, Shulman & Corbetta, 2005) or that it gets activated when participants judge visual feedback as unrelated to their own actions (Farrer & Frith, 2002; Farrer et al., 2003). These results paint a more-or-less complete picture of the role of the parietal cortex in sense of agency, whereby certain parietal areas subserve our capacity to detect when we are *not* the agents of our actions (Kincade et al., 2005; Sperduti et al., 2011; Farrer & Frith, 2002; Farrer et al., 2003) or when, even if we are, something along the causal chain that normally leads to our agentic experience has gone awry (Eimer & Schlaghecken, 2003; Chambon et al., 2013).

Another area that has been consistently associated with human agency is *the pre-supplementary motor area (pre-SMA)*. This area has been linked to higher-order cognitive aspects of self-generated action (Picard & Strick, 2001) and to the conscious experience of intending to act (Fried et al., 1991). Continuous theta-burst stimulation of the pre-SMA has been found to significantly decrease the outcome component of intentional binding (see below for more information on the intentional binding effect) (Moore, Ruge, Wenke, Rothwell & Haggard, 2010), this suggesting this area's role in pre-emptively connecting intentions with action outcomes.

So far, I have briefly surveyed the main theories of sense of agency, as well as the brain areas that are thought to subserve the sense of agency. Next, I focus on the challenges and practicalities associated with the measurement of our agentic experiences.

Measurement

Since its infancy, research into sense of agency has progressed considerably. However, its adequate measurement represents a persistent challenge. That is, in part, because of the lack of experimental control we have over voluntary actions. Voluntary actions, as opposed to reflex actions or other perceptual phenomena, are *necessarily* free from stimulation or “immediacy”, which results in a lack of experimental control (Haggard, 2008). One solution to this has been to constrain the degrees of freedom that the participant has (for example, by limiting the choice they have to *the timing* of an action, rather than *the choosing* of the action itself). This approach comes with certain drawbacks. For example, it can be argued that such methods do not address voluntary action at full capacity – the context participants find themselves in constrains their volitional capacities so, in this respect, the choice to act does not belong purely to them.

In addition to this, another challenge to the experimental manipulation of sense of agency is the failure of most paradigms to tap into the sense of value that normally characterises volition in real life (Haggard, 2008). Indeed, many agency studies can be criticised for failing to capture the reasons why participants should make those specific voluntary actions (yet see a more recent study by Khalighinejad, Schurger, Desantis, Zmigrod and Haggard (2017), where participants could indeed choose to make reason- or value-based actions, i.e. skip random or potentially long waits for stimulus onset in a perceptual decision task).

Despite these limitations, several measures of sense of agency have been developed, which go beyond the higher-level concerns summarised in the preceding paragraph and, instead, focus on capturing a key computational feature of voluntary action that *can* be successfully tested experimentally, namely the fact that individuals must themselves generate the information that is required to perform a voluntary act (Haggard, 2008). These measures can be split into two categories, depending on the aspect of agency that experimenters want to capture. According to the theoretical distinction drawn by Synofzik, Vosgerau and Newen (2008), the judgment of agency (also referred to as “explicit agency") involves the higher-order experience of recognising one's actions as pertaining to oneself. It is influenced by a general-purpose causal mechanism (i.e. "I caused this to happen") as well as by background information and contextual beliefs related to the action. Measures tapping into this kind of agency are based on participants' explicit conscious experience of their own agency. They typically require participants to make action recognition judgments in conditions of agentic uncertainty, judgments about the feedback of their movements or about the level of control they have over their movements' putative outcomes (Moore, 2016).

On the other hand, the feeling of agency (also referred to as "implicit agency") relates to low-level sensorimotor processes and represents the background buzz of control we feel over our actions and their effects. Measures aimed at capturing the feeling of agency do not rely on participants' explicit agentic awareness. Rather, they make inferences about the experience of agency by examining perceptual correlates of agentic actions. These include the perceived intensity of action outcomes (*sensory attribution paradigms*; Blakemore, Frith & Wolpert, 1999) and the perceived timing of actions and their effects (*intentional binding paradigms*; Haggard, Clark & Kalogeras, 2002).

Explicit agency methods face the usual challenges associated with self-reports (e.g. demand effects). For this reason, it is sometimes argued that paradigms such as intentional binding, which indirectly measure sense of agency, are better at capturing participants' actual volitional experience.

INTENTIONAL BINDING

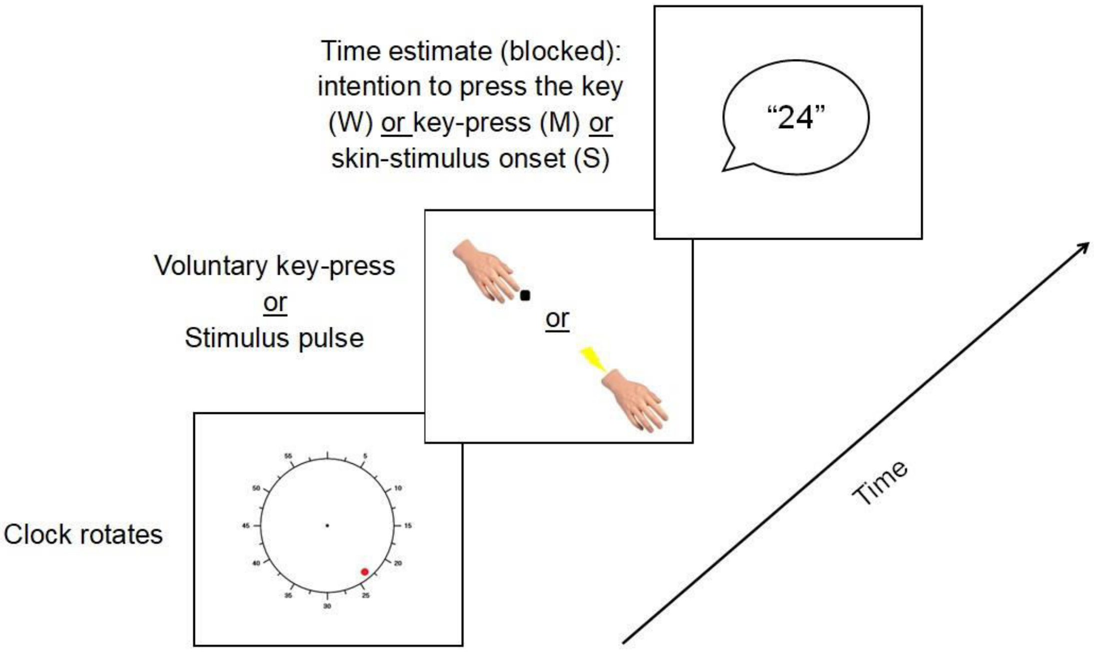
An implicit agency measure

Intentional binding is probably the most widely used measure of implicit agency (Moore, 2016). It has its origins in the work that Wilhelm Wundt conducted more than a century ago. He used a clock apparatus called "Komplikationspendl" (i.e. the complication clock) to explore individuals' time course of attention by asking them to attend to the position of a clock hand on a clock face each time they noticed the onset of an auditory stimulus and, afterwards, report the time of the stimulus's occurrence (Moore & Obhi, 2012).

A century later, Benjamin Libet revisited Wundt's work and adapted it to the study of human volition. In Libet, Gleason, Wright and Pearl's (1983) seminal study, participants sat in front of a clock face stimulus on which was a rotating spot instead of a conventional clock hand. The spot completed one revolution of the clock face every 2560ms and participants had to use its location on the clock's edge to make three types of chronometric judgments – W judgments (i.e. note the time they became aware of their intention to make an action), M judgments (i.e. note the time they made an action) or S judgments (i.e. note the time they became aware of an exogenous stimulus touch their skin) (see **Figure 1.4**).

Figure 1.4

Standard trial structure in Libet et al. (1983)

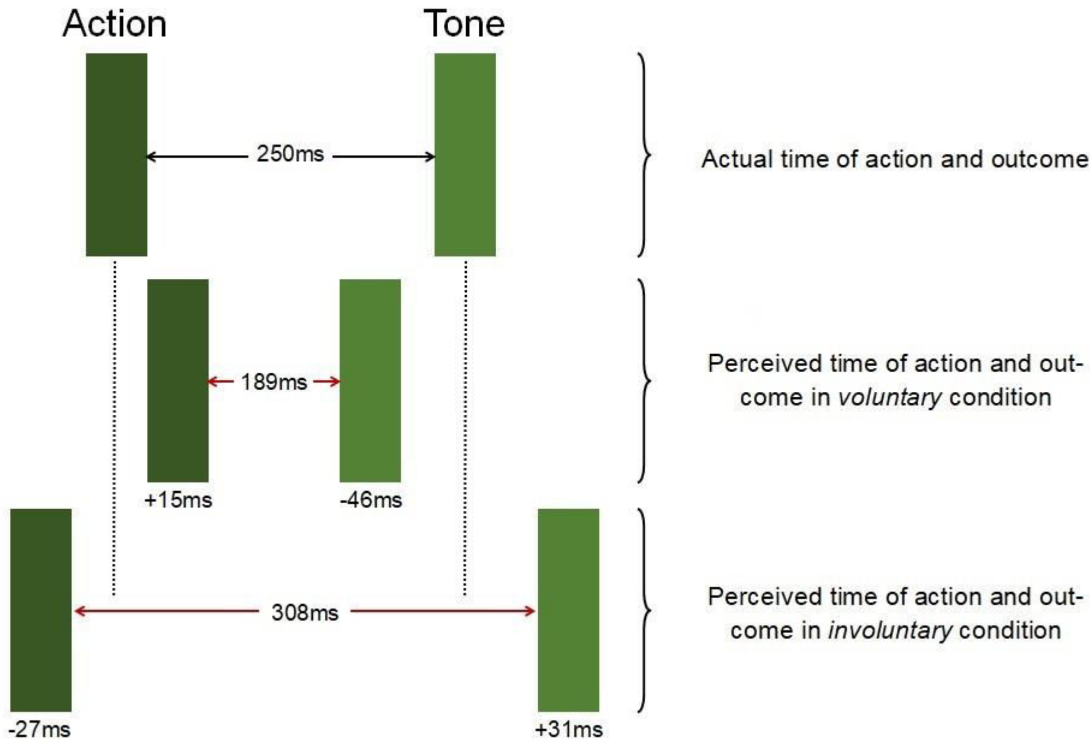


Note. Participants pressed a key at their own pace or received a near-threshold stimulus pulse on the back of their right hand. Depending on the condition type, they had to report the time they became aware of their intention to press the key (W), pressed the key (M) or felt the stimulus touch their skin (S).

This clock stimulus developed by Libet et al. (1983) was subsequently adopted by Haggard et al. (2002) to explore the perceived time of operant, or *agentic*, actions and their outcomes (in Libet et al.'s study the actions did not produce an outcome). It was found that when participants' actions systematically resulted in an outcome, they perceived the time of the action and that of the outcome to shift towards each other (see **Figure 1.5**). Importantly, this effect was attributed to voluntariness, as it was not found in a separate set of conditions where involuntary movements were induced using Transcranial Magnetic Stimulation (TMS). In these conditions, involuntary movements and their outcomes were associated with temporal repulsion, rather than binding (see **Figure 1.5**). This led the authors to suggest that subjective temporal attraction is an implicit marker of sense of agency. This effect has been christened "intentional binding" (sometimes referred to as "temporal binding").

Figure 1.5

Participants' judgments of actions and tones in a typical intentional binding experimental setup



Note. The time interval between voluntary as opposed to involuntary (i.e. TMS-induced) movements is perceived as being shorter (Haggard et al., 2002).

Underlying mechanisms

Ever since the discovery of this binding effect, certain authors have attempted to explain it by formulating different theories. For example, an early influential account of intentional binding postulates that intentional binding reflects a temporal contiguity prior for self-caused events (Eagleman & Holcombe, 2002). On this view, the presence of this prior, coupled with sensorimotor uncertainty, serves to bring actions and outcomes together in subjective experience. In other words, because sensorimotor uncertainty is inherent in our agentic experience (i.e. sensorimotor signals are noisy), we, as agents, fall back on this prior belief that dictates that our actions and their outcomes should be close to one another in space and time. This, in turn, produces the binding effect.

Another influential theory of binding has at its core the relationship between time perception and voluntary action. Wenke and Haggard (2009) have suggested that this relationship can be accounted for by an “internal clock” mechanism, whereby voluntary actions slow down an internal clock in anticipation of the action effects. This process, it is thought, decreases the number of internal clock cycles that occur between an action and its associated outcome, and this, in turn, produces the subjective temporal compression that characterises the binding effect.

Moving forward, the “pre-activation account” formulated by Waszak, Cardoso-Leite and Hughes (2012) to explain outcome binding is also relevant in the present context. This account has its origins in the ideomotor and common coding theories of action control (Lotze, 1852; Harleß, 1861; James, 1890), both of which posit that performing an action results in a bidirectional association between the action’s motor code and the action’s sensory consequences. The pre-activation account, thus, suggests that our actions pre-activate the representations associated with their outcomes. This pre-activation raises the neural activity in the perceptual units representing these outcomes to some pedestal level, which makes them reach awareness faster. This, then, results in outcomes subjectively shifting towards the actions that caused them.

Finally, a more recent account put forward by Lush et al. (2019) makes use of a cue integration approach to explain the binding effect. On these authors' view, the presence of a temporal contiguity prior for self-caused events results in action and action outcome timing estimates carrying information about each other. This information is then usefully combined so that the timing estimate reported for either actions or action outcomes represents a precision weighted average of the two events. Thus, a relatively highly precise action estimate will result in a less precise outcome estimate (this, in turn, resulting in the time of the outcome subjectively shifting towards that of the action) and vice versa.

Having briefly summarised the initial work on intentional binding, as well as some of the possible mechanisms that might explain it, I will now turn the lens on diverse paradigms that investigators have used throughout time to measure this phenomenon.

Common measurement paradigms

The intentional binding effect has been replicated using different methodologies that are different from the original Libet clock in a number of respects (e.g. the measurement apparatus itself, the dependent variable of interest). For example, *magnitude* and *interval estimation approaches* reliably reproduce the critical finding by requiring participants to simply estimate the length of the interval between their self-paced button-press and the ensuing tone. It has been found that participants' interval estimates are shorter for voluntary actions than involuntary movements (Humphreys & Buehner, 2009; Moore et al., 2009). Moreover, Cravo, Claessens and Baldo (2011) have successfully introduced a novel intentional binding paradigm inspired form Humphreys and Buehner (2009) that makes use of *simultaneity judgments*. This task asks participants to make an action that results in a tone which occurs simultaneously with an independent flash, and to judge whether the tone and the flash co-occur. Their results showed that, in agentic conditions (where the tone was reliably perceived as occurring earlier relative to its actual time of occurrence), the flash had to be presented even earlier than the tone for the two events to be perceived as simultaneous.

In addition to the above-mentioned methods, Humphreys and Buehner (2010) reproduced the binding effect at higher action-effect intervals than those found using the Libet clock. These authors designed *a temporal reproduction task* whereby participants are asked to reproduce the intervals between their key-presses and subsequent tones by holding down a key. They found that the intentional binding effect persisted at action-effect intervals in excess of 1200ms, which are much higher than the upper limit of the intervals in Haggard and colleagues' (2002) study (i.e. 650ms). This way of assessing intentional binding (i.e. by means of paradigms that measure participants' perceived duration of action-effect intervals) inspired Nolden, Haering and Kiesel (2012) to introduce a binding task adapted from *the psychophysical method of constant stimuli*. They asked participants to either press a key (the active condition) or hold their fingertips on a key that popped up (the passive condition). In both instances, the key generated the appearance of a square on the screen in front of them. They found that, in the active relative to the passive condition, participants perceived the key-press-square intervals as being shorter than they actually were.

Moving beyond novel intentional binding paradigms that make use of action-effect interval judgments, Cavazzana, Begliomini and Bisiacchi (2014) replicated the binding effect using *a visual alphabet paradigm* inspired from Soon, Brass, Heinze and Haynes (2008). This paradigm asks participants to view a stream of numbers and letters on the computer screen, make an action or hear a tone, and then report the letter that was on the screen at the time of the event of interest. Cornelio Martinez, Maggioni, Hornbaek, Obrist and Subramian (2018) took Cavazzani et al.'s (2014) work one step further, comparing this visual alphabet method of assessing intentional binding with several others, including a novel *auditory alphabet paradigm*, and they too reproduced the critical finding.

However, despite all these numerous innovative measures, the original Libet clock method (Libet et al., 1983; Haggard et al., 2002) often remains the preferred way of measuring intentional binding. This methodology is not without its issues, which I turn to in the next section.

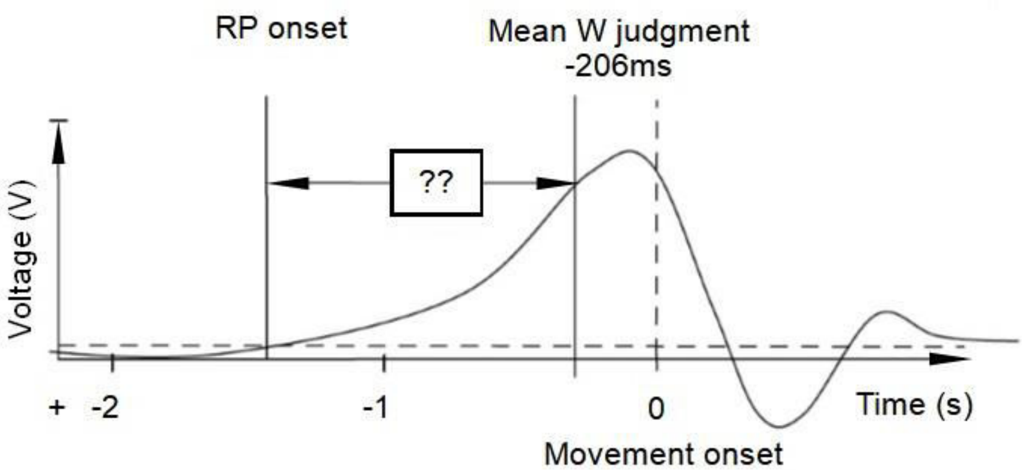
ISSUES WITH THE LIBET CLOCK METHOD

Issues with the Libet clock task

In Libet et al. (1983), whilst participants were making the temporal judgments of interest (i.e. W, M or S – see above), they were connected to an EEG machine that was recording activity from their scalps. The authors famously found that a preparatory cortical activity called the Readiness Potential (RP) (which was originally discovered by Kornhuber and Deecke in 1965 and termed "Bereitschaftspotential") always preceded the subjects' awareness of their intention to act (i.e. W) by several hundred milliseconds (see **Figure 1.6**). This central finding has, ever since then, sparked a controversy with respect to the existence of free will and the place intentionality has in voluntary action. Libet himself offered a possible solution to this problem – following W onset, there was an additional period of a few hundred milliseconds before movements were executed, during which, Libet argued, individuals could terminate (or veto) unconsciously initiated actions.

Figure 1.6

Schematic results of Libet and colleagues' (1983) findings following Haggard (2005a)



Note. Neural preparation in motor areas of the brain can begin up to 1s before movement onset. By contrast, the conscious experience of intending to move can begin much later (on average 206ms before movement onset). Based on this, Libet et al. (1983) argue that conscious intention cannot cause the neural processes that lead to action.

The implications of this finding have given rise to a myriad of critiques of Libet and colleagues' work. For example, certain authors have commented upon the ecological validity of the actions present in the Libet task (i.e. wrist flexes or button-presses) and, consequently, upon the sort of free will this task permits (e.g. Breitmeyer, 1985; Bridgeman, 1985). Other critics have discussed (e.g. Näätänen, 1985; Latto, 1985) and experimentally put to test (e.g. Keller & Heckhausen, 1990; Trevena & Miller, 2010; Schurger, Sitt & Dehaene, 2012) the meaning of the RP, or have addressed potential EEG artefacts present in the original study (e.g. Eccles, 1985; Fingo, 1985).

The validity and reliability of W judgments have also been criticised, with certain investigators deeming these judgments to be inaccurate, invalid or unreliable (e.g. Latto, 1985; Stamm, 1985; Salter, 1989; Gomes, 1998; Miller, Vieweg, Kruize & McLea, 2010) and others highlighting that the ability to make these judgments might be marred by humans' lack of awareness of the central processes that cause their own behaviour (e.g. Vanderwolf, 1985). Another criticism here is represented by certain biases that might have artificially pushed W judgments forward in time, away from the onset of the RP. *The representational momentum effect* (Hubbard & Bharucha, 1988) is one such bias that has been considered. When using a clock to make subjective temporal judgments, this effect refers to participants' tendency of under- or overestimating the location of the rotating spot on the clock face at the time of the event of interest in line with its counterclockwise or clockwise direction, respectively. Applied to W judgments, this result highlights how participants' judgments of their intentions to act were artificially pushed forward because the clockwise motion of the rotating spot caused them to overestimate these events.

Another bias that touches upon the perceptual burden of the Libet task is *the flash-lag effect* (Nijwahan, 1994). Experiments investigating the flash-lag effect ask participants to indicate the position of a moving spot the moment a nearby spot is being flashed. The typical finding is that the judged location of the moving spot is extrapolated into the future. One can see how this result too can be applied to W judgments – on this view, participants might have judged their intentions as occurring later in time (relative to the RP onset) because they extrapolated the location of the spot they used to time them into the future (i.e. further down the clock perimeter than the spot actually was at W time).

Lastly, somewhat similarly to the flash-lag effect, *the prior-entry phenomenon* (for a review, see Spence & Parise, 2010) suggests that the accuracy of W judgments might be marred by the tendency of attending more to the location of the rotating spot on the clock face (which is more salient) than to the precise onset of our intentions (which is less salient). In other words, when prompted with the dual task of paying attention to the moment they became aware of their intentions to act and reporting this awareness using the location of the spot on the clock face, participants might have switched their attention from the former, less taxing task component (i.e. the act of becoming aware of their intentions) to the latter, more taxing component (i.e. the act of reporting this awareness), this resulting in a lag that pushed W away from the RP onset.

Issues with the Libet clock stimulus

The biases explained in the preceding section point towards another problematic aspect of the Libet clock task – the clock stimulus itself. In the original Libet study, the clock rotated at a speed of 2560ms per revolution, was marked in steps of five (5, 10, 15, etc.) and in slightly more granular steps of 2.5 (2.5, 7.5, 12.5, etc.) and featured a diameter that had a visual angle of 1.8°. However, Libet and colleagues do not seem to have motivated their preference for these specific clock settings in any published work. This invites the question as to whether these clock configurations were set this way for a specific purpose. More importantly, this also invites the question as to whether these configurations affect the dependent variables (i.e. endogenous or exogenous event timing judgments) the clock is designed to measure. As far as this latter aspect is concerned, the specialised literature reveals that there is a controversy, whereby some authors seem to validate the clock as an experimental stimulus to probe timing and others cast doubt on its validity and reliability.

At one end of the spectrum, Pockett and Miller (2007) support the use of the Libet clock in subjective timing of events experiments, stating that it produces robust results. In their study, they set out to simultaneously manipulate seven different factors of the clock method to investigate these manipulations' influence on participants' *M judgments*. These factors were whether 1) participants had to act on a spontaneous urge vs make a definite decision to press the key, 2) they had to report the start vs the end of their key-presses, 3) they had to follow the spot round with their eyes vs gaze at the centre of the screen when making M judgments, 4) the rotating spot was light vs dark, 5) the clock radius was large vs small, 6) the spot radius was large vs small, and, finally, whether 7) the clock speed was fast vs slow. The authors found only the second factor (i.e. whether participants reported the start or end of their key-presses) to moderately affect participants' M judgments.

At the other end of the spectrum, some authors have shown, amongst other things, that the task of monitoring the clock can influence the capacity for subjective timing of events (e.g. because of the representantional momentum effect – Joordens, van Duijn & Spalek, 2002; Joordens, Spalek, Razmy & van Duijn, 2004) or further cloud participants' ability to temporally pinpoint events of interest (e.g. Lau, Rogers & Passingham, 2006; Danquah, Farrell & O'Boyle, 2008; Miller et al., 2010; Miller, Sheperdson & Trevena, 2011). For example, contrary to Pockett and Miller (2007), Lau et al. (2006) showed that the clock *can* significantly alter individuals' *M judgments*. Unlike Pockett and Miller's study (2007) (which varied parameters of the Libet clock to look at participants' behavioural performance), theirs kept the parameters of the clock constant but looked into the relationship between individuals' cognitive capacity for action timing and execution and the neural activity in the cingulate motor area (CMA), which is thought to subserve this capacity. They reasoned that above-threshold neural activation in the CMA should *positively* correlate with an enhanced rather than decreased ability to accurately time actions. However, their findings showed that neural enhancement in the CMA correlated with participants' action time estimates *negatively* – the more intense the activity in the CMA was, the more anticipatory (i.e. less accurate) participants' perceived times of action were (relative to the actual times of action). They concluded that the Libet clock is problematic because it alters neural representations of actions and, by extension, the perceived onset of actions.

A further issue with the Libet clock stimulus was highlighted by Danquah et al. (2008), who asked participants to make *S judgments* using a clock whose rotating spot completed a full revolution at increasingly higher speeds. The authors used the same clock speeds as Pockett and Miller (2007) (1280, 2560 and 5120ms per clock rotation). They found that participants' performance varied as a function of clock speed, which led them to suggest that individuals' chronometric capabilities are highly sensitive to changes to the clock stimulus settings.

The effect the Libet clock has on *W judgments* has also been covered. In a context dissimilar to that of the aforementioned three studies (a go/no-go reaction time task, rather than a purely volitional task), Miller et al. (2010) kept the Libet clock settings constant. However, they changed task instructions, asking participants to time their awareness of their intentions to act, the onset of their actions, the onset of low- or high-pitched tones they were presented with, or to not report any event at all. What is relevant to the rationale of my work is that they found performance costs to arise when participants made W judgments using the clock method (i.e. their W judgments were implausibly early or late – relative to their estimated actual time of occurrence – and not very accurate).

In a similar vein, Miller et al. (2011) investigated the way in which monitoring the clock affects the RP preceding participants' *W* or *tone judgments*. Across two different experiments, each containing the same two conditions, they recorded electroencephalographic activity from the participants' scalps while they were making movements (Experiment 1) or discriminating between low- or high-pitched tones (Experiment 2). In one condition, participants had to time the required events (i.e. their intentions to act or the tone-pitch discrimination) using the Libet clock. In the other, there was no clock or temporal judgment (i.e. participants just had to move – Experiment 1 – or discriminate between the two types of tones – Experiment 2 –). They found that participants' RPs were significantly more negative in the condition in which they had to use the clock to make W judgments or time the pitch of the tones.

GENERAL AIMS AND OBJECTIVES

From the summary above, it becomes apparent that the dispute revolving around the ability of the Libet clock stimulus to probe subjective timing of events merits further investigation.

As can be seen, so far authors have centred their efforts on addressing the effect the clock has on M, S, W and tone judgments, focussing generally on whether *the act of monitoring the clock* influences these judgments rather than on how *different clock configurations* might affect them. While Pockett and Miller (2007) and Danquah et al. (2008) have indeed tackled this latter research question in the context of M and S judgments, to date no authors seem to have done so in the context of W judgments or intentional binding. This is important for a host of reasons.

Firstly, because M and tone judgments form the basis of the intentional binding effect and, as we have just seen, they can be affected by different clock stimulus settings, it is probable that these clock manipulations might affect intentional binding too.

Secondly, both intentional binding and W judgments still remain widely used to measure implicit agency (e.g. Aytemur & Levita, 2020; Vastano, Ambrosini, Ulloa & Brass, 2020) and intention timing (e.g. Jo, Hinterberger, Wittmann & Schmidt, 2015; Moore & Bravin, 2015). Consequently, it is important for measures that address them to be able to depict them accurately.

Finally, a close inspection of the extant literature reveals that, typically, the precise Libet clock settings used in W judgments or intentional binding studies tend to vary (e.g. a wide range of clock markings – Libet et al., 1983; Haggard et al., 2002; Trevena & Miller, 2002; Lau et al., 2006 – or clock hand lengths – Engbert & Wohlschläger, 2006; Moore & Haggard, 2008; Capozzi, Bechio, Garbarini, Savazzi & Pia, 2016; Ruess, Thomascke & Kiesel, 2018a) or be under-reported (e.g. Walsh & Haggard, 2013; Khalighinejad & Haggard, 2015; Goldberg, Busch & van der Meer, 2017). Garaizar, Cubillas and Matute (2015) have attempted to tackle this variation by building an open-source Libet clock software that they have encouraged all investigators to use when conducting subjective timing of events or binding studies. However, this has not been widely adopted. This highlights how, if the above-mentioned variation in parameters is found to significantly affect the magnitude of W judgments or that of the binding effect, this can hinder replication attempts or introduce substantial confounds in the interpretation of results across the literature.

I believe all these reasons combined warrant a systematic investigation into the effect of various Libet clock parameters on intentional binding and W judgments. If this investigation finds that these parameters influence intentional binding and W judgments similarly to how they affect other basic time judgments (see above), then this would highlight these variables' sensitivity to the clock stimulus as well as the need for the use of uniform clock parameters and consistency in their reporting. Null effects, on the other hand, would also be worthwhile, as they would speak to the robustness of intentional binding and W judgments paradigms that use the clock stimulus and emphasise that different clock configurations can be creatively used to accommodate diverse experimental needs.

Beyond these practical implications, my thesis also has the potential to make valuable theoretical contributions to the agency literature as regards the neurocognitive origins of implicit sense of agency. That is because many manipulations of the clock stimulus imply subjecting participants to varying degrees of perceptual uncertainty. So, any significant effect on intentional binding might also be indicative of the putative relationship between uncertainty and human agency (Moore et al., 2009; Synofzik et al., 2009; Moore & Fletcher, 2012; Faro, McGill & Hastie, 2013). Crucially, this is a relationship that Eagleman and Holcombe (2002) propose underpins the binding phenomenon itself (see above).

In conclusion, the research carried out in this thesis sets out to go beyond the effort of the authors whose work I summarised at the beginning of this sub-section and see how variations in Libet clock settings (rather than the mere act of monitoring the Libet clock; see Lau et al., 2006 and Miller et al., 2010; 2011) systematically impacts on intentional binding and W judgments (rather than on M and S judgments; see Pocket & Miller, 2007 and Danquah et al., 2008). More specific aims and objectives are outlined below:

1. The aim of **Chapter 2** is to look into how different clock configurations (namely the clock speed, number of clock markings and length of the clock hand) that putatively induce varying degrees of perceptual uncertainty might affect intentional binding
2. **Chapter 3** investigates how different types of clock radius might influence intentional binding
3. **Chapter 4** goes beyond the clock stimulus itself to see how other facets of the Libet clock method, namely the action initiation instructions, might change the binding effect
4. **Chapter 5** looks into how all the different clock stimulus configurations mentioned above affect W judgments

The thesis concludes with a discussion on the aggregated theoretical and practical implications of all the key findings from the chapters above.

CHAPTER 2

Examining the effect of Libet clock stimulus parameters on intentional binding

ABSTRACT

Intentional binding refers to the subjective temporal compression between actions and their outcomes. It is widely used as an implicit agency index and it is commonly assessed by administering the task developed by Haggard et al. (2002) based on the Libet clock stimulus. Despite this task's popularity, it is unclear how sensitive the intentional binding effect is to the settings of the clock stimulus. Here, I present five experiments examining the effects of clock speed, number of clock markings and length of the clock hand on binding. My results show that the magnitude of intentional binding significantly increases with faster clock speeds yet remains non-significantly affected by the number of clock markings and by the clock hand length. I discuss the theoretical and practical implications of these results.

INTRODUCTION

As we have seen in **Chapter 1**, sense of agency refers to the feeling of control over actions and their effects (Haggard, 2005a) and, according to the *multifactorial two-step account* proposed by Synofzik et al. (2008), it is split between an explicit and an implicit component.

One widely used measure of implicit sense of agency is intentional binding (Haggard et al., 2002), which refers to the subjective temporal compression between voluntary (as opposed to involuntary) actions and their outcomes. Traditionally, intentional binding is measured using the Libet clock introduced by Libet et al. (1983). However, although this clock method is widely used in binding investigations (Moore & Obhi, 2012), it is not clear how and why the clock stimulus parameters were set as they were. More importantly, it is unclear whether and to what extent these clock stimulus parameters modulate intentional binding.

Libet clock stimulus parameters are important because there is a degree of inconsistency in the clock settings used across different studies. One such example is inconsistency in *clock markings*. Libet et al. (1983) used a clock marked at conventional intervals (5, 10, 15, etc.) with additional radial lines equally spaced between these intervals (2.5, 7.5, 12.5, etc.). However, other investigators used an unnumbered clock (e.g. Lau et al., 2006), a clock marked at conventional intervals only (e.g. Haggard et al., 2002), or a clock marked at 60 equally spaced positions (1, 2, 3, etc.) (e.g. Demanet, Muhle-Karbe, Lynn, Blotenberg & Brass, 2013). Another parameter that varies across the binding literature is *the rotating object length*. Some authors prefer to use shorter (e.g. 12mm – Engbert & Wohlschläger, 2006; Wenke, Waszak & Haggard, 2009; Takahata et al., 2012) and others longer clock hands (e.g. 20mm – Dogge et al., 2012; 22mm – Ruess et al., 2018a; 25mm – Desantis, Roussel & Waszak, 2011).

The use of uniform stimulus parameters across studies is important for replication purposes, particularly if changes in these settings have an effect on timing judgments, which form the basis of the intentional binding effect. Indeed, some investigations notice that this might be the case. For example, Danquah et al. (2008) looked into how manipulations of *the clock rotation speed* influenced S judgments. They used a spot marker that rotated at speeds of 1280ms, 2560ms or 5120ms per revolution. They found that participants' awareness of the somatosensory stimulus used was less anticipatory (relative to its actual time of occurrence) the faster the clock speed was.

Further to the issue of clock parameters influencing timing judgments, it is also possible that they might influence the expression of the intentional binding effect itself. As mentioned in **Chapter 1**, Eagleman and Holcombe (2002) proposed that intentional binding reflected a temporal contiguity prior for self-caused events. On this view, intentional binding is driven by the presence of this prior which, when coupled with sensorimotor uncertainty, serves to bring actions and outcomes together in subjective experience. Clock parameters might be relevant here, as less information content in the clock stimulus is likely to augment uncertainty. For example, speeding up the rotation speed of the clock hand could increase uncertainty, which might be expected to increase intentional binding (this reflecting an increased influence of the temporal contiguity prior).

In light of the inconsistency across studies in the Libet clock settings, coupled with evidence that stimulus parameters modulate timing estimates (Danquah et al., 2008), I conducted a systematic investigation of the impact of Libet clock stimulus parameters on intentional binding. In five experiments, I investigated the effect of variations in the clock speed, number of clock markings and length of the clock hand. My aim was to isolate Libet clock parameters in order to draw attention to the role of stimulus features in the expression of intentional binding. My overarching hypothesis was that information content would be inversely related to intentional binding.

EXPERIMENT 1

In this experiment, I investigated the influence of clock speed on intentional binding. We have already seen that this clock feature might affect basic timing judgments (Danquah et al., 2008). Moreover, if changing the clock speed has an effect on uncertainty, then, in light of Eagleman and Holcombe's (2002) temporal contiguity hypothesis, this may also have an influence on the expression of the binding effect. Following Danquah et al. (2008), I used three different rotation speeds – the clock rotated at 1280ms (fast), 2560ms (standard rotation speed) and 5120ms (slow) per clock revolution. In each clock speed condition, I ran the standard intentional binding procedure whereby participants are asked to either press a button that, across different blocks of trials, occurs in isolation or is followed by an outcome, or hear a tone generated by the computer. In each of these four conditions, participants were required to estimate the time either the button-press or the tone occurred. In line with Eagleman and Holcombe's (2002) hypothesis, I predicted that intentional binding would increase with increasing clock speed (i.e. with increasing uncertainty).

METHODS

Participants

I recruited 40 participants (M_{age} = 26.3yrs, *SD* = 8.64, age range: 18 – 61, 15 males) to this study using personal participant databases and the Goldsmiths Research Participation Scheme. They were compensated with £7 or 7 course credits for approximately 1hr30mins of experimental time. A survey of recent Libet-clock studies indicates a sample size of between 20 and 36 participants as being appropriate (e.g. Malik & Obhi, 2019 – *N* = 36; Christensen et al., 2019 – *N* = 20; Antusch et al., 2020 – *N* = 36). I used this as a guide for my own experiments. My final dataset contained 39 participants (M_{age} = 26.3yrs, *SD* = 8.75, age range: 18 – 61, 14 males) after one participant was excluded (see "Data analysis"). All participants were right-handed, had normal or corrected-to-normal vision, with no self-reported psychiatric or neurological disorders or substance usage that might interfere with their cognitive performance. They provided written informed consent prior to their participation and the experiment was approved by the Department Ethics Committee at Goldsmiths, University of London.

Materials

I measured intentional binding using a Libet clock task programmed in JAVA (version 6; ORACLE, 2011). The clock measured 21mm in diameter, featured a 9mm hand and was marked at conventional intervals (5, 10, 15, etc.) in all blocks and conditions. The tone in the baseline and both operant conditions (see below) was presented at 1,000 Hz and lasted for 100ms.

Procedure

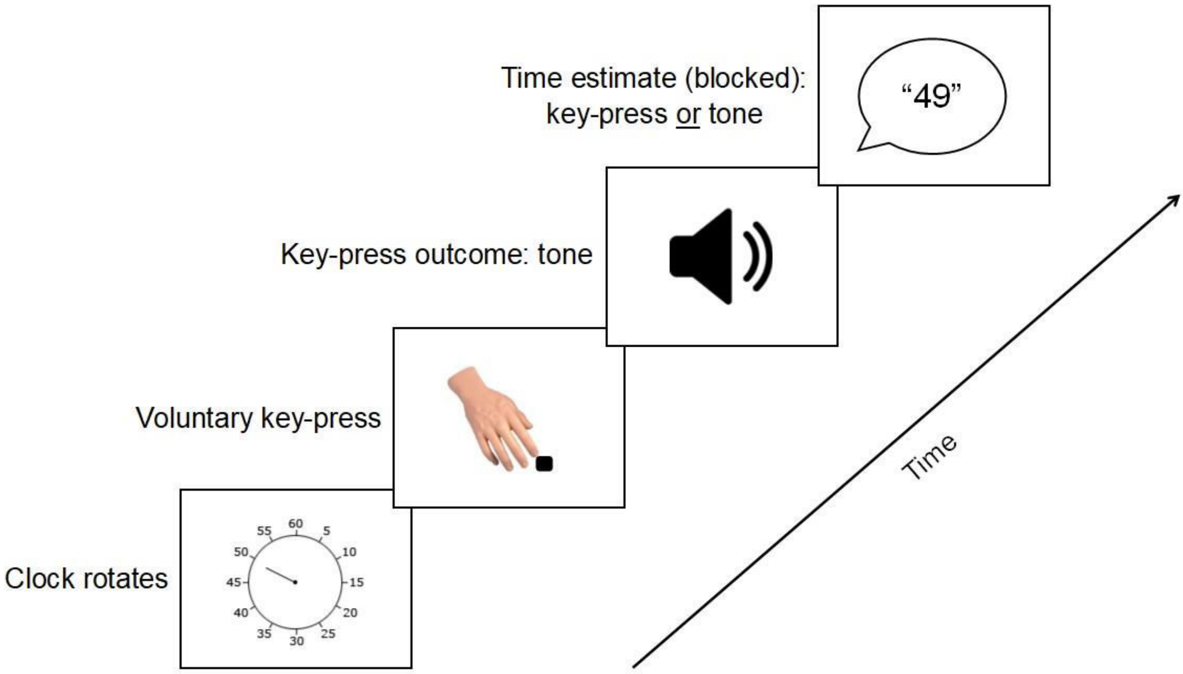
I presented participants with three different clock speeds across separate conditions (1280ms, 2560ms and 5120ms per clock revolution). As part of each clock speed condition, they completed four different binding blocks (operant action, operant tone, baseline action, baseline tone), each containing 30 trials, resulting in 12 blocks (360 trials) per participant.

The speed conditions were counterbalanced across participants and the binding blocks were fully randomised within participants.

Participants also completed three practice trials (2560ms speed) prior to each binding block. That resulted in 12 additional trials per participant, which were discarded. Afterwards they started the main task.

Figure 2.1

Standard trial structure in operant blocks following Haggard et al. (2002)



Note. Participants were required to press a key at their own pace, which was followed by a tone 250ms later. Depending on the operant block type, they had to report the time of their action or of the action outcome.

As can be seen in **Figure 2.1**, in the operant blocks, participants were required to press a pre-specified key whenever they felt the urge to do so. Their key-press was always followed by a tone after a fixed interval of 250ms. They had to estimate the position of the clock hand on the clock face when they pressed the key (operant action block) or heard the tone (operant tone block). Once the clock hand stopped rotating after a random delay following the tone (1000 – 2500ms), I required participants to verbally report the estimated clock time to me, and I inputted the number. I favoured this approach, rather than allowing participants to input their time estimates themselves, so as to encourage the participants' sustained attention on the task.

Participants also had to complete two baseline blocks. In the baseline action block, participants pressed the key whenever they felt the urge to do so and subsequently verbally reported the clock time corresponding to their key-press to me (the clock here stopped rotating after a random time between 1000 – 2500ms following the action). I specified to all participants that actions performed in the baseline block would never be followed by an outcome. In the baseline tone block, participants were instructed to pay attention to the location of the clock hand on the clock face when they heard a tone generated by the computer. This tone occurred at a random time between 2500 – 5000ms after trial onset and I again asked participants to report the perceived time of the tone to me.

Participants sat at a distance of approximately 65cm from the clock face across all speed conditions (visual angle: approximately 1.8°). I always informed participants of any change in clock speed before completing a new condition, gave them opportunities to rest in-between conditions and reminded them not to pre-plan their movements and be as accurate as possible when reporting their time estimates to me.

Data analysis

I calculated raw judgment errors as the perceived time minus the actual time of action or tone onset. This resulted in four raw judgment errors for the four binding blocks. I computed *Action binding* as the mean operant – mean baseline action judgment error and *Tone binding* as the mean operant – mean baseline tone judgment error. I computed these measures for each clock speed condition.

I excluded individual trials containing raw judgment error outliers within participants (M +/-2.5 SDs). This criterion resulted in 1.85%, 1.92% and 1.83% trials excluded across all four binding blocks in the 1280ms, 2560ms and 5120ms clock speed condition, respectively. I removed participant outliers at the group level if a combination of factors indicated univariate or multivariate outliers (Field, 2013; these included skewness and kurtosis values greater than approximately +/-2.000, significant results rendered by the Kolmogorov-Smirnov test, visual inspection of boxplots and histograms). This criterion resulted in the exclusion of one participant.

Statistical analyses

I conducted all data and statistical analyses using MATLAB (v. R2012a, MathWorks, Natick, MA) and IBM SPSS Statistics (v. 22 & 23; 2014, 2018). After excluding the above-mentioned participant, the results of the Kolmogorov-Smirnov test indicated that all but two factors were normally distributed. Closer inspection of the skewness and kurtosis values, histograms and boxplots of the factors in question raised no major issues, so I proceeded forward using parametric analyses (which are relatively robust against these minor perturbations in normality; Field, 2013). Thus, I ran a 3 x 2 repeated-measures ANOVA with Clock speed (1280ms, 2560ms, 5120ms) and Event (Action binding, Tone binding) as within-subject factors and mean baseline-corrected judgment error as a dependent variable.

Results

Participants' mean action and tone timing scores across all four baseline and operant blocks, as well as their mean binding scores across all three clock speed conditions are shown in **Table 2.1**.

Table 2.1

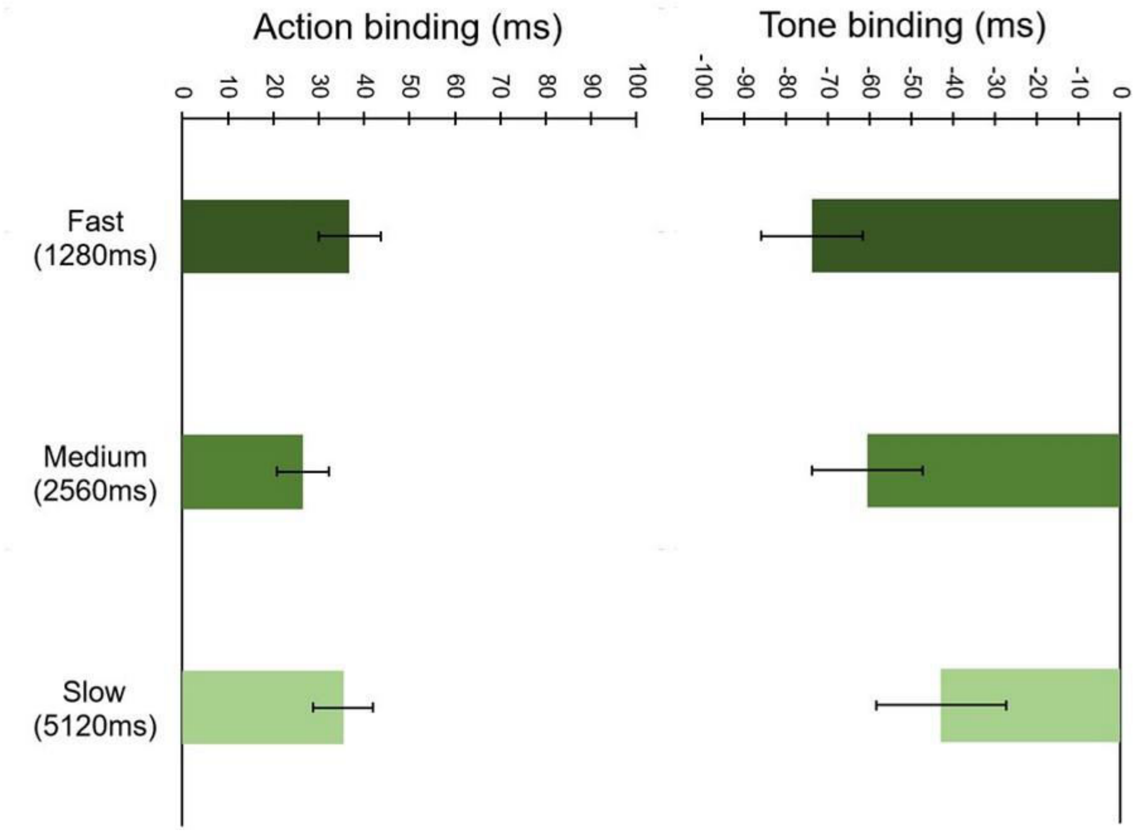
Participants' mean raw and baseline-corrected action and tone judgment errors across all three clock speed conditions in Experiment 1

Rotation speed condition (ms/revolution)	Judged event		Mean raw judgment error (ms) (SD)	Mean shift from baseline (ms) (SD)
Fast (1280ms)	Action	baseline	-23.25 (68.30)	36.80 (42.92)
		operant	13.54 (79.20)	
	Tone	baseline	28.09 (56.65)	-73.78 (76.52)
		operant	-45.69 (100.06)	
Medium (2560ms)	Action	baseline	-7.72 (72.28)	26.62 (35.78)
		operant	18.90 (84.81)	
	Tone	baseline	16.29 (75.17)	-60.49 (82.62)
		operant	-44.19 (108.28)	
Slow (5120ms)	Action	baseline	11.75 (72.61)	35.38 (41.71)
		operant	47.13 (78.62)	
	Tone	baseline	26.79 (79.97)	-42.88 (97.44)
		operant	-16.09 (124.52)	

The binding data for each clock speed and event type are shown in **Figure 2.2**. I found no significant main effect of Speed, $F(2, 76) = 2.26$, $p = .111$, $\eta_p^2 = .056$, and an expected significant main effect of Event, $F(1, 38) = 55.66$, $p < .001$, $\eta_p^2 = .594$, reflecting that action and tone binding scores were significantly different from each other. Importantly, I found an ambiguous, non-significant Speed x Event interaction, $F(2, 76) = 2.71$, $p = .073$, $\eta_p^2 = .067$. This suggests that Speed might have affected action and tone binding differently, yet this is inconclusive.

Figure 2.2

Participants' performance as a function of clock speed in Experiment 1



Note. Error bars depict SE.

In order to follow this interaction up, I conducted a repeated-measures ANOVA with Clock speed (1280ms, 2560ms, 5120ms) as a within-subject factor and mean baseline-corrected *action* judgment error as a dependent variable. There was no significant main effect of Speed on Action binding scores, $F(2, 76) = 0.93$, $p = .396$, $\eta_p^2 = .024$.

A subsequent repeated-measures ANOVA with Clock speed (1280ms, 2560ms, 5120ms) as a within-subject factor and mean baseline-corrected *tone* judgment error as a dependent variable revealed a trend-level main effect of Speed on Tone binding, $F(2, 76) = 3.12$, $p = .050$, $\eta_p^2 = .076$. Bonferroni-corrected pairwise comparisons examining the differences in tone binding scores as a function of clock speed showed that Tone binding ambiguously differed between the 1280ms and 5120ms conditions, $p = .067$, Cohen's d = -

0.381, whereas it was non-significantly different between the 1280ms and 2560ms, p = .936, Cohen's d = -0.164, and the 2560ms and 5120ms conditions, p = .370, Cohen's d = -0.252. This suggests there is a trend for the change in speed from extremely slow (5120ms) to extremely fast (1280ms) to increase Tone binding, and it might be this effect that drives the inconclusive Speed x Event interaction reported above.

DISCUSSION

I manipulated the rotation speed of the Libet clock to investigate if a faster clock speed would increase intentional binding. Despite observing robust binding effects, binding was not significantly altered by the speed manipulation. Nevertheless, there was weak evidence that intentional binding varied with clock speed. In particular, the direction of this ambiguous effect was partly in the predicted direction – although action binding was not significantly increased by an increase in clock speed, tone binding did show this trend, being larger in the fastest as opposed to the slowest speed condition.

EXPERIMENT 2

This experiment consisted of a direct replication of Experiment 1 in order to resolve the ambiguous Speed x Binding interaction. Based on the results reported above, I expected an interaction between binding and clock speed driven by an increase in tone binding as a function of an increase in clock speed.

METHODS

Participants

Experimenter M. G. recruited 40 participants (M_{age} = 20yrs, SD = 2.1, age range: 18 – 26, 8 males) using the Goldsmiths Research Participation Scheme. The sample size was based on the same rationale as reported in Experiment 1. They were compensated with 7.5 course credits for approximately 1hr30mins of experimental time. I applied the same participant inclusion criteria as I did in Experiment 1. Participants provided written informed consent prior to their participation and the experiment was approved by the Department Ethics Committee at Goldsmiths, University of London.

Materials, Procedure & Data analysis

The materials, procedure and data analysis protocols were identical to those of Experiment 1. I excluded 1.95% trials from the 1280ms, 1.83% from the 2560ms and 1.97% from the 5120ms clock speed condition. No participants were excluded this time.

Statistical analyses

The Kolmogorov-Smirnov test did not render any significant results. I also did not find any abnormal skewness and kurtosis values, boxplots or histograms, so I proceeded forward using parametric statistics. Based on the ambiguous results of Experiment 1, I used simple contrasts when investigating one key effect (i.e. the difference in tone binding scores between the 1280ms and 5120ms clock speed conditions) and Bonferroni-corrected post-hoc tests when examining all other mean differences.

In the case of one analysis below, Mauchly's Test revealed that the assumption of sphericity was violated, so I report the Greenhouse-Geisser correction.

Due to the ambiguity present in Experiment 1, when investigating the interaction between clock speed and intentional binding as a whole and the effect of extreme speeds on tone binding, I supplemented frequentist analyses with four Bayes Factors (BFs). BFs represent a measure of the relative likelihood of one hypothesis relative to the other given the available data and can aid the interpretation of non-significant or ambiguous results (Dienes, 2014).

Following Dienes (2014), priors for the three BFs for the Speed x Binding interaction were derived by splitting the 3 (Clock speed: 1280ms, 2560ms, 5120ms) x 2 (Event: Action binding, Tone binding) 2-degree of freedom effect from Experiment 1 into three subsidiary 2 (Clock speed) x 2 (Event) 1-degree of freedom effects involving all two-level speed comparisons (1280 vs 2560ms; 2560 vs 5120ms; and 1280 vs 5120ms). In each case, I computed the mean difference in action and tone binding as a function of clock speed and summed these differences to form an omnibus clock speed effect on binding (collapsed across action and tone binding) for each clock speed difference. Finally, I computed the mean difference in tone binding between extreme clock speeds (1280 vs 5120ms) in Experiment 1. These four values were subsequently used as priors for the assessment of different clock speed effects on binding in Experiment 2.

Each BF was calculated for the M and SE of the effect of interest in Experiment 2 using a half-normal distribution with 0 as the mean and the magnitude of the effect in Experiment 1 as the SD. Following convention, I interpreted BFs greater than 3 as providing moderate (or greater) evidence for the alternative hypothesis, less than 0.33 as moderate evidence for the null hypothesis, and between 0.33 – 3 as insensitive evidence (Dienes, 2011; Jeffreys, 1961).

Results

Participants' mean performance across all binding blocks and clock speed conditions is depicted in **Table 2.2**.

Table 2.2

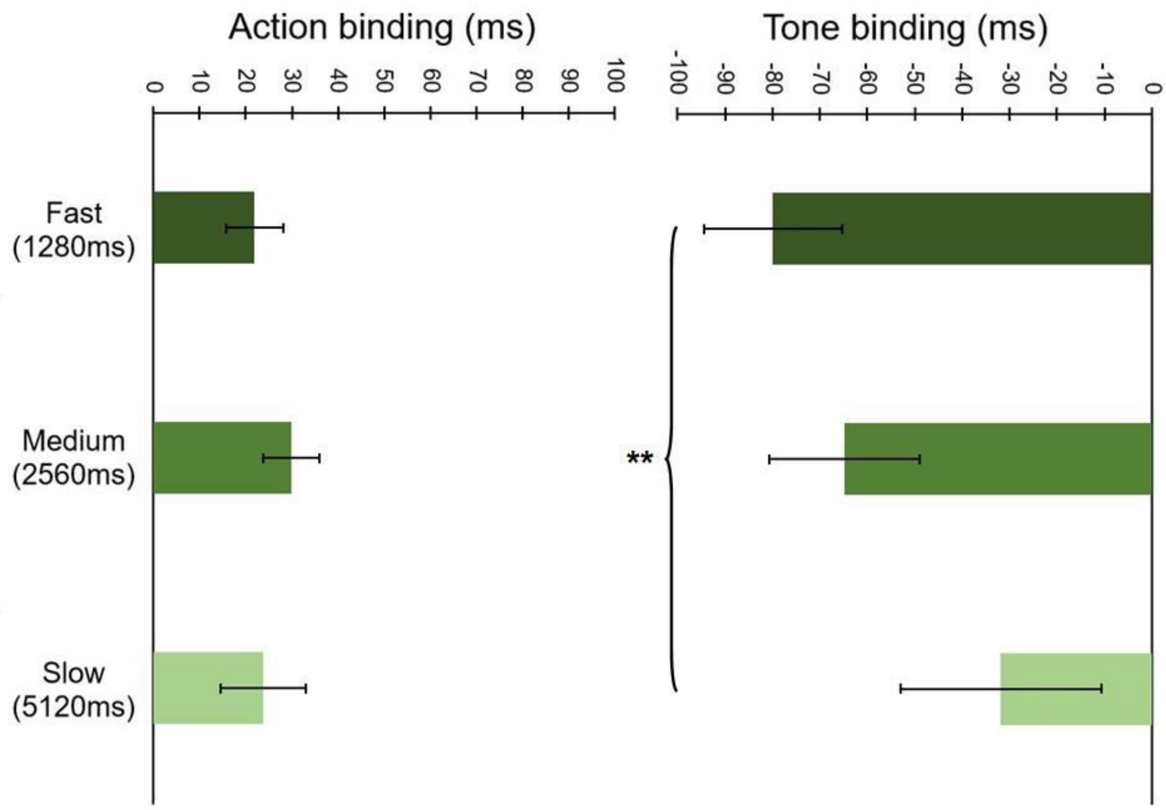
Participants' mean raw and baseline-corrected action and tone judgment errors across all three clock speed conditions in Experiment 2

Rotation speed condition (ms/revolution)	Judged event		Mean raw judgment error (ms) (SD)	Mean shift from baseline (ms) (SD)
Fast (1280ms)	Action	baseline	-28.45 (55.55)	21.89 (38.87)
		operant	-6.56 (57.58)	
	Tone	baseline	-30.79 (52.63)	-79.91 (92.08)
		operant	-110.71 (114.46)	
Medium (2560ms)	Action	baseline	-38. 57 (70.07)	29.80 (38.66)
		operant	-8.77 (64.29)	
	Tone	baseline	-38.31 (62.10)	-64.84 (100.33)
		operant	-103.15 (133.59)	
Slow (5120ms)	Action	baseline	-9.82 (77.43)	23.74 (58.18)
		operant	13.91 (87.33)	
	Tone	baseline	-33.63 (73.19)	-31.80 (133.47)
		operant	-65.43 (164.59)	

In **Figure 2.3**, participants' mean action and tone binding scores are plotted as a function of clock speed. Unlike in Experiment 1, here I found a significant main effect of Speed, $F(2, 78) = 4.94$, $p = .010$, $\eta_p^2 = .112$. I again found a significant main effect of Event, $F(1, 39) = 31.76$, $p < .001$, $\eta_p^2 = .449$. Crucially, this time I also observed a significant Speed x Event interaction, $F(2, 78) = 4.29$, $p = .017$, $\eta_p^2 = .099$.

Figure 2.3

Participants' performance as a function of clock speed in Experiment 2



Note. Error bars depict SE. ** indicates a significant difference between tone binding in the 1280ms vs 5120ms condition ($p < .01$).

Two further repeated-measures ANOVAs examined the influence of clock speed on action and tone binding separately. As in Experiment 1, there was a non-significant main effect of Speed on Action binding, $F(2, 78) = 0.47$, $p = .624$, $\eta_p^2 = .012$. By contrast, there was a significant main effect of Speed on Tone binding, $F(2, 78) = 6.08$, $p = .003$, $\eta_p^2 = .135$, reflecting that tone binding was larger in the 1280ms than in the 5120ms condition, $F(1, 39) = 10.18$, $p = .003$, $\eta_p^2 = .207$, thereby replicating this observation from Experiment 1. Moreover, a close inspection of **Table 2.2** reveals that this increase in tone binding is driven by changes in the perceived time of operant (rather than baseline) tones. Tone binding did not significantly differ between the other speed conditions, 1280ms vs 2560ms, p = .599, Cohen's d = -0.206, and 2560ms vs 5120ms, p = .114, Cohen's d = -0.340.

Computation of Bayes factors (BFs) for the Speed x Binding interaction revealed that the differences between the 1280ms and 2560ms, and the 2560ms and 5120ms clock speed conditions were insufficiently sensitive to corroborate the null hypothesis that these clock speeds do not affect action and tone binding differently, $BF_{[0, 23.47]}$ = 0.77 and $BF_{[0, 8.85]}$ = 2.47, respectively. By contrast, the difference between the 1280ms and 5120ms conditions seems to have done so, $BF_{[0, 32.32]}$ = 10.31. Cumulatively, these results show a relationship between manipulations of the Libet clock speed and intentional binding that seems to be driven primarily by the effect of extreme speeds on tone binding.

The BF I calculated to probe the difference in tone binding between the extreme ends of the clock speed spectrum revealed strong evidence in favour of a difference in tone binding between the 1280ms vs the 5120ms speed condition, $BF_{[0, 30.9]}$ = 53.36.

Together, these results suggest that clock speed affects action and tone binding differently and that this effect is primarily driven by changes in tone binding, which, as hypothesised, increased in the fast relative to the slow clock speed condition.

DISCUSSION

This direct replication of Experiment 1 resolves the ambiguity present in Experiment 1. I found a significant interaction between clock speed and binding, suggesting that action and tone binding are sensitive to manipulations of the measurement stimulus in a different way. More specifically, tone rather than action binding seems to be more sensitive to these manipulations – I found tone binding to increase linearly with an increase in clock speed and significantly differ between the extremities of the clock rotation speed. These results were strengthened by Bayes Factors, which brought support in favour of the above-mentioned interaction and, most especially, in favour of greater tone binding in the 1280ms as compared with the 5120ms condition.

Taken together, the results of Experiment 1 and 2 suggest that clock speed has an effect on intentional binding. This result emphasises the need for consistency across studies in the setting of clock speed. Moreover, it also sheds light on the potential mechanisms underpinning binding. Eagleman and Holcombe (2002) proposed that intentional binding reflects a temporal contiguity prior for self-caused events, and my results are in line with this proposal – the greater the uncertainty (with increasing clock speed), the stronger the binding effect. This also implies that the expression of the binding effect itself is dependent, at least to some extent, on stimulus parameters, warranting a consideration of other stimulus parameters that might similarly function as a further source of perceptual uncertainty.

EXPERIMENT 3

In this experiment I investigated the effect of clock markings on intentional binding. Unlike clock speed, this parameter varies considerably across studies (e.g. Libet et al., 1983; Haggard et al., 2002; Lau et al., 2006; Demanet et al., 2013). I measured intentional binding using two different clock marker settings: the standard variant of the Libet clock marked at conventional intervals (5, 10, 15, etc. – *condition 5*') and a variant where the clock was marked more granularly (1, 2, 3, etc. – *condition 5' + 1'*). Clock markings were also expected to influence uncertainty such that uncertainty should be greater in the 5' condition. In turn, I predicted that intentional binding would be greater in the 5' condition.

METHODS

Participants

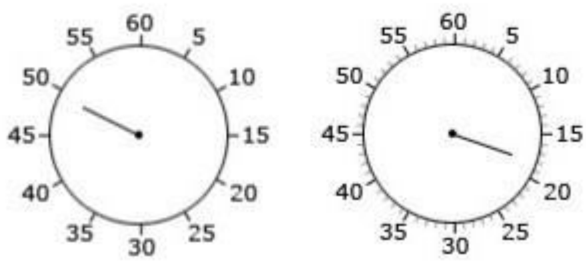
I recruited 40 participants (M_{age} = 30.15yrs, *SD* = 9.25, age range: 18 – 61, 11 males) using personal participant databases and the Goldsmiths Research Participation Scheme. I elected this sample size in a similar way as I did in both experiments reported above. They were compensated with £7.5 or 5 course credits for approximately 1hr of experimental time. The final dataset contained 39 participants (M_{age} = 30.25yrs, *SD* = 9.35, age range: 18 – 61, 10 males) after one participant exclusion was made (see “Data analysis”). I applied the same participant inclusion criteria as in Experiments 1 and 2. All participants provided written informed consent prior to participation and the experiment was approved by the Department Ethics Committee at Goldsmiths, University of London.

Materials

I measured intentional binding using the same Libet clock task as reported above. The clock rotated at the standard speed of 2560ms per revolution, featured a 9mm hand and measured 21mm in diameter. Across all relevant conditions, the tone was presented at 1,000 Hz and lasted for 100ms. Crucially, this time, I used two types of clock markings across two separate conditions (5’ – the clock was marked in steps of five; 5’ + 1’ – the clock was marked in steps of five and more granular steps of one; see **Figure 2.4**).

Figure 2.4

The two types of clock markings (5’ – left, 5’ + 1’ – right) used across all experimental conditions in Experiment 3



Procedure

I used the same procedure as in Experiments 1 and 2 (see **Figure 2.1**).

Data analysis

I used the same data analysis methods as in the previous two experiments. This time, across all four binding blocks, I excluded 2.07% trials from the 5' and 1.92% from the 5' + 1' clock markings conditions. I excluded one participant as an outlier on the basis of kurtosis scores and boxplot inspection.

Statistical analyses

After I excluded the above-mentioned subject, the results of the Kolmogorov-Smirnov rendered significant results in the case of one factor alone. Yet, I found no abnormal skewness and kurtosis values or histograms, so I proceeded forward using parametric statistics (Field, 2013). I used the same type of frequentist analyses as I did in Experiments 1 and 2. That is, I conducted a factorial 2 x 2 repeated-measures ANOVA with Clock markings (5', 5' + 1') and Event (Action binding, Tone binding) as within-subject factors and mean baseline-corrected judgment error as a dependent variable.

Results

Participants' mean action and tone timing scores across all four baseline and operant blocks, as well as their mean binding scores across both clock markings conditions, are shown in **Table 2.3**.

Table 2.3

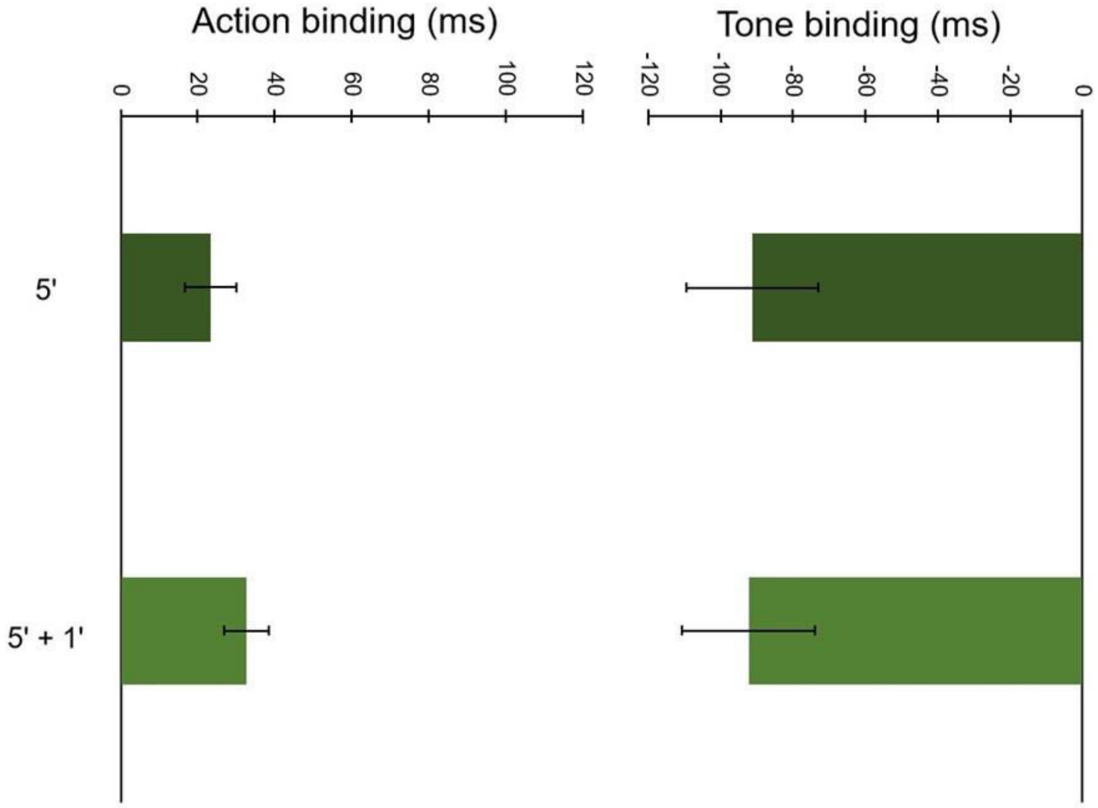
Participants' mean raw and baseline-corrected action and tone judgment errors across both clock markings conditions

Clock markings condition	Judged event		Mean raw judgment error (ms) (SD)	Mean shift from baseline (ms) (SD)
5'	Action	baseline	-2.68 (89.33)	23.29 (41.81)
		operant	20.60 (87.22)	
	Tone	baseline	5.03 (75.54)	-91.36 (113.65)
		operant	-86.33 (136.47)	
5' + 1'	Action	baseline	-22.73 (82.24)	32.54 (35.85)
		operant	9.80 (86.86)	
	Tone	baseline	4.33 (71.36)	-92.31 (114.57)
		operant	-87.97 (135.83)	

A close inspection of **Table 2.3** and **Figure 2.5** reveals a robust intentional binding effect that did not differ a great deal as a function of changes in clock markings. There was a non-significant effect of Clock markings, $F(1, 38) = 0.35$, $p = .557$, $\eta_p^2 = .009$, and an expected significant main effect of Event, $F(1, 38) = 51.01$, $p < .001$, $\eta_p^2 = .573$, reflecting the different directions of shift for Action binding (positive) and Tone binding (negative). The interaction between Clock markings and Event was non-significant, $F(1, 38) = 0.57$, $p = .451$, $\eta_p^2 = .015$. These results suggest that manipulations of clock markings do not influence binding as a whole or action and tone binding differently.

Figure 2.5

Participants' performance as a function of clock markings in Experiment 3



Note. Error bars depict SE.

DISCUSSION

In this experiment I examined the impact of clock markings on intentional binding. Contrary to my prediction, intentional binding did not significantly vary across marking conditions. This suggests that this source of perceptual uncertainty might not influence the expression of intentional binding. More broadly, these results further suggest that inconsistencies in clock markings across the literature are unlikely to be problematic. Nevertheless, it is possible that my manipulations were not salient enough to impact intentional binding. To probe this possibility further, in the next experiment I presented participants with more extreme manipulations of clock markings.

EXPERIMENT 4

This experiment examined the impact of different clock markings configurations on intentional binding. To this end, participants completed the intentional binding task in three separate clock marking conditions: 1) no markings, 2) markings at 30' intervals, and 3) markings at 15' intervals (see **Figure 2.6**). In line with the uncertainty hypothesis (Eagleman & Holcombe, 2002), I predicted that intentional binding would increase with a decrease in the number of markings such that the greatest binding effect would be observed in the "No markings" condition.

METHODS

Participants

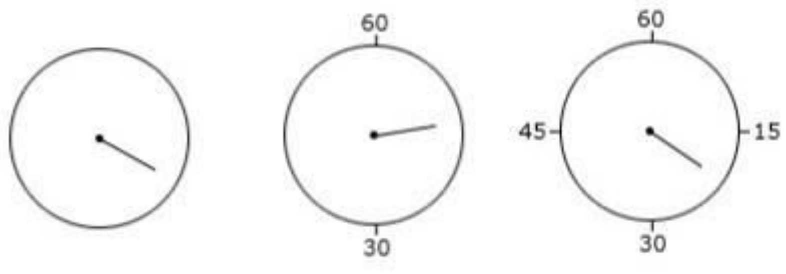
I recruited 40 participants (M_{age} = 21.35yrs, *SD* = 5.96, age range: 18 – 36, 6 males) to participate in this study using personal participant databases and the Goldsmiths Research Participation Scheme. I based my sample size on criteria identical to those I used in Experiments 1, 2 and 3. They were compensated with £10 or 7.5 course credits for approximately 1hr30mins of experimental time. The final dataset included 36 participants (M_{age} = 21.02yrs, *SD* = 5.87, age range: 18 – 36, 6 males) after four participants exclusions were made (see "Data analysis"). I used the same participant inclusion criteria as with the previous experiments above. Participants provided written informed consent prior to participation and the experiment was approved by the Department Ethics Committee at Goldsmiths, University of London.

Materials

I measured intentional binding using the same Libet clock task as in Experiment 3. This time, I used three types of clock markings across three separate conditions (No markings – the clock was not marked at all, 30' – the clock was marked in steps of 30, 15' – the clock was marked in steps of 15; see **Figure 2.6**).

Figure 2.6

The three types of clock markings (No markings – left, 30' – centre, 15' – right) used across all experimental conditions in Experiment 4



Procedure

The procedure was identical to the one I used in Experiment 3.

Data analysis

The data analysis protocol was identical to that of the previous experiments. Across all four binding blocks, I excluded 1.57% trials from the No markings, 1.82% from the 30' and 1.85% from the 15' condition. I also excluded four participants that were identified as outliers based on boxplot inspection.

Statistical analyses

After the above-mentioned participant exclusions were made, the results of the Kolmogorov-Smirnov test rendered ambiguously significant results in the case of one factor. However, a visual inspection of the skewness and kurtosis values and of the histograms of all factors did not render any problems, so I proceeded forward using parametric statistics (Field, 2013). Thus, I analysed the data using a 3 x 2 repeated-measures ANOVA with Clock markings (No markings, 30', 15') and Event (Action binding, Tone binding) as within-subject factors and mean baseline-corrected judgment error as a dependent variable.

Results

Participants' mean action and tone timing scores across all baseline and operant blocks, and their mean binding scores across all clock markings conditions are shown in **Table 2.4**.

Table 2.4

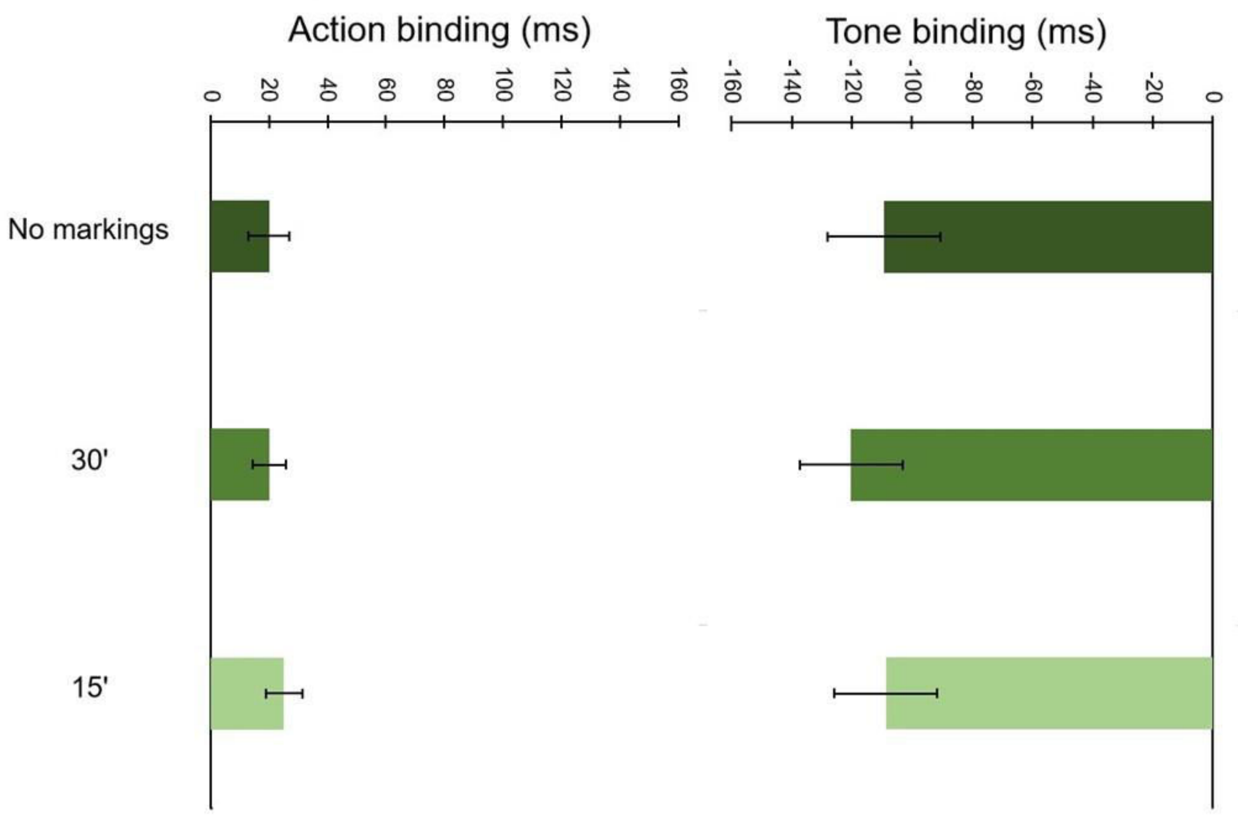
Participants' mean raw and baseline-corrected action and tone judgment errors across all three clock markings conditions

Clock markings condition	Judged event		Mean raw judgment error (ms) (SD)	Mean shift from baseline (ms) (SD)
No markings	Action	baseline	-15.30 (45.71)	19.95 (42.28)
		operant	4.65 (39.86)	
	Tone	baseline	-29.23 (55.12)	-109.32 (113.28)
		operant	-138.56 (104.81)	
30'	Action	baseline	-18.94 (45.59)	19.98 (34.20)
		operant	1.04 (41.29)	
	Tone	baseline	-25.27 (48.99)	-120.20 (103.31)
		operant	-145.47 (106.49)	
15'	Action	baseline	-22.54 (54.23)	25.05 (37.50)
		operant	2.51 (57.48)	
	Tone	baseline	-32.69 (56.55)	-108.77 (101.71)
		operant	-141.46 (115.36)	

As can be seen in **Figure 2.7**, there was a non-significant main effect of Clock markings, $F(2, 70) = 1.03$, $p = .361$, $\eta_p^2 = .029$, an expected significant main effect of Event, $F(1, 35) = 58.34$, $p < .001$, $\eta_p^2 = .625$, and the interaction between Clock markings and Event was non-significant, $F(2, 70) = 0.28$, $p = .756$, $\eta_p^2 = .008$. These results suggest that changes to clock markings do not affect binding as a whole or action and tone binding differently.

Figure 2.7

Participants' performance as a function of clock markings in Experiment 4



Note. Error bars depict SE.

DISCUSSION

In this experiment I investigated whether more extreme manipulations of clock markings would impact intentional binding. As in Experiment 3, I found that intentional binding was not significantly affected by this manipulation. Taken together, Experiments 3 and 4 suggest that this source of perceptual uncertainty does not exert a strong influence on intentional binding. In turn, this suggests that inter-study variability in clock markings is unlikely to contribute to variability in the magnitude of binding across studies.

EXPERIMENT 5

In the final experiment reported in this chapter, I examined the impact of another potential source of uncertainty, namely the clock hand length, on intentional binding. This stimulus parameter is not widely reported in studies. However, where it is reported it varies considerably across the literature (e.g. Engbert & Wohlschläger, 2006; Wenke et al., 2009; Takahata et al., 2012; Dogge et al., 2012; Ruess et al., 2018a; Desantis et al., 2011). This variability, coupled with its under-reporting, highlights the need to examine the possible effect on intentional binding. To this end, I measured intentional binding in three clock hand length conditions (8mm, 10mm and 13mm). Shorter clock hand lengths are associated with greater distance between the hand and the clock markings and, thus, should evoke greater uncertainty. In line with the uncertainty hypothesis (Eagleman & Holcombe, 2002), I predicted intentional binding would increase as a function of a decrease in the length of the clock hand.

METHODS

Participants

I recruited 40 participants (M_{age} = 22.42yrs, *SD* = 4.44, age range: 18 – 35, 10 males) using personal participant databases and the Goldsmiths Research Participation Scheme. I chose this sample size according to the same principles present in all the experiments reported above. They were compensated with £10 or 7.5 course credits for approximately 1hr30mins of experimental time. The final dataset contained 39 participants (M_{age} = 22.38yrs, *SD* = 4.49, age range: 18 – 35, 9 males) after a single participant exclusion. I decided upon the participant inclusion criteria as I did in all experiments above. Participants provided written informed consent prior to participation and the experiment was approved by the Department Ethics Committee at Goldsmiths, University of London.

Materials

I measured intentional binding using the same Libet clock task as reported above. The clock rotated at the standard speed of 2560ms per revolution, measured 21mm in diameter and was marked at conventional intervals (5, 10, 15, etc.). Across all relevant conditions, the tone was presented at 1,000 Hz and lasted for 100ms. This time, I used three types of clock hand lengths across three separate conditions (8mm, 10mm or 13mm; see **Figure 2.8**).

Figure 2.8

The three types of clock hand lengths (8mm – left, 10mm – centre, 13mm – right) used across all experimental conditions in Experiment 5

Procedure

I used the same procedure as in all experiments above (see **Figure 2.1**).

Data analysis

The entire data analysis protocol used in this experiment was identical to the one I used in the previous experiments reported above. This time, across all four binding blocks, I excluded 1.77% from the 8mm, 1.85% from the 10mm and 2.07% trials from the 13mm clock hand length conditions.

Due to abnormal kurtosis values and significant results rendered by the Kolmogorov-Smirnov test, I excluded a single subject from all subsequent analyses, who was also identified by boxplots as being an outlier.

Statistical analyses

After the above-mentioned participant exclusion was made, the Kolmogorov-Smirnov test rendered non-significant results for all factors. A visual inspection of the skewness and kurtosis values and histograms of all factors mirrored the results of the Kolmogorov-Smirnov test, so I proceeded forward using parametric statistics (Field, 2013). I ran a 3 x 2 repeated-measures ANOVA with Clock hand length (8mm, 10mm, 13mm) and Event (Action binding, Tone binding) as within-subject factors and mean baseline-corrected judgment error as a dependent variable.

Results

Participants' mean action and tone timing scores across all baseline and operant blocks, and their mean binding scores across all clock hand length conditions are shown in **Table 2.5**.

Table 2.5

Participants' mean raw and baseline-corrected action and tone judgment errors across all three clock hand length conditions

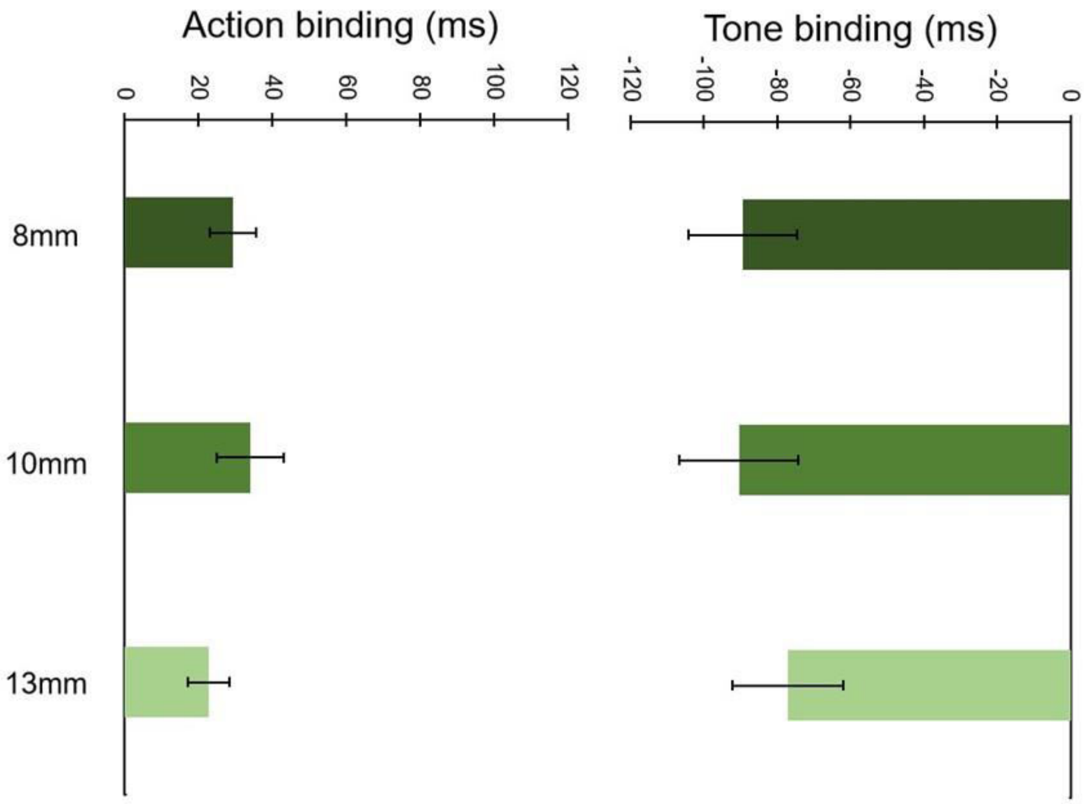
Clock hand length condition (mm)	Judged event		Mean raw judgment error (ms) (SD)	Mean shift from baseline (ms) (SD)
8	Action	baseline	-4.61 (51.55)	29.45 (39.71)
		operant	24.83 (67.11)	
	Tone	baseline	-17.67 (48.97)	-89.38 (92.57)
		operant	-107.05 (115.92)	
10	Action	baseline	-17.58 (56.82)	34.14 (56.30)
		operant	16.56 (63.35)	
	Tone	baseline	-28.04 (57.73)	-90.44 (100.84)
		operant	-118.48 (110.24)	
13	Action	baseline	-10.26 (43.66)	22.88 (34.98)
		operant	12.61 (48.14)	
	Tone	baseline	-40.07 (40.10)	-77.06 (95.10)
		operant	-117.14 (112.80)	

A visual inspection of **Table 2.5** and **Figure 2.9** reveals a robust intentional binding effect unaffected by manipulations of the clock hand length.

The main effect of Clock hand length was non-significant, $F(2, 76) = 0.10$, $p = .899$, $\eta_p^2 = .003$, whereas the main effect of Event was significant, $F(1, 38) = 64.07$, $p < .001$, $\eta_p^2 = .628$ (this showing that action and tone binding were significantly different from each other). The interaction between Clock hand length and Event was also non-significant, $F(2, 76) = 2.14$, $p = .124$, $\eta_p^2 = .053$. Cumulatively, these results suggest that the length of the Libet clock hand does not affect binding as a whole or action and tone binding differently.

Figure 2.9

Participants' performance as a function of clock hand length



Note. Error bars depict SE.

DISCUSSION

My manipulation of clock hand length did not seem to impact intentional binding. Not only do these results show that this source of uncertainty does not seem to influence intentional binding, they also suggest that the inconsistencies in the literature regarding this parameter setting are unlikely to be problematic.

GENERAL DISCUSSION

In five experiments I manipulated stimulus parameters of the Libet clock to examine their impact on intentional binding. I found that increasing the clock speed influences intentional binding by increasing tone binding. By contrast, manipulations of clock markings and the length of the clock hand do not seem to affect either the action or tone component of intentional binding. These results have theoretical implications regarding (tone) binding's sensitivity to uncertainty and practical implications regarding the use of the Libet clock task in the measurement of intentional binding.

Theoretical implications

It has been noted that despite the widespread use of intentional binding as a measure of sense of agency, its underlying mechanisms are still unclear (Moore & Obhi, 2012). The clock speed findings reported above potentially provide some insights in this regard. More specifically, I have interpreted the effect of clock speed on intentional binding in terms of the hypothesis that intentional binding is indicative of a temporal contiguity prior for self-caused events (Eagleman & Holcombe, 2002). According to this view, increases in uncertainty would be expected to increase (up to a point) the magnitude of intentional binding. In line with this, I found that high uncertainty (engendered by a clock speed increase) resulted in stronger intentional binding. Interestingly, I found that only tone binding was affected by this speed manipulation. That might be because in the case of tone outcomes we only have access to re-afferent sensory information to determine the onset of an event, whereas for actions we also have access to internal motor signals. Accordingly, the reduced "evidence-base" for the awareness of tone outcomes may render it more vulnerable to modulations from clock speed. This interpretation is also supported by the magnitude of the standard deviations in action and tone timing judgments that can be observed in my data (see **Tables 2.1** and **2.2**). In particular, my data show that tone timing judgments tend to be more variable than action timing judgments, especially in operant conditions. This may be indicative of greater uncertainty associated with tones.

My finding concerning the effect of clock speed is in line with other studies looking at the role of uncertainty on intentional binding. For example, Wolpe et al. (2013) assessed the effect of changes in tone intensity on intentional binding. Low tone intensity was associated with increased action and tone binding, mirroring my own findings. However, I *only* found an effect on tone binding. Moreover, this effect was driven by changes in operant tone judgment errors, whereas in Wolpe and colleagues' study the change in tone binding was driven by changes in baseline tone judgment errors. In light of this result, Wolpe et al. (2013) argue that tone binding was not genuinely affected by uncertainty. By contrast, my findings suggest that it can be. Despite these differences, taken together these findings strongly suggest that the intentional binding effect is driven, at least in part, by sensory and/or motor uncertainty.

The apparent role of uncertainty in intentional binding is also consistent with broader theoretical accounts of agency processing. For example, Moore and Fletcher (2012) have proposed that sense of agency is determined by the integration of different agency cues. These include, but are not limited to, motor and sensory signals, as well as external situational variables. These cues are combined optimally, such that cues are weighted according to their reliability (for a more ample explanation, see **Chapter 1**). Interestingly, whereas I have favoured Eagleman and Holcombe's (2002) temporal contiguity prior hypothesis, Wolpe et al. (2013) used Moore and Fletcher's (2012) cue integration framework to explain their findings regarding the effect of uncertainty on intentional binding. My view is that cue integration and the temporal contiguity prior hypothesis are not mutually exclusive. Indeed, the latter may provide a temporal template that sets the context for the specific directional shifts observed for self-caused events, whilst the former helps explain the processing of these events and their interaction.

Alternative explanations of the effect of clock speed on binding

It is worth noting that in **Chapter 1** I am mentioning three phenomena that might, at first sight, constitute valid alternative neurocognitive mechanisms of my findings (in lieu of the uncertainty account I have proposed above). The representational momentum effect (Hubbard & Bharucha, 1988), the flash-lag effect (Nijwahan, 1994) and the prior-entry phenomenon (Spence & Parise, 2010) all distort timing judgments by distancing them from their actual time of occurrence due to various reasons (i.e. overestimation of the clockwise motion of the clock hand, extrapolation into the future of the clock hand, intramodal division of attention, respectively). Thus, one argument might be that participants' motor and tone judgments that form the basis of the intentional binding effect deviated from their actual time of occurrence not because of participants' overreliance on their temporal contiguity priors, but because of these three well-documented artefacts of the Libet clock.

What is more, one prediction based on Eagleman and Holcombe's (2002) uncertainty account is that binding should be greater in the high-speed condition, where uncertainty is greatest. One might further expect the variance to be greatest in this condition, reflecting the greatest uncertainty. However, this is not the case – the reporting resolution increases linearly with a decrease in speed, this facilitating greater variance in the slowest speed condition. This, at first glance, seems to undermine the idea that participants' increased binding in the high-speed condition reflects their overreliance on their temporal contiguity priors because of extreme uncertainty.

However, it is my view that all the above-mentioned alternative mechanisms can be discounted. As regards the latter point I am making in the preceding paragraph, that is because the variance in the slow-speed condition is *artificially inflated* by the increased reporting resolution, *not* by extreme uncertainty associated with the slowing down of the clock hand. As regards the other biases I am expanding upon, that is because all of them should affect timing judgments (so called *raw* judgment errors) equally across all binding blocks (baseline and operant) and, thus, should cancel each other out upon the calculation of the binding effect (so called *baseline-corrected* judgment errors). This implies that any change in baseline-corrected judgment errors that reaches statistical significance (like the effect of clock speed on binding I report in this chapter) is due to some other mechanism. In my case, based on the evidence provided, this mechanism is likely to be rooted in the link between uncertainty and participants' temporal contiguity priors.

Practical implications

The stimulus parameters for the Libet clock vary considerably across studies. The principal finding of Experiments 1 and 2 is that intentional binding seems to be influenced by clock speed. This result requires further experimentation to consider the experimental features and cognitive variables that mediate it. However, it also indicates that differential clock speeds across studies may contribute to inter-study heterogeneity in the magnitude of intentional binding. In turn, this result emphasises the need for consistency in this stimulus parameter, so that future investigations can accurately isolate the effect of their manipulations from that of other confounds on binding. This is important across experiments, especially if one is comparing effect sizes. It is also important within experiments if one is comparing intentional binding across groups.

The other experiments featured in this series examining clock markings and clock hand length suggest that changes to these parameters do *not* significantly influence the expression of intentional binding. One potential reason for these results might be the prior beliefs we have concerning the aspect of standard analogue clocks, which might have acted as a *surrogate prior* that helped participants make time judgments despite the uncertainty introduced by my manipulations. Interestingly, these are parameters that often vary across experiments, but my results suggest that they are unlikely to be problematic. This is reassuring for those seeking to compare or aggregate findings from different experiments in which these parameters vary. It is also useful in terms of guiding the design of future studies – for example, when testing certain populations, it might be preferable to tweak some of these settings so as to reduce the perceptual burden of the task. My findings show that this is unlikely to significantly alter intentional binding.

Conclusions

In conclusion, the experiments presented in this chapter found that changes in the Libet clock rotation speed seem to increase intentional binding whereas manipulations of clock markings or the length of the clock hand do not seem to have a significant effect. My results highlight the importance of maintaining consistency with respect to the speed of the Libet clock across studies and shed further light on the relationship between uncertainty and binding.

In light of these compelling results, the next chapter focusses on the link between another Libet clock feature that varies widely across the literature (the clock radius) and intentional binding.

This chapter is based on work that has been submitted for publication:

Ivanof, B. E., Terhune, D. B., Coyle, D., Gottero, M. & Moore, J. W. Examining the effect of Libet clock stimulus parameters on temporal binding.

Author contributions:

I, Bianca E. Ivanof, reviewed all the relevant literature and designed all five experiments reported above, collected the data for four of them, analysed the data from all of them, wrote and edited the manuscript. Devin B. Terhune provided valuable advice on the statistical methods used in all five experiments and edited the manuscript. David Coyle provided the software package used in all five experiments. Marta Gottero collected the data for Experiment 2. James W. Moore was involved in all aspects reported above and edited the manuscript.

CHAPTER 3

The influence of the Libet clock radius on intentional binding

ABSTRACT

The Libet clock radius is changed across the intentional binding literature similarly to how other clock parameters are. This study assessed whether this variability in clock radius could be problematic for the comparability of results across the binding literature. As part of the traditional intentional binding experimental setup, I varied the clock radius across three separate conditions to see if this would influence participants' action or tone binding. I found that the choice of radius significantly interacts with intentional binding, potentially due to an effect of radius on tone binding. I discuss this result and its implications and speculate upon potential mechanisms that might drive it.

INTRODUCTION

Thus far, this thesis has explored the effects the Libet clock speed, number of clock markings and length of the clock hand have on intentional binding. Among these components, I found the clock speed, but not the number of clock markings or length of the clock hand, to impact on binding.

These experiments were focussed primarily on the potential influence of uncertainty on intentional binding that these changes induce. Here I continue with this systematic investigation, looking at how manipulations of another clock parameter, namely the clock radius, might influence binding. Unlike the previous experiments, clock radius is unlikely to influence uncertainty per se. However, it is something that varies markedly between studies and, therefore, requires further investigation.

As we have seen is the case with the afore-mentioned clock settings, the radius also varies across the intentional binding literature. For example, some investigators prefer to use a large clock radius (e.g. 45mm, visual angle: approximately 8.5° – Isham, Banks, Ekstrom & Stern, 2011; 60mm, visual angle: approximately 12.4° – Buehner, 2015; 100mm, visual angle: approximately 16.2° – Capozzi et al., 2016) and others clock faces that have a medium (e.g. approximately 30mm – Ruess, Thomaschke & Kiesel, 2018a; 2018b) or a small (e.g. approximately 10mm, visual angle: 1.8° – Strother, House & Obhi, 2010; Aarts & van den Bos, 2011; Dogge, et al., 2012; Vinding, Jensen & Overgaard, 2015) radius. This rather large spread of radius sizes inspired me to assess whether the choice of clock radius might affect action or tone binding. If it does then it would have implications for the comparability of results across studies.

A closer look over the literature on visual perception suggests that the radius of a visual stimulus might influence subjective timing of events. It has been noted that the human vision system is split into different sub-systems, two of which are the *smooth pursuit eye movements* and the *saccadic* systems (Robinson, 1965). The former system typically gets activated only in the presence of a specific, moving target (Rashbass, 1961; Fender, 1962) and works in concert with the saccadic system in order to maximise smoothness and accuracy when the eyes are tracking an object (Lisberger, Morris & Tychsen, 1987). When smooth pursuit eye movements are not able to keep up with the target anymore, the tracking is increasingly punctuated with corrective saccades, which serve to bring the target back to or near the centre of the fovea (the area of highest acuity) (Coren, Bradley, Hoenig & Girgus, 1975).

I suspect that this interplay between smooth pursuit eye movements and corrective saccades might be relevant in the context of a phenomenon called *saccadic chronostasis*, which refers to the subjective visual lengthening of a post-saccadic visual stimulus (Yarrow, Haggard, Heal, Brown & Rothwell, 2001). This phenomenon commonly occurs when we glance at a clock with a moving second hand and, for a moment, we think it has stopped working (hence the term *“clock stopping” experience* that authors sometimes use in order to refer to saccadic chronostasis) (Yarrow, Haggard & Rothwell, 2010). It is worth noting, though, that we do not have such “clock stopping” experiences every time we glance at a clock – rather, they occur only when the clock hand changes just before or during the saccade (Brown & Rothwell, 1997). I believe that this latter point adds to the above-mentioned putative link between smooth pursuit eye movements, corrective saccades and chronostasis. If we assume that smooth pursuit eye movements are instrumental to the tracking of the rotating object on the clock face (which can be considered an example of a specific, moving target; see above Rashbass, 1961; Fender, 1962), my proposition is that an increase in radius might be associated with an increase in smooth pursuit eye movements and, consequently, with an increase in the number of corrective saccades. This, in turn, might provide greater scope for chronostasis to occur (because there would be more corrective saccades before or during which the clock hand would change position). If this is the case, an increase in radius should be associated with more “clock stopping” experiences, which could systematically alter participants’ action and tone time judgments.

In order to explore the relationship between clock radius and binding, I again administered the typical intentional binding Libet clock task, whereby participants are asked to time their actions or action outcomes by using the location of a clock hand on a clock face whose radius changed across separate conditions.

METHODS

Participants

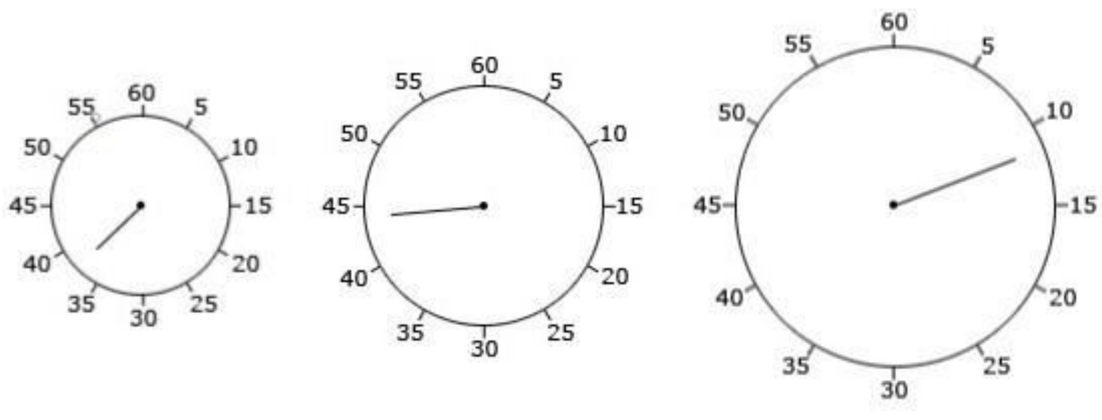
I recruited 40 participants (M_{age} = 29.2yrs, *SD* = 13.83, age range: 18 – 76, 7 males) to this study using the Goldsmiths Research Participation Scheme and personal participant databases. They were compensated with 7.5 course credits or £10 for approximately 1hr30mins of experimental time. From this sample, I excluded one individual (see “Data analysis”). The remaining 39 participants (M_{age} = 28.7yrs, *SD* = 14.48, age range: 18 – 76, 7 males) were all right-handed, had normal or corrected-to-normal vision and no self-reported history of psychiatric or neurological disorders or substance usage that might interfere with their cognitive performance. I chose this sample size using the same technique I reported in the previous chapter – that is, in light of the sample sizes present in other recent works on binding (e.g. Malik & Obhi, 2019 – *N* = 36; Christensen et al., 2019 – *N* = 20; Antusch et al., 2020 – *N* = 36), I judged a sample size of 39 participants as appropriate. All participants gave written informed consent prior to their participation and the experiment was approved by the Department Ethics Committee at Goldsmiths, University of London.

Materials

Intentional binding was measured using the same Libet clock paradigm used in **Chapter 2**. The clock hand rotated at a speed of 2560ms per clock revolution and the clock was marked at conventional intervals (5, 10, 15, etc.). The tone in the baseline and both operant blocks was presented at 1,000 Hz and lasted for 100ms. The clock radius changed across three separate conditions and so did the clock hand length, in order to preserve the clock hand-to-clock face ratio (small radius – 10.5mm, clock hand length – 9mm; medium radius – 17.5mm, clock hand length – 13mm; large radius – 23.5mm, clock hand length – 19mm; see **Figure 3.1**).

Figure 3.1

The three types of clock radius (small – 10.5mm, left; medium – 17.5mm, centre; large – 23.5mm, right) used across all experimental conditions in this study



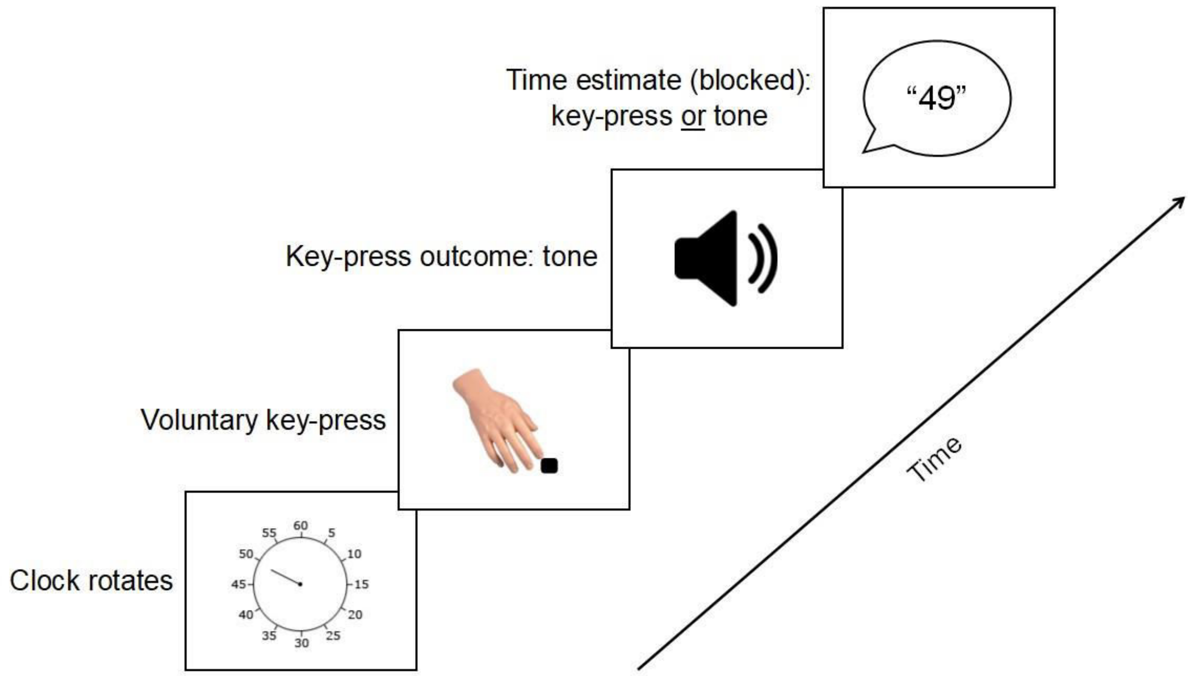
Procedure

As part of each clock radius condition, participants completed four different binding blocks (operant action, operant tone, baseline action, baseline tone), each containing 30 trials, resulting in 12 blocks (360 trials) per participant. The clock radius conditions were counterbalanced across participants and the binding blocks were fully randomised within participants.

Participants also completed three practice trials prior to each binding block (during these practice trials, they were presented with a clock face that had a small radius). That resulted in 12 additional trials per participant, which were discarded. Afterwards they started the main task.

Figure 3.2

Standard trial structure in operant blocks following Haggard et al. (2002)



Note. Participants were required to press a key at their own pace, which was followed by a tone 250ms later. Depending on the operant block type, they had to report the time of their action or of the action outcome.

As can be seen in **Figure 3.2**, in the operant blocks, participants were required to press a pre-specified key whenever they felt the urge to do so. Their key-press was always followed by a tone after a fixed interval of 250ms. They had to estimate the position of the clock hand on the clock face when they pressed the key (operant action block) or heard the tone (operant tone block). Once the clock hand stopped rotating after a random delay following the tone (1000 – 2500ms), I required participants to verbally report the estimated clock time to me, and I inputted the number. Similarly to how I proceeded in the experiments present in **Chapter 2**, I favoured this approach, rather than allowing participants to input their time estimates themselves, so as to encourage participants' sustained attention on the task.

Participants also had to complete two baseline blocks. In the baseline action block, participants pressed the key whenever they felt the urge to do so and subsequently verbally reported the clock time corresponding to their key-press to me (the clock here stopped rotating after a random time between 1000 – 2500ms following the action). I specified to all participants that actions performed in the baseline block would never be followed by an outcome. In the baseline tone block, participants were instructed to pay attention to the location of the clock hand on the clock face when they heard a tone generated by the computer. This tone occurred at a random time between 2500 – 5000ms after trial onset and I again asked participants to report the perceived time of the tone to me.

Participants sat at a distance of approximately 65cm from the clock face across all radius conditions (visual angle across the small, medium and large radius conditions: approximately 1.8°, 3.1°, 4.1°, respectively). I always informed participants of any change in clock radius before completing a new condition, gave them opportunities to rest in-between conditions and reminded them not to pre-plan their movements and be as accurate as possible when reporting their time estimates to me.

Data analysis

I calculated raw judgment errors as the perceived time minus the actual time of action or tone onset. This resulted in four raw judgment errors for the four binding blocks. I computed *Action binding* as the mean operant – mean baseline action judgment error and *Tone binding* as the mean operant – mean baseline tone judgment error. I computed these measures for each clock radius condition.

I excluded individual trials containing raw judgment error outliers within participants (M +/-2.5 SDs). This criterion resulted in 1.73%, 1.55% and 1.79% of trials excluded from the small, medium and large clock radius condition, respectively. I removed participant outliers at the group level if a combination of factors indicated univariate or multivariate outliers (Field, 2013; these included skewness and kurtosis values greater than approximately +/-2.000, significant results rendered by the Kolmogorov-Smirnov test, visual inspection of boxplots and histograms). This criterion resulted in the exclusion of one participant.

Statistical analyses

After the above-mentioned participant was excluded, the Kolmogorov-Smirnov test rendered significant results for some factors. However, visual inspection of skewness and kurtosis values and histograms of these factors posed no problems, so I proceeded forwards using parametric statistics (which are relative robust against these minor perturbations in normality; Field, 2013). Thus, I conducted a factorial repeated-measures ANOVA with Clock radius (Small, Medium, Large) and Event (Action binding, Tone binding) as within-subject factors and mean baseline-corrected judgment error as a dependent variable, and further one-way repeated-measures ANOVAs. In the case of two analyses, the assumption of sphericity was violated, so I report the Greenhouse-Geisser correction.

Results

Participants' performance across all three clock radius conditions can be seen in **Table 3.1** and **Figure 3.3**.

Table 3.1

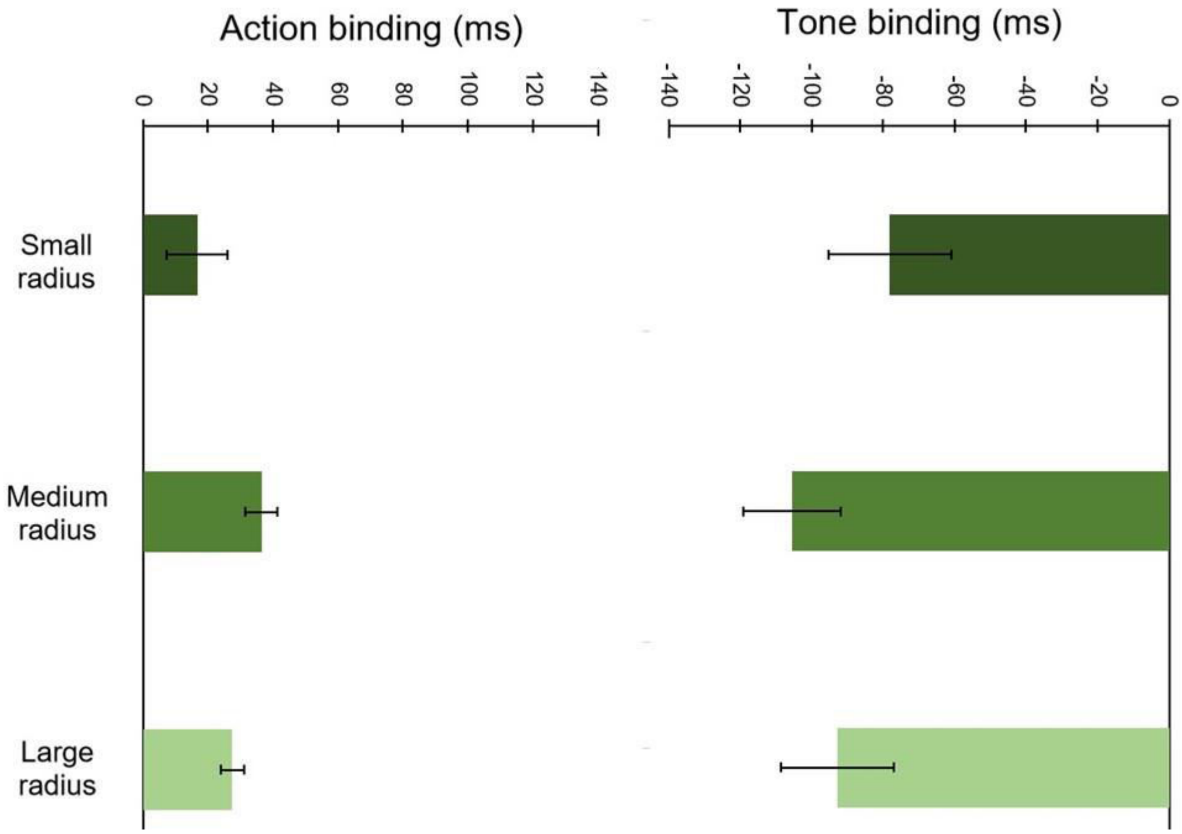
Participants' mean raw and baseline-corrected action and tone judgment errors across all three clock radius conditions

Clock radius condition	Judged event		Mean raw judgment error (ms) (SD)	Mean shift from baseline (ms) (SD)
Small radius	Action	baseline	-34.35 (74.24)	16.63 (59.21)
		operant	-17.72 (83.80)	
	Tone	baseline	-30.10 (68.91)	-78.14 (107.58)
		operant	-108.24 (123.67)	
Medium radius	Action	baseline	-50.16 (71.34)	36.43 (31.66)
		operant	-13.73 (69.87)	
	Tone	baseline	-40.40 (61.91)	-105.52 (84.90)
		operant	-145.93 (116.49)	
Large radius	Action	baseline	-55.60 (67.95)	27.50 (22.26)
		operant	-28.09 (73.86)	
	Tone	baseline	-42.69 (63.16)	-92.72 (98.73)
		operant	-135.41 (113.29)	

My analysis rendered no main effect of Radius, $F(2, 76) = 0.15$, $p = .858$, $\eta_p^2 = .004$, and a significant main effect of Event, $F(1, 38) = 58.64$, $p < .001$, $\eta_p^2 = .607$, this reflecting that action and tone binding were significantly different from each other. Importantly, I found a significant interaction between Clock radius and Event, $F(1.60, 60.97) = 4.08$, $p = .029$, $\eta_p^2 = .907$. This suggests the choice of clock radius impacts on action and tone binding differently.

Figure 3.3

Participants' performance as a function of clock radius



Note. The error bars represent SE.

In order to follow this interaction up, I conducted two further one-way repeated-measures ANOVAs looking into the influence of Clock radius on Action and Tone binding separately. There seems to be no effect of Radius on Action binding, $F(1.62, 61.61) = 2.43$, $p = .106$, $\eta_p^2 = .060$, and a trend-level, medium-sized effect of Radius on Tone binding, $F(2, 76) = 2.51$, $p = .088$, $\eta_p^2 = .062$. This combination of results suggests that clock radius interacts with action and tone binding differently and that this relationship might be driven by the effect of radius on tone binding, yet this is inconclusive.

DISCUSSION

This study looked into the effect of clock radius manipulations on intentional binding. Participants performed the standard intentional binding task and timed their actions and action outcomes by using a clock whose radius changed from small to large across three separate conditions. My data suggest that action and tone binding are affected by changes in clock radius differently and that this might be driven by the effect of radius on tone binding. In the remainder of this section, I discuss the implications of these findings and speculate upon potential mechanisms that might drive them.

My results recommend that investigators exercise caution when choosing the radius size in intentional binding experiments. This is important given the variability in this parameter setting across experiments using the Libet task (e.g. large radius – Isham et al., 2011; Buehner, 2015; Capozzi et al., 2016; medium radius – Ruess et al., 2018a; 2018b; small radius – Strother et al., 2010; Aarts & van den Bos, 2011; Dogge et al., 2012; Vinding et al., 2015). The variability in this parameter setting is particularly important if one wants to replicate and/or compare binding effects across experiments. In this regard, my results suggest that particular attention should be paid to the reporting of clock radius settings as well as to the participants' distance from the clock stimulus, so as to ensure that this aspect of the clock is controlled and repeatable. My results also suggest that binding is most pronounced when the radius of the clock face measures 17.5mm and the visual angle is 3.1° (see **Figure 3.3**). Although increasing the magnitude of binding is not necessarily a goal of my experiment, it does suggest that binding is most likely here, and therefore I would recommend that researchers use this setting in future research.

The question as to what neurocognitive mechanisms might drive the effect of radius on binding remains open. I have suggested that the most salient factor is the perceptual disturbance (chronostasis) that might result from an increase in corrective saccades during smooth pursuit eye movements. This might become more of an issue as the radius of the clock gets larger. In terms of how this might influence binding, one possibility is that it engenders more anticipatory awareness of actions and outcomes. This interpretation finds a home in the *shifted perceptual onset account* that some authors have evoked to explain chronostasis (Yarrow, Haggard & Rothwell, 2004; Yarrow et al., 2010). This account postulates that chronostasis might reflect the brain's tendency of filling in the "gap" in perception left by saccades by ante-dating the post-sacaddic image to just before the saccadic onset, thereby providing us with a continuous awareness of the state of the objects in the world. It is therefore possible that the more such ante-dating experiences accumulate, the more time judgments would lag behind reality.

Nonetheless, one issue with this possibility is that the effect might be expected to be equally present in all blocks, and as such should not influence the magnitude of binding. Consequently, it might be that some other mechanism is perhaps at work in operant blocks that is influencing timing judgments. This may be specific to agency or is perhaps the result of some other perceptuo-cognitive process that occurs in the context of action-outcome pairings.

Limitations and future directions

Perhaps the most salient limitation of this study is the lack of a measure of smooth pursuit eye movements and corrective saccades (such as eye-tracking). Moreover, because of research design constraints (i.e. long testing sessions), I was unable to directly manipulate the presence/absence of smooth pursuit eye movements by, for example, introducing three additional clock radius control conditions whereby participants would make time judgments by fixating the centre of the clock face (as compared with using smooth pursuit eye movements). Future research should thus aim to better isolate and investigate the influence of the human vision system on participants' performance on the Libet task.

CHAPTER 4

The influence of action initiation instructions on intentional binding

ABSTRACT

Action initiation instructions vary across intentional binding studies. This study assessed whether this inter-study variability in action initiation instructions might impact on binding and, consequently, on the comparability of results across the binding literature. In the context of the typical intentional binding experimental setup, across three different conditions, participants were instructed to make an action either a) before a full revolution of the clock face had been completed, b) at any point in time after a full revolution of the clock face had been completed, or c) whenever they felt like it. My results suggest tone binding to increase in the condition where participants pressed the pre-specified key whenever they felt like it. This suggests that instructions and the constraints they may impose may have a significant effect on intentional binding. This finding is notable in light of the above-mentioned variability of instructions given to participants across binding studies.

INTRODUCTION

So far, I have investigated how manipulations of the Libet clock stimulus affect intentional binding. These experiments have looked at the clock speed, number of clock markings, length of the clock hand and size of the clock radius. Here I aim to broaden my scrutiny of the binding methodology, moving beyond the stimulus itself to focus on how participant instructions influence intentional binding.

In Libet-style experiments, the instructions given to participants are thought to be of considerable importance (Jensen, Di Costa & Haggard, 2015). One reason for this is that the task is not necessarily intuitive – rarely are we required to introspect on the timing of our intentional or sensorimotor experiences. As such, participants need clear instructions on how to perform the task. The other reason is that the instructions themselves set up the volitional boundaries of the task. Participants are encouraged to act freely, but within certain constraints (a juxtaposition that has provoked controversy over the volitional status of this laboratory task – Haggard, 2008).

This latter aspect is of central interest to this chapter because, as happens with the Libet clock stimulus settings investigated thus far, authors also vary in their preference for the kinds of constraints they apply (if at all). For example, some authors specify participants that they should not make an action during the first revolution of the rotating object (e.g. Libet et al., 1983; Haggard et al., 2002; Engbert & Wohlschläger, 2006). Others instruct participants to not make an action during the first or last revolution of the rotating object (Strother et al., 2010), or earlier than the first half of its first revolution or later than its third revolution (Majchrowicz & Wierzchoń, 2018). Sometimes, participants are not given any restrictions on when they can perform the action (e.g. Lynn, Muhle-Karbe, Aarts & Brass, 2014; Cubillas, Landáburu & Matute, 2019). This rather large spread of instructions has prompted me to investigate whether manipulating the instructed constraints over action affects implicit sense of agency as indexed by intentional binding. A closer look over the volition literature indicates this might be the case.

In his original work, Benjamin Libet (1985) states that an action can be regarded as voluntary and a function of the individual's will as long as a) it arises endogenously (rather than in response to external stimulation), b) the individual feels introspectively they are performing an action on their own initiative, which they can also choose whether or not to perform and c) there are no restrictions or compulsions that control the individual's initiation and performance of said action. This taxonomy has inspired subsequent volition researchers to create different frameworks that de-construct the qualities of a voluntary act, an example of which is the so called "What, When, Whether" model of intentional action (Brass & Haggard, 2008). This model rests on three kinds of information-generation. According to it, an action is intentional as long as the agent can decide WHAT to do, WHEN to do it and WHETHER to do it. Thus, the WHAT component of intentional action reflects the decision of what action to make. The WHEN component focusses on the temporal dynamics of self-initiated actions. The WHETHER component refers to the choice a free agent has as to whether to make an action or not.

In line with this "What, When, Whether" model of intentional action, certain authors have set out to test its assumptions to see how individuals' sense of agency varies according to different permutations of each of these components. Barlas and Obhi (2013), for example, looked into how manipulations of the WHAT component impacts on participants' implicit sense of agency. As part of the typical intentional binding experimental setup, they varied the number of action alternatives from none (the "no choice" condition), to three (the "medium" condition) and seven (the "high" condition). They found participants' binding to increase as a function of the number of action alternatives.

More interesting for the purpose of this chapter, the WHEN component too has been manipulated experimentally. Manipulations of the WHEN component alone have been suggested to not affect implicit agency as measured by intentional binding. This evidence comes from Wenke et al. (2009), who looked at how different permutations of the WHAT and WHEN components *combined* affect intentional binding. They manipulated the WHAT component by asking participants to make either left or right key presses, either as instructed or as they freely chose. They manipulated the WHEN component by asking participants to press the key within certain time intervals, either when instructed to do so or when they freely chose. Interestingly, the authors found no difference in binding as a function of whether participants' timing of their own actions was externally imposed vs freely chosen. However, they did find a significant increase in binding in the condition in which participants could *both* freely choose which key to press and in which time interval to do so (relative to the condition in which they could not choose which key to press and when to press it).

Some neuroscience research may also, indirectly, offer some insight into the likely effect of the WHEN component on human agency. For example, it has been shown that the freedom to choose when to act is associated with more activation in the dorsolateral prefrontal cortex (DLPFC) (an area consistently implicated in action-selection; for a review, see Khalighinejad et al., 2016) or in the pre-SMA (an area consistently implicated in intentionality and self-generated actions; see Lau et al., 2004; Passingham, Bengtsson & Lau, 2010). For example, Jahanshahi et al. (1995) used PET scanning to see what brain areas get activated in Parkinson's patients vs neurotypical individuals during a rest condition vs a condition in which both groups could make self-initiated movements every three seconds or another condition in which these movements were externally triggered (and, thus, participants could not choose *when* to trigger them). Relevant to the present context, in the neurotypical group the investigators found increased activity in the right DLPFC in the self-initiated compared to the externally generated action condition. In a similar experiment, Deiber, Honda, Ibañez, Sadato and Hallett (1999) varied the type of movement initiation (self-initiated vs visually triggered), as well as the pace of these movements (repetitive vs sequential) and the speed with which participants could perform these movements (slow vs fast). They found significantly more activity in the pre-SMA for self-initiated than for visually triggered movements.

From the summary above, it becomes apparent that manipulations of the components of Brass and Haggard's (2008) model of intentional action affect human agency. However, as you can see, Wenke and colleagues' (2009) study offers inconclusive evidence with respect to the role of the WHEN component in sense of agency. I believe that this issue warrants a re-assessment, particularly in the context of the Libet clock method. This is because Libet's original motivation for instructing participants to refrain from pressing the button during the first revolution of the clock was so as to reduce the possibility of impulsive reflex actions, thus augmenting the volitional status of the action (Libet et al., 1983). However, the findings of the aforementioned neuroscience studies, combined with the predictions of the “What, When, Whether” model, suggest that any instructed constraint on the timing of action should reduce sense of agency and, by extension, intentional binding.

To shed further light on this issue, I simplified the design of Wenke and colleagues (2009) so as to assess the *individual* contribution of the WHEN component on intentional binding. I did this by asking subjects to perform the standard intentional binding Libet clock task and, across different conditions, make actions a) before one revolution of the clock face had been completed, b) at any point in time after one revolution of the clock face had been completed, and c) whenever they wanted to. Based on Libet’s criteria for agentic actions (1985), the predictions of Brass and Haggard’s (2008) “What, When, Whether” model of intentional action, and the results of the aforementioned neuroscience studies, I predicted an increase in binding in the “Whenever” condition relative to the other two conditions.

METHODS

Participants

I recruited 40 participants (M_{age} = 26.8yrs, *SD* = 11.90, age range: 18 – 77, 13 males) to this study using the Goldsmiths Research Participation Scheme and personal participant databases. They were compensated with 7.5 course credits or £10 for approximately 1hr30mins of experimental time. From this dataset, two participants were excluded (see “Data analysis” below). The remaining 38 participants (M_{age} = 25.8yrs, *SD* = 13.25, age range: 18 – 77, 12 males) were all right-handed, had normal or corrected-to-normal vision and no self-reported history of psychiatric or neurological disorders or substance usage that might interfere with their cognitive performance. I chose this sample size using the same technique I reported in the previous chapters – that is, in light of the sample sizes present in other recent works on binding (e.g. Malik & Obhi, 2019 – *N* = 36; Christensen et al., 2019 – *N* = 20; Antusch et al., 2020 – *N* = 36), I judged a sample size of 38 participants as appropriate. All participants gave written informed consent prior to their participation and the experiment was approved by the Department Ethics Committee at Goldsmiths, University of London.

Materials

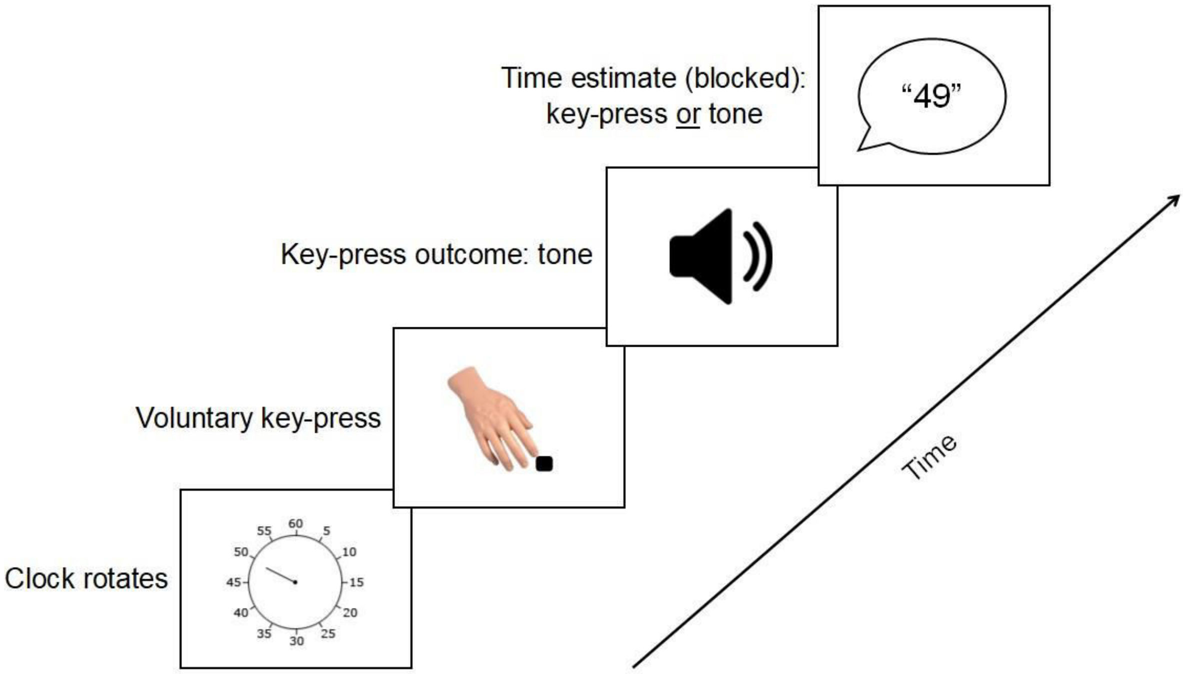
Intentional binding was measured using the standard Libet clock paradigm. The clock hand rotated at a speed of 2560ms per clock revolution and the clock was marked at conventional intervals (5, 10, 15, etc.). The clock face had a diameter of 21mm and the clock hand measured 9mm. The tone in the baseline and both operant conditions was presented at 1,000 Hz and lasted for 100ms.

Procedure

As can be seen in **Figure 4.1**, in the operant blocks, participants were required to press a pre-specified key. Their key-press was always followed by a tone after a fixed interval of 250ms. They had to estimate the position of the clock hand on the clock face when they pressed the key (operant action block) or heard the tone (operant tone block). Once the clock hand stopped rotating after a random delay following the tone (1000 – 2500ms), I required participants to verbally report the estimated clock time to me, and I inputted the number. Similarly to how I proceeded in all previous experiments, I favoured this approach, rather than allowing participants to input their time estimates themselves, so as to encourage participants' sustained attention on the task.

Figure 4.1

Standard trial structure in operant blocks following Haggard et al. (2002)



Note. Participants were required to press a key, which was followed by a tone 250ms later. Depending on the operant block type, they had to report the time of their action or of the action outcome.

Participants also had to complete two baseline blocks. In the baseline action block, participants pressed the key and subsequently verbally reported the clock time corresponding to their key-press to me (the clock hand here stopped rotating after a random time between 1000 – 2500ms following the action). I specified to all participants that actions performed in the baseline block would never be followed by an outcome. In the baseline tone block, participants were instructed to pay attention to the location of the clock hand on the clock face when they heard a tone generated by the computer. This tone occurred at a random time between 2500 – 5000ms after trial onset and I again asked participants to report the perceived time of the tone to me.

Crucially, in blocks requiring an action (operant action, operant tone and baseline action) participants were given one of three different instructions:

1) Make an action before one revolution of the clock has been completed ("Before" condition)
2) Make an action at any point in time after one revolution of the clock has been completed ("After" condition)
3) Make an action whenever they want to ("Whenever" condition).

For each of the blocks requiring an action (operant action, operant tone, baseline action), participants completed one block for each instruction type. In addition, they completed a single baseline tone block (as there are no actions, there was no instruction manipulation). This resulted in each participant completing 10 blocks of trials. Each block consisted of 30 trials, and the first three trials were treated as practice trials and were discarded prior to analysis. Thus, the raw data I conducted my analyses on contained 270 trials per participant (27 trials x 10 blocks).

Participants sat at a distance of approximately 65cm from the clock face across all instructions conditions (visual angle: approximately 1.8°). I always informed participants of any change in instructions before completing a new condition, gave them opportunities to rest in-between conditions and reminded them not to pre-plan their movements and be as accurate as possible when reporting their time estimates to me.

Data analysis

I calculated raw judgment errors as the perceived time minus the actual time of action or tone onset. This resulted in four raw judgment errors for the four binding blocks. I computed *Action binding* as the mean operant – mean baseline action judgment error and *Tone binding* as the mean operant – mean baseline tone judgment error. I computed these measures for each of the three action initiation instruction conditions.

I excluded individual trials containing raw judgment error outliers within participants (M +/-2.5 SDs). This criterion resulted in the exclusion of 1.90%, 1.92% and 1.98% of trials from the “Before”, “After” and “Whenever” condition, respectively. I screened the data further in order to make a decision as to what type of statistical tests to use (parametric or non-parametric) by checking for skewness and kurtosis values greater than approximately +/-2.000 and significant results rendered by the Kolmogorov-Smirnov test, and by visually inspecting the boxplots and histograms of all factors (Field, 2013). I excluded two participants from the entire dataset due to experimenter errors (error in the coding of the data).

Statistical analysis

The results of the Kolmogorov-Smirnov test indicated that no factors were abnormally distributed. Closer inspection of the skewness and kurtosis values, histograms and boxplots of all factors mirrored the results of the Kolmogorov-Smirnov test, so I proceeded forward using parametric statistics.

I conducted a factorial repeated-measures ANOVA with Action initiation instructions (Before, After, Whenever) and Event (Action binding, Tone binding) as within-subject factors and mean baseline-corrected judgment error as a dependent variable, and further one-way repeated-measures ANOVAs. Where necessary, I conducted Bonferroni-corrected pairwise comparisons.

Results

Participants’ performance across all three action initiation instructions conditions can be seen in **Table 4.1** and **Figure 4.2**.

Table 4.1

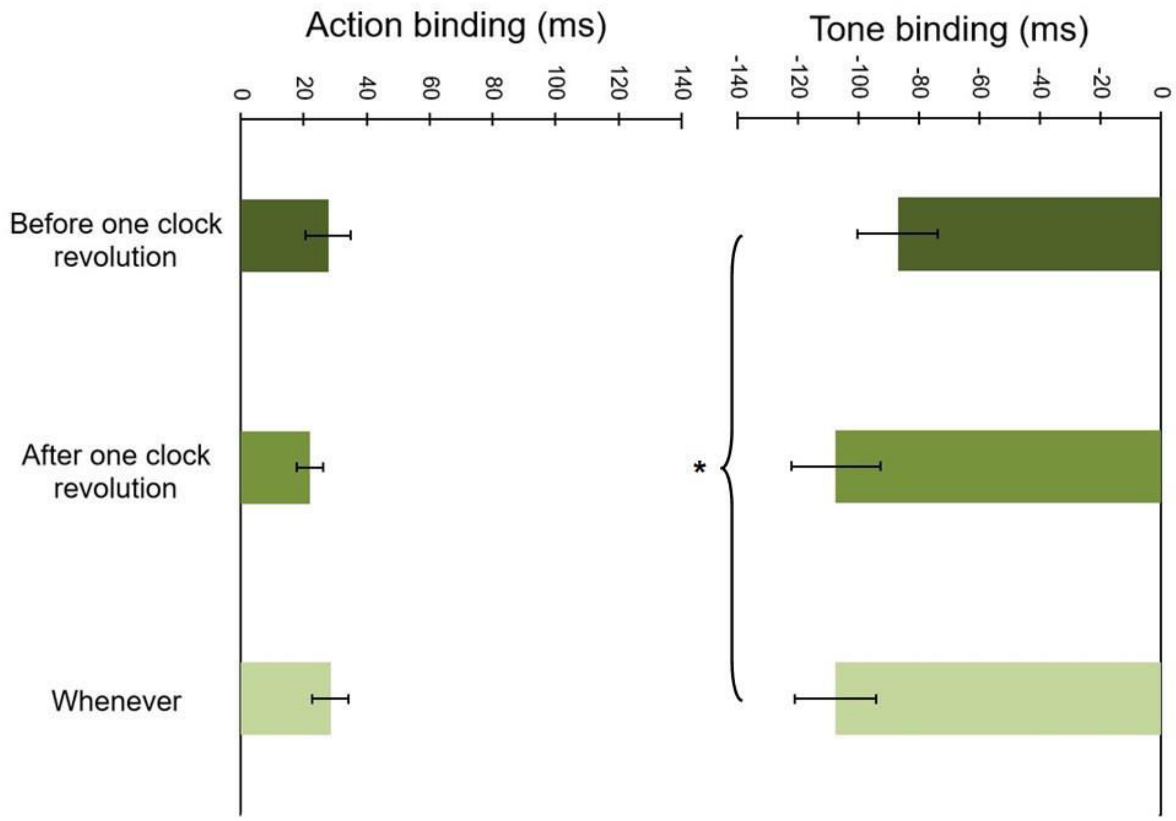
Participants' mean raw and baseline-corrected action and tone judgment errors across all three action initiation instructions conditions, and their mean raw judgment error in the baseline tone block

Action initiation instructions condition	Judged event		Mean raw judgment error (ms) (SD)	Mean shift from baseline (ms) (SD)
Before one clock revolution	Action	baseline	-3.31 (53.52)	27.85 (43.94)
		operant	24.54 (56.18)	
	Tone	operant	-99.13 (107.33)	-87.06 (81.62)
After one clock revolution	Action	baseline	-15.74 (41.79)	22.11 (25.63)
		operant	6.37 (49.50)	
	Tone	operant	-119.55 (109.39)	-107.49 (90.67)
Whenever	Action	baseline	-14.72 (55.18)	28.47 (35.71)
		operant	13.74 (57.72)	
	Tone	operant	-119.74 (105.54)	-107.68 (82.27)
	Tone	baseline	-12.06 (63.48)	

My analysis rendered a medium-sized, ambiguously non-significant main effect of Action initiation instructions, $F(2, 74) = 3.01$, $p = .055$, $\eta_p^2 = .075$, and a significant main effect of Event, $F(1, 37) = 94.92$, $p < .001$, $\eta_p^2 = .720$, reflecting that action and tone binding were significantly different from each other. The interaction between Action initiation instructions and Event was non-significant $F(2, 74) = 1.98$, $p = .145$, $\eta_p^2 = .051$.

Figure 4.2

Participants' performance as a function of action initiation instructions



Note. The error bars represent SE. * indicates a significant difference in tone binding between the "Before" and "Whenever" condition (p < .05).

Further exploratory analyses

Due to a visual inspection of mean tone binding scores and their associated error bars in **Figure 4.2**, I wanted to test whether a follow-up analysis looking into the effect of Action initiation instructions on Tone binding would render significant results. Thus, I conducted a one-way repeated-measures ANOVA with Action initiation instructions (Before, After, Whenever) as a within-subject factor and mean baseline-corrected *tone* judgment error as a dependent variable. I found a significant main effect of Action initiation instructions on Tone binding, $F(2, 74) = 3.28$, $p = .043$, $\eta_p^2 = .082$. Bonferroni-corrected pairwise comparisons revealed this was due to a significant increase in tone binding in the

“Whenever” relative to the “Before” condition, p = .032, Cohen’s d = 0.437. The two other comparisons in tone binding between the “Before” and “After”, and the “After” and “Whenever” conditions were non-significant, p = .124, Cohen’s d = 0.343, and p = .100, Cohen’s d = 0.003, respectively.

Cumulatively, these results suggest that the link between action initiation instructions and intentional binding is ambiguous, yet that this link is likely to be driven by a significant increase in tone binding in the condition in which participants could act whenever they wanted to relative to the condition in which they were constrained to act before a certain point in time.

DISCUSSION

This chapter investigated the influence of action initiation instructions on intentional binding. I looked at binding when participants were instructed to move a) before one revolution of the clock face had been completed, b) at any point in time after one revolution of the clock face had been completed, and c) whenever they felt like it. My results reveal that action initiation instructions did not interact with action and tone binding differently. However, paradoxically, follow-up exploratory analyses indicate that there was a significant increase in tone binding in the “Whenever” relative to the “Before” condition. This finding, coupled with the ambiguous main effect of instructions on binding as a whole, suggests that manipulations of the WHEN component of intentional action *might* affect human agency.

Theoretical implications

The above-mentioned significant increase in tone binding as a function of constraints placed on the timing of action goes against the findings of Wenke and colleagues (2009). These authors found no difference in binding as a function of whether the action initiation time was externally imposed vs freely chosen. One possibility for this difference in results might reside with the experimental design I used. Unlike Wenke et al. (2009), my design assessed the *individual* contribution of the WHEN component to intentional binding, whereas theirs combined the WHAT and WHEN components factorially as part of four conditions. These authors interpreted their results in the context of *a unitary action selection mode*. On their view, because of *coordination costs* (between multiple action-related information systems), what affects agency is whether the agent can initiate actions fully internally (freedom to choose both what action to make and when to make it) vs fully externally (no freedom). Agency is boosted when these coordination costs are kept to a minimum (i.e. the same system is used to both prepare the action and generate the action experience). In this respect, because my design has not split the action selection mode into two (between the WHAT and WHEN components) and has, thus, kept these costs to a minimum, my results support Wenke and colleagues' interpretation and might suggest that the type of action selection (fully internal vs fully external) affects implicit agency.

Moreover, my results might also corroborate Libet's (1985) criteria for fully agentic actions, the predictions made by the "What, When, Whether" model of intentional action and the indirect evidence supplied by a host of neuroscience studies looking into the link between action initiation mode and some of the classic volition centres of the human brain (the pre-SMA and the DLPFC). More specifically, my results might bring support to the view that an action is fully agentic only if there are no restrictions or compulsions that control the individual's initiation and performance of said action (see Libet, 1985 and Brass & Haggard, 2008). This is something the increased activation in the right DLPFC and in the pre-SMA found by Jahanshahi et al. (1995) and Deiber et al. (1999), respectively, in conditions where agents had full control over the timing of their actions also tentatively suggests. What is more, this is also something that the non-significant difference in tone binding between the "Whenever" and "After" condition might point towards, a finding the implications of which I turn to in the next section.

Practical implications

Beyond the theoretical implications mentioned above, it is worth emphasising that the results of my exploratory analyses did not reveal a significant change in tone binding between the condition in which participants were free to act whenever they felt like it (the "Whenever" condition) and that in which they were free to act whenever they felt like it only after one revolution would have passed (the "After" condition). This might suggest that participants did not perceive these conditions as being qualitatively different and, thus, did not perceive significant constraints on their agentic capabilities in the "After" condition. Therefore, given that the instructions given in the "After" condition are the most common across the binding literature, the most poignant practical implication of this result might be that *a great proportion* of the inter-study variability in binding results is likely to not be marred by this aspect of participants' instructions.

Nonetheless, the change in tone binding as a function of instructed constraints (i.e. in the "Before" vs "Whenever" condition) my data tentatively point to might indeed render important practical implications for the comparability of results across *a small proportion* of the binding literature. I have already shown how investigators vary in their preference for action initiation instructions (see "Introduction"). Given this, my results highlight that this inter-study variability in instructions might mar the comparability and replicability of results across the binding literature that contrasts results obtained from *tasks that ask participants to act before one clock revolution will have passed* vs *those that do not*. Thus, as a precautionary measure, I recommend binding researchers to be consistent in the instructions used and in their reporting.

Moreover, my results might suggest that, if we want to measure volition, instructions of any kind (even those that are intended to facilitate volitional experience; see Libet et al., 1983) are likely to have a *negative* effect on it. Some authors might criticise this view, as granting participants full control over the timing of their actions might lead them to interpret volition as *randomness*. Indeed, one well-documented caveat of common agency measures is that the instructions participants receive with respect to the timing of their actions prompt them to adopt a production strategy that rests on the suppression of habitual or stereotyped actions (Haggard, 2008; Jahanshahi & Dirnberger, 1999). In light of this, it seems reasonable to aim at keeping random responses at bay and bolstering volition by instructing participants more carefully. However, my results point towards the possibility that the level of agency participants experience can be bolstered precisely when restrictions are lifted and participants can act *whenever they see fit* or *whenever they see fit after one point in time designated by the experimenter*.

Limitations and future directions

It should be noted here that, despite the significant effect of action initiation instructions on tone binding revealed by my exploratory analyses, instructions did not seem to interact with action and tone binding differently (see the ns Action initiation instructions x Event interaction). In light of this, and in order to properly elucidate how the WHEN component affects human agency in Libet-style experiments, future research should first seek to directly replicate the present study.

In conclusion, this chapter brings my scrutiny of the effects of manipulations of the Libet clock method on intentional binding to a close by showing that, beyond the clock speed and radius, the action initiation instructions too can affect binding. The next and final experimental chapter of this thesis will steer our focus away from implicit agency to Libet's original dependent variable of interest – W judgments. Consequently, the next chapter assesses how changes to the Libet clock stimulus affect W judgments.

CHAPTER 5

An investigation into how different Libet clock parameters affect intention timing

ABSTRACT

W judgments constitute a widely used intention timing measure. These judgments are typically obtained by using the classic Libet-style paradigm whereby participants are asked to report the time they become aware of their intention to act by using the location of a rotating object on a clock face. The intention timing literature varies in the Libet clock parameters used and reported, which poses questions with respect to the comparability of W judgments across the literature and to their sensitivity to changes in these parameters. Here, I present four experiments that systematically manipulate the Libet clock speed, number of clock markings, length of the clock hand and type of clock radius to see whether this affects intention timing. My results show W judgments can be significantly influenced by the clock speed and number of clock markings. The meaning and implications of these results are discussed.

INTRODUCTION

Up to this point, this thesis has focussed on sense of agency – more specifically, on how changes to the Libet clock may influence the expression of the intentional binding effect. It is worth noting, however, that the Libet clock stimulus itself was originally designed to measure intention awareness, and it is to this that I now turn. This is relevant for two reasons. Firstly, although there has been some debate about the clock stimulus and the role that parameters of the clock may play in dictating intention timing judgments, there has only been very limited research looking at this. As such, I feel that it would be fruitful to redirect my rigorous scrutiny of the Libet clock to the issue of intention awareness.

Secondly, intention awareness is a key feature of human agentic experience (Haggard, 2005a). As stated before, sense of agency refers to the reflexive feeling that “I” can willingly control external events. For this reason, it necessarily follows *the experience of conscious intention*. This experience is suggested to contain two components: a sense of urge and a reference forward to the goal object or event (Haggard, 2005b). The latter component involves, among other features, a prediction of the goal state and an orientation towards the goal, which compound to give rise to our sense of agency. The former component is more egocentric and internal and resembles the urge (i.e. the W judgments) that Libet et al. (1983) asked subjects to report. It is this “urge” that forms the focus of this chapter.

Across the volition literature, W (or intention time) judgments constitute a measure of individuals’ subjective timing of their urge to act. They are widely used to address a variety of issues (e.g. the relationship between meditation practice and conscious intention – Jo et al., 2015 – or motor and attentional impulsivity and conscious intention – Caspar & Cleeremans, 2015). These timing judgments have been associated with brain activity in the pre-supplementary motor area and the intraparietal sulcus (Lau et al., 2004).

As mentioned in **Chapter 1**, Libet and colleagues (1983) originally introduced W judgments in an attempt to study mind – brain causality. In their study, they asked participants to report the time they became aware of their intention to act using the location of a rotating spot on the face of an analogue clock. Simultaneously, the investigators were recording EEG activity from the participants’ scalps. Their central finding was that participants’ intention time judgments were always preceded by a gradual build-up of electrical brain activity (the RP). This led them to conclude that the brain initiates actions before we become consciously aware of our intention to act.

Ever since the publication of this seminal study, the meaning of this finding has been intensely scrutinised. For example, various authors have questioned the capacity of individuals to make W judgments due to the reason that, in everyday life, we are typically unaccustomed to differentiate between *our intention to act* and *the initiation of our action per se* (e.g. Gomes, 2002; yet, see how this point is deconstructed in Haggard & Cole, 2007). Others have suggested that the perceived time of an event might not reflect the time it entered consciousness (e.g. Durgin & Sternberg, 2002), which challenges the accuracy of W judgments. Certain authors have also argued that W judgments depend, at least in part, upon neural activity that *follows* action initiation (Lau, Rogers & Passingham, 2007), or that we *infer* the time of our intentions (W time) from the perceived time of action execution (M time) (Maoz, Mudrik, Rivlin, Ross & Mamelak, 2014).

The meaning of the relationship between W time and the RP onset has also been debated. For instance, Banks and Pockett (2007) have discussed the coupling between W time and M time in the context of intentional binding. These authors have argued that, if in the case of voluntary actions W gets attached to M and, thus, it too moves forward in time towards the outcome of M, this might lengthen (yet not *fully* explain) the lag between the RP onset and W. In a similar vein, Haggard and Libet (2001) have likened the perceived onset of our intentions to the perceived onset of a speech stimulus and have noted that W judgments might illustrate the tendency to judge the centre of an event which extends in time (*the p-centre phenomenon*; see also Fingo, 1985 for a similar hypothesis whereby the RP is postulated to represent the beginning of an intention awareness *process* rather than the marker of *a single event*). The validity of the RP itself has been discussed as well. Haggard and Eimer (1999) have argued that, when it comes to shedding light on mind – brain causality, the smaller lag between the lateralised readiness potential (so called LRP) and W time, rather than the larger lag between the RP and W time, represents a more appropriate measure of motor preparation.

Some concerns with Libet's findings also focus on the clock stimulus itself. It has been suggested by some (e.g. Klein, 2002; Joordens et al., 2002; 2004) that the time delay between W and the RP onset might be artificially lengthened by certain phenomena that could be triggered by properties of the clock stimulus. For example, the representational momentum effect (Hubbard & Bharucha, 1988) could cause participants to overestimate their intention time judgments in line with the clockwise motion of the rotating object on the clock face. The flash-lag effect (Nijwahan, 1994) may cause them to extrapolate their intention time judgments into the future. Finally, the prior-entry effect (Spence & Parise, 2010) might result in participants reporting their intention time judgments later than they actually were due to intermodal division of attention (between the act of becoming aware of their intentions and the act of reporting this awareness using the location of the spot on the perimeter of the clock face).

In light of potential issues with the clock stimulus, some researchers have investigated its effect on W judgments. For example, in an attempt to check whether individuals are capable of accurately reporting the time of their intentions, Miller et al. (2010) asked participants to make W judgments in the context of a go/no-go task using the Libet clock. They found the clock to significantly bias these judgments by making them implausibly early or late (relative to their estimated actual time of occurrence) and inaccurate. In a similar vein, Miller et al. (2011) found the RP preceding intention time judgments to be significantly lengthened by the act of monitoring the clock (relative to a condition in which participants were asked to make these judgments without using a clock). Banks and Isham (2011) found the lag between W and M time to depend a great deal on the type of clock used, with the smallest lag being noticed when using a digital clock (-30ms) as opposed to a standard analogue clock (-138ms) or a clock containing numbers that changed at random (-380ms). Such findings speak to the way in which both the manner of reporting intention time and the type of clock used can significantly bias intention time judgments.

As we can see, the use of the Libet task in the context of W judgments has been questioned. However, to date, no investigators have probed the influence of different clock parameters on W judgments. This matters because, as we have just seen, the mere act of producing these judgments by using the Libet clock seems to influence them. This, in turn, is crucially important, as the timing of W (in relation to the RP or the LRP) is fundamental to the interpretation of Libet's findings with respect to free will.

Furthermore, there are practical benefits from this investigation. There is a striking lack of consistency in the settings of the Libet clock across studies looking at W judgments. There are differences in clock radius (e.g. Caspar & Cleeremans, 2015; Rigoni, Demanet & Sartori, 2015), in rotating object length (e.g. Haggard & Cole, 2007; Strother & Obhi, 2009) and in the number of clock markings (e.g. Rigoni, Brass & Sartori, 2010; Matute, Cubillas & Garaizar, 2017). Confounding this issue of inconsistency is a general under-reporting of the Libet clock parameters used. If I find that the settings of the Libet clock influence W judgments, then this will emphasise the need to report these settings and to be consistent in them.

In conclusion, this series of four studies explores, for the first time, how systematically manipulating the clock speed, number of clock markings, length of the clock hand and type of clock radius impacts on W judgments.

EXPERIMENT 1

In this experiment I investigated the effect of clock speed on W judgments. In **Chapter 2**, we have already seen how clock speed can affect intentional binding by increasing tone binding in line with an increase in speed. Moreover, Danquah et al. (2008) have showed it can also affect S judgments by making them significantly less anticipatory in line with an increase in speed. This issue is pertinent, because Libet et al. (1983) used S judgments to correct W estimates. They used this correction method as a means to verify the subjects' ability to report "the actual time" of intention awareness. Because S judgments (as opposed to W judgments) can be reliably operationalised as the difference between the perceived (as reported by the participants) and actual time (as recorded by the EMG machine) of somatosensory stimulation, Libet and colleagues likened them to W judgments and used them to deduce the putative difference between the perceived and actual time of intention onset.

Although this correction method has been contested by many (e.g. Wasserman, 1985; Salter, 1989), the suggested parallels between S and W judgments, coupled with the above-mentioned significant effects of clock speed on both intentional binding and S judgments, warrant an investigation of this issue in the context of W judgments.

I used the same clock speeds as I did in **Chapter 2** while administering the typical intention timing task whereby participants are asked to report the time they became aware of their intention to act by using the location of the clock hand on the clock face they had in view at the time of the event of interest.

METHODS

Participants

26 participants (M_{age} = 20.61yrs, *SD* = 4.00, age range: 19 – 37, no males) were recruited to the study using the Goldsmiths Research Participation Scheme. They were compensated with 7.5 course credits for approximately 1hr30mins of experimental time. The testing session for this experiment alone took 15mins. I initially aimed at testing 40 individuals, as a survey of recent intention time Libet clock studies indicated a sample size of between 13 and 60 participants as being appropriate (e.g. *N* = 13, Tabu et al., 2015; *N* = 17, Alexander et al., 2016; *N* = 60, Matute et al., 2017; *N* = 24, Isham, 2020). However, the COVID-19 outbreak prevented me from finishing the data collection. From the sample of 26 participants, one was excluded (see "Data analysis"). The remaining 25 participants (M_{age} = 19.84yrs, *SD* = 5.68, age range: 19 – 37, no males) were all right-handed, had normal or corrected-to-normal vision, with no self-reported psychiatric or neurological disorders or substance usage that might interfere with their cognitive performance. They provided written informed consent prior to participation and the experiment was approved by the Department Ethics Committee at Goldsmiths, University of London.

Materials

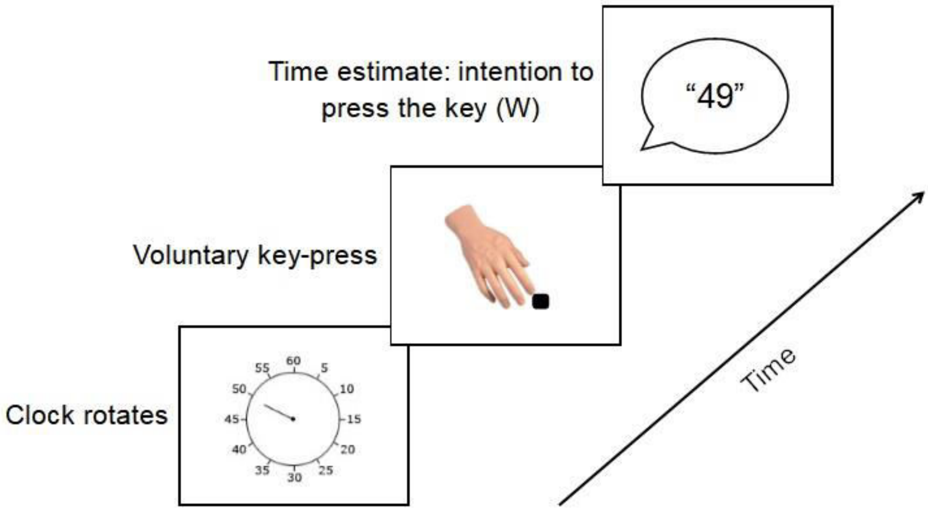
Intention time was measured using a Libet clock task programmed in JAVA (version 6; ORACLE, 2011). The clock measured 21mm in diameter, featured a 9mm hand and was marked at conventional intervals (5, 10, 15, etc.) in all blocks and conditions. The clock rotated at three different speeds across separate conditions – 1280ms (fast), 2560ms (the standard rotation speed used in the field) and 5120ms (slow) per clock revolution. The clock hand stopped rotating after 1000 – 2500ms ms following the action (see below).

Procedure

As part of each clock speed condition, participants completed an intention timing task containing 27 trials. This resulted in three conditions (81 trials) per participant, which were fully randomised within participants. They also completed three practice trials prior to each speed condition. That resulted in nine additional trials per participant, which were discarded. Afterwards, they started the main task.

Figure 5.1

Standard trial structure in this series of experiments



Note. Participants were required to press a key at their own pace and report the time they first became aware of their intention to press the key.

As can be seen in **Figure 5.1**, participants were instructed to sit in front of a computer depicting a standard Libet clock and press a pre-specified key with their right index finger whenever they felt like it. They were asked to report the moment at which they first became aware of the urge to press the key. To make this judgment they simply had to note where on the clock face the hand was pointing to when they had the urge to press the key. They reported this number to me at the end of the trial, and I would enter it on their behalf. Like in all previous experiments, I favoured this approach, rather than allowing participants to input their time estimates themselves so as to encourage participants' sustained attention on the task.

Participants sat at a distance of approximately 65cm from the clock face across all speed conditions (visual angle: approximately 1.8°), were informed of any change in clock speed before completing a new condition, given opportunities to rest in-between conditions and were reminded to be as accurate as possible when reporting their intention time estimates.

All four studies presented in this chapter were run in a single session. The study types (i.e. clock speed, markings, clock hand length and radius) were counterbalanced across participants and the experimental conditions (i.e. three clock speed, five clock markings, three clock hand length and three radius conditions) were randomised within participants.

Data analysis

The variable of interest here was the so-called W judgment, which corresponds to the difference in time between participants' reported intention to move and the time of their actual key-presses. This measure was computed on each trial and then averaged across trials, to give an average W judgment for each participant, for each clock speed condition.

Individual trials containing raw judgment error outliers within participants were excluded (M +/-2.5 SDs). This criterion resulted in 1.20%, 1.60% and 2.26% trials excluded from the 1280ms, 2560ms and 5120ms clock speed condition, respectively. Outliers at the group level were removed if a combination of factors indicated univariate or multivariate outliers (Field, 2013; these included skewness and kurtosis values greater than approximately +/-2.000, significant results rendered by the Kolmogorov-Smirnov test, visual inspection of boxplots and histograms). This was the case with one participant, who was excluded on the basis of boxplot inspection and abnormal kurtosis values.

Statistical analyses

After the above-mentioned participant exclusion was made, the Kolmogorov-Smirnov test did not render any significant results. I also did not find any abnormal skewness or kurtosis values, boxplots or histograms, so I proceeded forward using parametric statistics. I conducted a one-way repeated-measures ANOVA with Clock speed (1280ms, 2560ms, 5120ms) as a within-subject factor and mean W judgment as a dependent variable. I ran Bonferroni-corrected pairwise comparisons.

Mauchly's Test revealed that the assumption of sphericity was violated in the case of one analysis, so below I report the Greenhouse-Geisser correction.

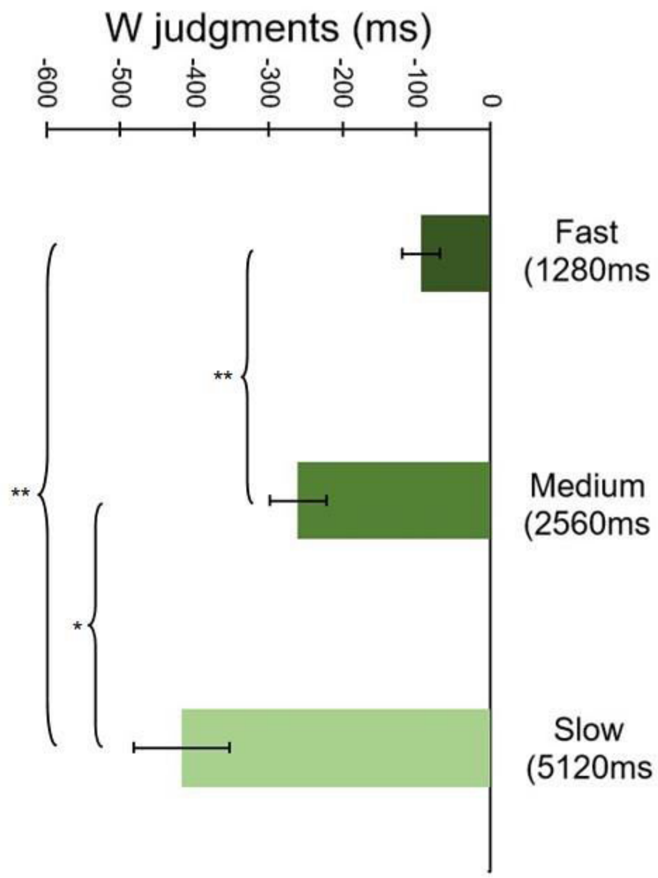
All data and statistical analyses were conducted using MATLAB (v. R2012a, MathWorks, Natick, MA) and IBM SPSS Statistics (v. 22 & 23; 2014, 2018).

Results

Participants' performance across each clock speed condition can be found in **Figure 5.2**.

Figure 5.2

Participants' mean W judgments (ms) as a function of clock speed



Note. Error bars depict SE. ** and * indicate a significant difference between W judgments in the 1280ms vs 5120ms, 1280ms vs 5120ms, and 2560 vs 5120ms condition ($p < .01$ and $p < .05$, respectively).

The analysis revealed a significant main effect of Clock speed on mean W judgment, $F(1.24, 29.98) = 15.16$, $p < .001$, $\eta_p^2 = .387$, this showing that Clock speed affected participants' W judgments.

Bonferroni-corrected pairwise comparisons examining this effect revealed a significant difference in mean W judgment between the 1280ms and 2560ms, $p = .001$, Cohen's $d = 0.825$, 2560ms and 5120ms, $p = .017$, Cohen's $d = 0.608$, and 1280ms and 5120ms clock speed conditions, $p = .001$, Cohen's $d = 0.831$. This suggests clock speed affects W judgments by making them significantly less anticipatory in line with an increase in speed.

Discussion

In this experiment, I investigated the effect three different Libet clock rotation speeds have on W judgments by asking participants to report the location of the clock hand on the clock face they had in view when they first became aware of their intention to press a key.

My data suggest W judgments to decrease (the delay between W and action onset is reduced) in line with an increase in clock hand rotation speed. This result echoes that of Danquah et al. (2008), who found S judgments too to decrease in line with an increase in speed. This speaks to the potential parallels between S and W judgments that other investigators (e.g. Wasserman, 1985; Salter, 1989) have discarded. Importantly, this result also renders possible implications for the link between W time and M time, and W time and the RP or LRP onset (*not* measured in this experiment) by suggesting that individuals' perceived intention time (relative to their perceived time of action initiation and to the onset of preparatory brain activity) can change depending on how fast the clock stimulus is rotating. These aspects emphasise both the need for consistency in this Libet clock parameter across the intention timing literature as well as mindfulness of its effects on intention timing when interpreting the relationship between brain, intention and action. In light of this, the next experiment considers the influence of another clock feature on W judgments; namely, clock markings.

EXPERIMENT 2

This experiment focusses on the potential effect of the number of clock markings on W judgments. As we may recall from **Chapter 2**, in the context of intentional binding research there is a degree of inconsistency in the number of clock markings present on the Libet clock. This is also true of studies on intention timing. For example, some investigators use a clock marked in steps of 5 with additional radial lines displayed in steps of 2.5 (e.g. Libet et al., 1983; Matute et al., 2017), others a clock marked in steps of 5 only (e.g. Haggard & Cole, 2007; Tabu et al., 2015) or a clock marked more granularly, in steps of 1 (e.g. Rigoni et al., 2010; Rigoni et al., 2015). Some authors modified the clock substantially and used a rectangular clock marked at 50 equally spaced positions, with four additional tic marks appearing in the corners of the rectangle (e.g. Miller et al., 2010; Miller et al., 2011) and others, at least for a select portion of the experimental task, seem to have used an unmarked clock (Caspar & Cleeremans, 2015).

In order to shed light on the putative effect of the number of clock markings on W judgments and its implications for the W judgments literature, I used the same types of clock markings as I did in **Chapter 2** while administering again the standard intention timing task described above.

METHODS

Participants

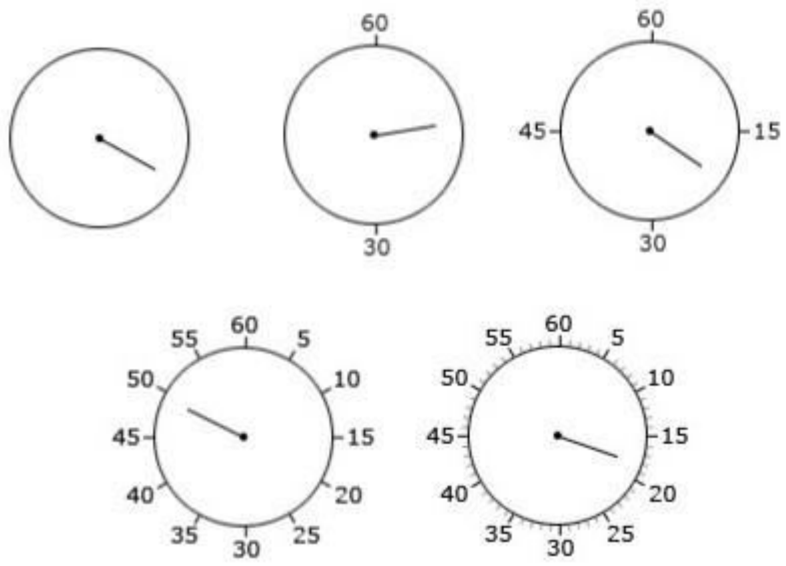
In this experiment took part the same participant sample that did in Experiment 1. The testing session for this experiment lasted 25mins. They provided written informed consent prior to their participation and the experiment was approved by the Department Ethics Committee at Goldsmiths, University of London.

Materials & Procedure

Intention time was measured using the same Libet clock task reported above. The clock rotated at the standard speed of 2560ms per revolution, featured a 9mm hand and measured 21mm in diameter. This time, I used five types of clock markings across five separate conditions ("No markings" – the clock was not marked at all, 30' – the clock was marked in steps of 30', 15' – the clock was marked in steps of 15', 5' – the clock was marked in steps of 5', 5' + 1' – the clock was marked in steps of 5' and in more granular steps of 1'; see **Figure 5.3**).

Figure 5.3

The five types of clock markings (No markings – top left, 30' – top centre, 15' – top right, 5' – bottom left, 5' + 1' – bottom right) used in Experiment 2



Data analysis

The same data analysis method as in the previous experiment was used. This time, 1.73%, 1.60%, 2.13%, 3.06% and 1.60% of trials from the "No markings", 30', 15', 5' and 5' + 1' conditions, respectively, were removed.

Statistical analysis

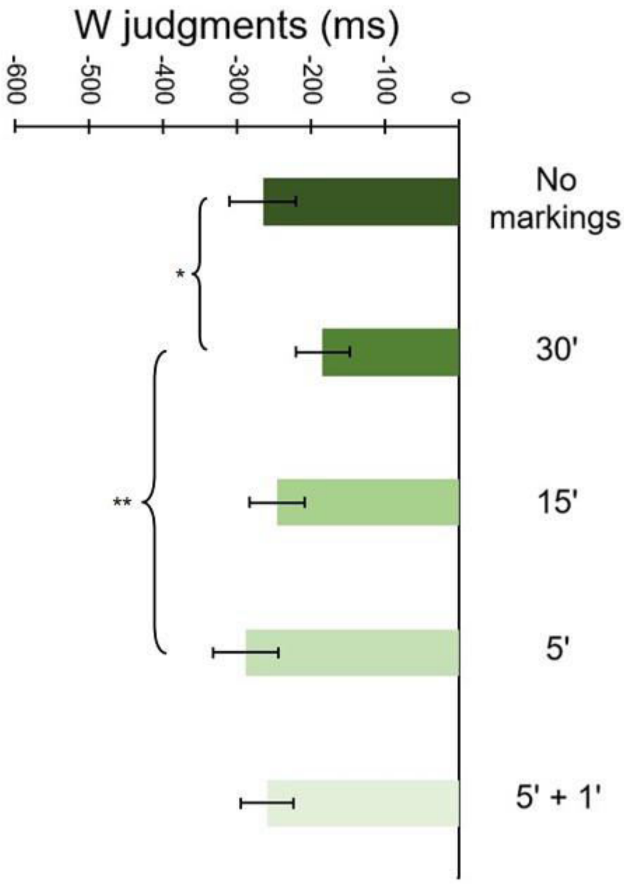
I conducted a one-way repeated-measures ANOVA with Clock markings (No markings, 30', 15', 5', 5' + 1') as a within-subject factor and mean W judgment as a dependent variable, and further Bonferroni-corrected pairwise comparisons.

Results

Participants' mean W judgments as a function of clock markings can be seen in **Figure 5.4**.

Figure 5.4

Participants' W judgments as a function of clock markings



Note. Error bars depict SE. ** and * indicate a significant difference between W judgments in the 30' vs 5' and "No markings" vs 30' conditions ($p < .01$ and $p < .05$, respectively).

A one-way ANOVA showed a significant main effect of Clock markings on W judgments, $F(4, 96) = 4.43$, $p = .002$, $\eta_p^2 = .156$, indicating that this clock parameter significantly affects intention time estimates.

Further Bonferroni-corrected pairwise comparisons conducted in order to shed light on this effect revealed a significant difference in W judgments between the "No markings" and 30' condition, $p = .036$, Cohen's $d = 0.646$, and the 30' and 5' condition, $p = .002$, Cohen's $d = 0.867$. There was also a trend-level, medium-sized difference in W judgments between the 30’ and 5’ + 1’ condition, p = .088, Cohen’s d = 0.569. No other comparisons approached significance, all ps > .05.

All these results combined suggest that the number of clock markings affects W judgments in such a way that, when the clock has a minimal amount of detail displayed (i.e. the 30’ condition), as opposed to no detail at all (i.e. the “No markings condition”) or a lot of detail (i.e. the 5’ and 5’ + 1’ conditions), these judgments are more anticipatory.

Discussion

This study looked into how the number of Libet clock markings affects intention timing. My results show W judgments to be significantly less anticipatory when the clock contains a small number of markings (i.e. the 30’ condition) relative to when it does not contain any markings at all (i.e. the “No markings” condition) or when it contains a large number of markings (i.e. the 5’ and 5’ + 1’ conditions). This speaks to how variations in this clock parameter can affect the comparability of results across the intention timing literature. Moreover, given that these results imply that W time is more or less closer to the RP and LRP onset or to M time depending on the number of Libet clock markings used, this also brings to light a further factor that might cloud the interpretation of the relationship between brain, intention and action as measured by Libet-style paradigms. In the next experiment, we turn our attention to clock hand length and the possible influence this might have on intention timing judgments.

EXPERIMENT 3

This experiment focusses on the influence the length of the clock hand might have on participants’ W judgments. I have already investigated this issue in **Chapter 2** in the context of intentional binding and found no significant effect of clock hand length on either action or tone binding. Nonetheless, this is another parameter that seems to vary across the W judgments literature (e.g. a clock hand measuring 12mm – e.g. Haggard & Cole, 2007 –, 17.5mm – e.g. Tabu et al., 2015 – or equal to the radius of the clock – e.g. Strother & Obhi, 2009). Moreover, this parameter is widely *under*-reported, which highlights the need to study the effect that this particular clock feature might have on W judgments.

METHODS

Participants

In this experiment, the sample comprised the same 25 individuals who took part in the previous two studies. This specific testing session lasted for 15mins. They provided written informed consent prior to their participation and the study was approved by the Department Ethics Committee at Goldsmiths, University of London.

Materials & Procedure

Intention time was measured identically as per the procedure reported at length above. I used a standard clock stimulus with the hand rotating at 2560ms per revolution, the clock face was marked in steps of five and measured 21mm in diameter. This time, I used three types of clock hand length (8mm, 10mm, 13mm; see **Figure 5.5**).

Figure 5.5

The three types of clock hand length (8mm – left, 10mm – centre, 13mm – right) used in Experiment 3

Data analysis

I used the same data analysis method as previously reported in the context of Experiments 1 and 2. This time, 1.33%, 1.60% and 1.60% of trials were removed from the 8mm, 10mm and 13mm condition, respectively.

Statistical analysis

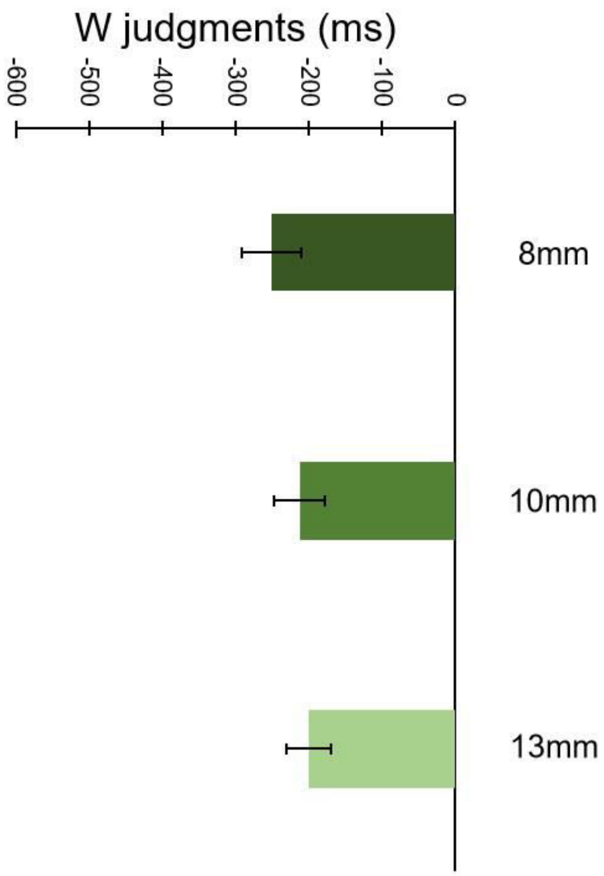
A one-way repeated-measures ANOVA with Clock hand length (8mm, 10mm, 13mm) as a within-subject factor and mean W judgment as a dependent variable was run.

Results

Participants' mean W judgments across all clock hand length conditions can be seen in **Figure 5.6**.

Figure 5.6

Participant's W judgments as a function of clock hand length



Note. Error bars depict SE.

The one-way ANOVA revealed no main effect of Clock hand length on W judgments, $F(2, 48) = 1.45$, $p = .245$, $\eta_p^2 = .057$. This suggests this particular Libet clock parameter does not significantly affect intention time judgments.

Discussion

This experiment looked into the effect that the length of the clock hand might have on W judgments. Unlike in Experiments 1 and 2, I found this particular Libet clock parameter to bear no significance on the magnitude of W judgments. This implies that variations in clock hand length are unlikely to represent a problem for the comparability of results across the intention timing literature or carry any significant weight on discussions about mind – brain causality in the context of Libet-style experiments.

EXPERIMENT 4

Further to the parameters investigated above, as part of this experiment the main point of focus was the clock radius. In **Chapter 3**, we have seen how this parameter varies widely across the intentional binding literature, with my data suggesting it significantly interacts with the binding effect. Intention timing investigators too vary in their preference for clock radius. For example, some prefer to use bigger Libet clocks (e.g. 74.3mm diameter, Caspar & Cleeremans, 2015; 90mm diameter, Rigoni et al., 2010; 101.6mm diameter, visual angle: approximately 9.6° Isham, 2020) while others opt for smaller clock faces (e.g. 13mm diameter, Haggard & Eimer, 1999; 35mm diameter, visual angle: 4° Tabu et al., 2015; 40mm diameter, Rigoni et al., 2015). This study's aim is to look into how different types of clock radius affect W judgments and, thus, to shed light on whether inconsistencies in this Libet clock feature might represent a problem for the intention timing literature.

METHODS

Participants

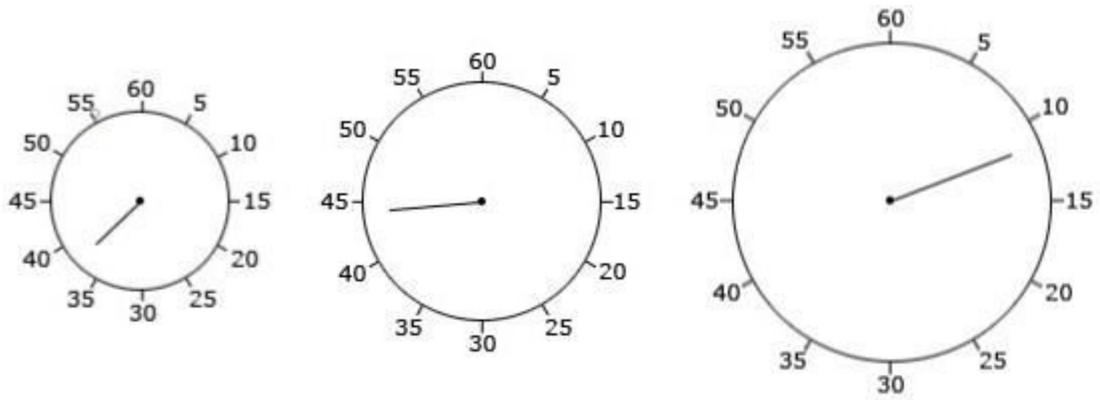
Similarly to the previous experiments reported above, the same participant sample comprising 25 individuals took part. Similarly to Experiment 3, this specific testing session lasted for 15mins. They consented to taking part in this experiment, which was approved by the Department Ethics Committee at Goldsmiths, University of London.

Materials & Procedure

I obtained participants' W judgments as part of the same experimental protocol reported above. The clock rotated at the standard speed of 2560ms per revolution and was marked in steps of five. This time, I used three different types of clock radius across three separate conditions. The clock hand length increased in line with an increase in radius so as to preserve the clock face-to-clock hand ratio (small radius – 10.5mm, clock hand length – 9mm, visual angle – approximately 1.8°; medium radius – 17.5mm, clock hand length – 13mm, visual angle – approximately 3.1°; large radius – 23.5mm, clock hand length – 19mm, visual angle – approximately 4.1°; see **Figure 5.7**).

Figure 5.7

The three types of clock radius (small – 10.5mm, left; medium – 17.5mm, centre; large – 23.5mm, right) used in Experiment 4



Data analysis

I used the same data analysis method I previously reported. As part of this specific study, I excluded 1.86%, 2% and 2.53% of trials from the small, medium and large radius condition, respectively.

Statistical analysis

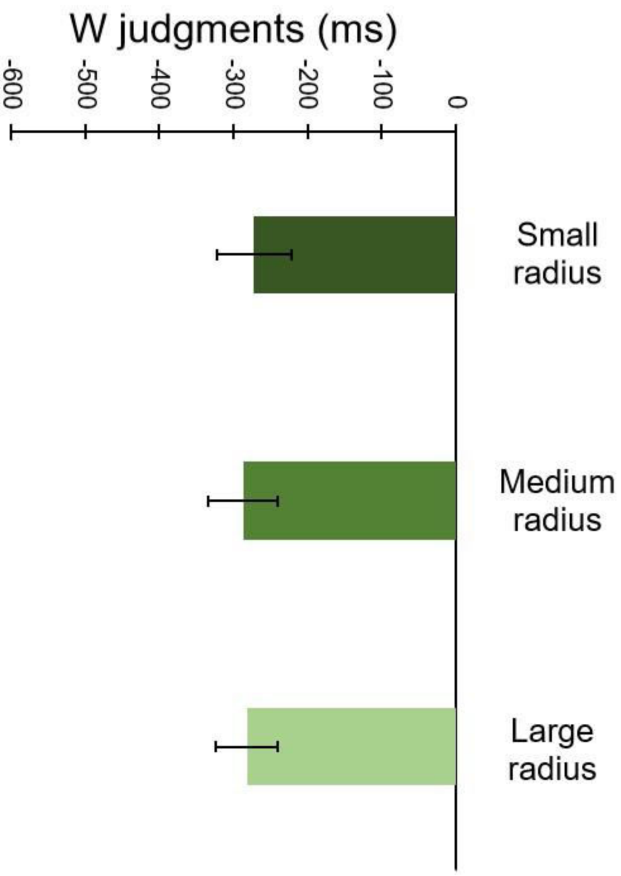
In order to analyse these data, I ran, as before, a one-way ANOVA with Clock radius (small, medium, large) as a within-subject factor and mean W judgment as a dependent variable.

Results

Participants' W judgments across all clock radius conditions can be found in **Figure 5.8**.

Figure 5.8

Participants' W judgments as a function of clock radius



Note. Error bars depict SE.

The one-way ANOVA revealed no main effect of Clock radius on mean W judgment, $F(2, 48) = 0.21$, $p = .809$, $\eta_p^2 = .009$. This suggests the choice of clock radius did not significantly impact on participants' intention timing.

Discussion

In this study I manipulated the Libet clock radius in order to see whether this changes W judgments to any significant extent. Interestingly, I found that whether the clock is small, medium or large does not change participants' intention timing. This suggests that this particular clock feature can be manipulated at ease across the specialised literature, with no implications for the inter-study variability in the magnitude of intention time judgments or for dialogues regarding the link between brain, intention and action as investigated using Libet-style tasks.

GENERAL DISCUSSION

This series of four studies investigated the effect the Libet clock speed, number of clock markings, length of the clock hand and type of clock radius have on intention timing. As part of the standard Libet clock experimental setup, participants were asked to report the time they became aware of their urge to press a key. My data suggest that W judgments can be significantly affected by the rotation speed of the clock, this result mirroring the effect of speed on tone binding discussed in **Chapter 2**. However, my data also suggest that W judgments can be significantly affected by the number of clock markings, which does not seem to be the case with intentional binding (see **Chapter 2**). Below I elaborate on the theoretical and practical implications of these results.

Theoretical implications

It has been noted that, when it comes to mind – brain causality, the link between the perceived time of our intentions to act as measured by the Libet clock stimulus (W time) and the onset of preparatory brain activity (the RP or the LRP) is difficult to interpret due to certain phenomena which can potentially lengthen the duration between the preparatory motor signal and W judgments (e.g. Klein, 2002; Joordens et al., 2002; 2004). More specifically, it might be the case that participants overestimate their intention times in line with the clockwise motion of the clock hand (the representational momentum effect; Hubbard & Bharucha, 1988), extrapolate them into the future (the flash-lag effect; Nijwahan, 1994) or misreport them due to intermodal division of attention (the prior-entry phenomenon; Spence & Parise, 2010). My data suggest that, beyond these confounding phenomena, the choice of clock speed and number of clock markings can also potentially account for further delays between the readiness potential and W judgments. This can also be the case when the metric of interest is the lateralised readiness potential (e.g. Haggard & Eimer, 1999; Miller et al., 2011). This implies that caution should be exercised when drawing any strong conclusions regarding the interplay between brain, intention and action as measured using the Libet clock stimulus. While my data *do not* tackle the issue of the relationship between mind and brain, they emphasise how some parameters of the measurement stimulus used in its assessment might introduce a confounding change in the temporal experience of intention.

A more thorough investigation of this issue requires a look into the possible mechanisms of the effects of these clock parameters on W judgments. For instance, as regards the linear effect of speed on W judgments, the answer might lie with an adaption of the cue integration hypothesis put forward by Lau et al. (2007) to explain why we typically experience the onset of our intentions much later than the onset of the readiness potential. On their view, the RP is split into an early and a late component, each of which has a lower and higher weight, respectively, attached to it. Due to the difficulty of estimating intention time, the brain is purported to combine the information rendered by the early and late RP components, which might result in us experiencing our intentions as arising closer to the more reliable late RP component. The same rationale could potentially be applied in the case of clock speed. Similarly to my interpretation of the effect of speed on tone binding (see **Chapter 2**), if we assume that higher speeds evoke more perceptual uncertainty in the subject than lower speeds do, this might explain why W judgments are progressively less anticipatory the faster the clock speed is. Participants might become more uncertain the faster the clock rotates, this increasing their reliance on the more reliable late component of the RP. This, in turn, results in increasingly less anticipatory (i.e. later) W judgments. Future research is advised to investigate this compelling matter further.

Practical implications

The data reported in this series of studies suggest that the inconsistency in and under-reporting of the Libet clock parameters used can affect the comparability of results across the intention timing literature and, consequently, mar replication attempts. This is less problematic in the case of the Libet clock speed, as this feature tends to remain constant (the standard speed used is approximately 2560ms per clock revolution). Nevertheless, this can indeed pose problems when it comes to the number of clock markings, as this parameter varies. For example, in an initial experimental phase, Caspar and Cleeremans (2015) seem to have asked their subjects to view a black spot rotating on *an unnumbered* clock face and use the location of the spot on the clock edge to make either W or M judgments. Then, in a subsequent phase, the participants were required to report these two events using *a numbered* clock. Fortunately, my data suggest that W judgments are similarly anticipatory (relative to the actual time of action onset) when an unnumbered clock vs a clock marked more granularly is used (see **Figure 5.4** for a visual comparison of the "No markings" vs 5' conditions). This suggests that, despite this intra-study variability in clock markings, the correspondence between participants' initial vs subsequent W and M judgments in Caspar and Cleeremans's study should not be marred. However, for consistency, I encourage future investigators to avoid such intra-study manipulations of Libet clock markings, especially if attempting to use a clock marked in steps of 30' (as my data suggest W judgments are particularly affected by this manipulation; see **Figure 5.4**).

At the opposite end of the spectrum, my results suggest that the clock radius or rotating object length can indeed be manipulated at ease across the intention timing literature in order to suit diverse experimental needs. For instance, the clock could be bigger and feature a longer hand so as to minimise the perceptual burden of the task when testing certain populations in whom this aspect of the task might be a problem, such as in specific psychiatric or neurological patients or certain age groups (e.g. older adults or younger children). It may also be the case that this alteration of the clock stimulus would be advantageous when testing typical populations, if it reduces perceptual/cognitive fatigue (which is a concern with these tasks, as trial numbers can often be quite high).

Limitations and future directions

As noted in the “Methods” section, testing for these experiments was halted by the COVID-19 outbreak. As such, it is likely that these studies are somewhat underpowered. This is of note especially where there were trends in the data that did not reach significance (i.e. in the clock markings experiment) – this effect might approach significance if the sample size were bigger.

It is also worth emphasising that I did not collect EEG data and was therefore not able to measure the RP or the LRP. Although not essential for the purposes of my experiments and the manipulations that I ran, these data would allow me to say with more certainty how changes in W relate to the preparatory motor signals in the brain. Without these data, my suggestions are merely speculative at this stage, and they would constitute something for future research to address.

CHAPTER 6

General discussion

Sense of agency is part and parcel of human experience. It lies at the heart of our self-awareness (Jeannerod, 2003), is crucial for social interaction (Obhi & Hall, 2011), and underpins our sense of responsibility (Caspar et al., 2016). This thesis has focussed primarily on how changes to the Libet clock stimulus and task instructions influence the intentional binding effect (Haggard et al., 2002). In this thesis I have also broadened my investigation of the Libet clock to examine the effect of manipulations on the experience of conscious intention.

My work suggests that intentional binding is significantly affected by manipulations of clock hand *rotation speed* and the clock *radius* (see **Chapters 2** and **3**). Beyond the clock stimulus per se, I have also found binding to be affected by manipulations of *the action initiation instructions* that participants get given in Libet-style experiments (see **Chapter 4**). In terms of our capacity for timing our intentions to act, my thesis has demonstrated that W judgments are particularly sensitive to manipulations of the clock hand *rotation speed* and *number of clock markings* (see **Chapter 5**).

This final chapter is split into four broad sections. The first two sections set out to summarise my key findings and discuss their theoretical and practical implications. In the third section, I acknowledge some of the limitations of my work and offer investigators potential avenues I deem worthy of future scientific exploration. I conclude with a section in which I outline the key take-home messages of my work.

MANIPULATIONS OF THE LIBET CLOCK STIMULUS AND INTENTIONAL BINDING

Theoretical implications

Summary of key results

The first experimental chapter of my thesis examined how different Libet clock manipulations that I hypothesised would induce varying degrees of perceptual uncertainty in my participants would affect binding. To this end, across five different experiments in which I used the standard Libet clock intentional binding experimental setup, I changed the clock speed (1280ms vs 2560ms vs 5120ms per clock rotation), number of clock markings (5' vs 1' + 5'; "No markings" vs 30' vs 15') and length of the clock hand (8mm vs 10mm vs 13mm). The rotation speed of the clock hand was the only Libet clock setting from this cluster that I found binding to be sensitive to. Tone binding increased as a function of an increase in speed and was significantly higher in the 1280ms as opposed to the 5120ms condition. According to Eagleman and Holcombe's (2002) explanation of the mechanisms that putatively underpin binding (see **Chapter 1**), I interpreted this result to reflect our overreliance in uncertain circumstances on temporal contiguity priors. On this view, when faced with extreme (perceptual) uncertainty, participants might have fallen back on a prior belief that dictates that actions and associated outcomes should be close together in space and time. This, in turn, might have caused them to perceive the outcomes of their key-presses earlier than they actually occurred.

In the second experimental chapter of my thesis, I looked at how different clock radius configurations might affect binding. In order to do this, I again administered the typical intentional binding experimental setup and, across three different conditions, I changed the Libet clock radius from small (10.5mm) to medium (17.5mm) and large (23.5mm). My results showed an interaction between intentional binding and clock radius, which follow-up analyses indicated that might have been driven by a trend for tone binding to change as a function of different types of radius.

As part of the third experimental chapter of my thesis, I moved beyond the clock stimulus per se to investigate how different types of action initiation instructions might affect binding. Motivated by methodological issues existent in volition research (e.g. Libet, 1985; Haggard, 2008; Nachev & Hacker, 2014), as well as by an influential model of intentional action (Brass & Haggard, 2008) and by empirical evidence that tests its assumptions (Wenke et al., 2009), I asked participants to complete the same intentional binding task I had used in the previous chapters, yet varied the degrees of freedom they had at their disposal with respect to the timing of their actions. Thus, across three different conditions, participants could make an action a) before (the "Before" condition) or b) after (the "After" condition) a full clock revolution had passed, or c) whenever they felt like it (the "Whenever" condition). I found tone binding to significantly increase in the "Whenever" relative to the "Before" condition. This result partially mirrored that of Wenke and colleagues (2009) and was in agreement with the "What, When, Whether" model of intentional action (Brass & Haggard, 2008) and with the findings of numerous neuroscience studies (e.g. Jahanshahi et al., 1995; Deiber et al., 1999; Lau et al., 2004; Passingham et al., 2010; Khalighinejad et al., 2016). I interpreted this result in the context of a unitary action selection mode, whereby what affects agency is whether our actions are initiated fully internally (i.e. in a free manner) or fully externally (i.e. in a constrained manner).

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The effect of my manipulations on tone vs action binding

The key findings summarised above reveal that only the tone component of intentional binding was influenced by changes to the Libet clock or task instructions. Importantly, differential results of various manipulations on action vs tone binding have been reported before. For instance, Moore et al. (2010) have shown how continuous theta-burst stimulation of the pre-SMA affects tone but not action binding, and Desantis et al. (2011) and Haering and Kiesel (2012) have reported how causality impacts on tone but not on action binding. Moreover, a systematic review and meta-analysis conducted by Tanaka, Matsumoto, Hayashi, Takagi and Kawabata (2019) has shown the magnitude of tone binding to be generally greater than that of action binding, and tone binding (but not action binding) to correlate with the overall magnitude of binding.

Such results might indicate that different mechanisms account for action vs tone binding. Indeed, this is something that certain volition researchers have sought to address. For example, in their review, Hughes, Desantis and Waszak (2013) attempted to clarify the nature of the predictive mechanisms involved in tone binding. They did so by splitting these mechanisms into four different parts: *temporal prediction* (the ability to predict the point in time at which a sensory event will occur), *temporal control* (the ability to use one's action to control the point in time at which a sensory event will occur), *non-motor identity prediction* (the ability to predict what sensory event will appear) and *motor identity prediction* (the ability to use one's action to control what sensory event will appear). From all these predictive mechanisms, they suggested tone binding to depend primarily on *temporal control*.

More recently, in another attempt to explain the mechanisms underlying action vs tone binding, Tanaka et al. (2019) took Hughes et al.'s (2013) work further. Contrary to Hughes and colleagues, they found tone binding to rely more on *temporal prediction* (and action binding on *temporal control*). Tanaka and colleagues interpreted these results in the context of the comparator model of sense of agency (Frith et al., 2000). According to them, *tone binding* might be more sensitive to the feedback signal (also known as a "prediction error") resulting from the comparison between the predicted and actual state of the motor system, whereas *action binding* might be more dependent upon the feed-forward signal generated by the efference copy of the motor command.

If we are to apply this explanation to my own findings, then this would mean that my Libet clock speed, radius and action initiation instructions manipulations might have affected tone rather than action binding due to preferentially interfering with the prediction error generated by the comparison between the predicted and actual state of the motor system than with the feed-forward signal generated by the efference copy of the motor command. However, there are two issues with this possibility. One is that it remains unclear why changes in clock speed and radius or in action initiation instructions would interfere with this prediction error that previous authors link to the processing of action outcomes and *not* to the monitoring of the measurement stimulus used to time these outcomes (i.e. the clock). Another is that, even in the event of this happening, it remains unclear why the other manipulations that I ran (i.e. of the number of clock markings and of the clock hand length) would not significantly interfere with this prediction error too. Future research is advised to delve into and untangle these compelling matters.

So far, I have attempted to address the differential effect of my manipulations on tone vs action binding. However, there is a further question that I deem worthy of exploration, which concerns the differential effect of my speed, radius and action initiation instructions manipulations in operant vs baseline blocks. The next section sets out to discuss this point in more detail.

The effect of my manipulations in operant vs baseline blocks

If my Libet clock manipulations had affected purely subjective timing of events, I would have expected similar results across all experimental blocks and, thus, no significant effects on binding. In other words, the manipulation effects should have been equally present in baseline and operant blocks, and therefore, when calculating the binding effects, they would have shown no overall change as a result. However, that was not the case, and certain manipulations did seem to change the magnitude of intentional binding by influencing operant blocks more than baseline ones.

A possible reason for this (at least in the case of the significant effect of clock rotation speed on binding) might be linked to Eagleman and Holcombe's (2002) uncertainty-based framework (see **Chapter 2**). That is, if the binding effect indeed reflects the influence of a temporal contiguity prior for self-caused events, then this might explain why my clock speed manipulations affected participants' timing judgments in cases where there were operant actions (and two events could be brought together in time), but not in cases where there were not any (such as in baseline blocks, where key-presses and tones occurred individually). If this is the case, in baseline blocks the temporal contiguity prior could not be triggered, and hence, uncertainty could not impact on individuals' subjective timing of events.

Up to this point, I have explained some of my findings in the context of uncertainty. I have favoured this explanation because I have been able to demonstrate the explanatory power of Eagleman and Holcombe's (2002) framework over the effect of clock rotation speed on binding and, by extension, over participants' performance in operant vs baseline blocks. However, it is important to acknowledge that there are possible alternative explanations, and it is to these that I now turn.

Alternative explanations to some of my findings

It is worth noting that, in **Chapter 2**, I assumed that my manipulations had an effect on perceptual uncertainty. In turn, I explained any changes in binding through recourse to Eagleman and Holcombe's (2002) uncertainty hypothesis. However, it is worth reminding ourselves that I did not verify that these manipulations had this effect, and moreover, I did not control for (by measuring) the possible influence of other perceptual/cognitive processes that might have been influenced by my manipulations.

For example, one candidate as an alternative explanation of my effect would be changes in *arousal*. On this alternative view, the clock manipulations may have aroused participants the more extreme they were (i.e. the faster the clock hand rotated, the less markings the clock face had and the shorter the clock hand length was). This increase in arousal, in turn, might have positively impacted on their sense of agency. Indeed, previous research has shown arousal to increase intentional binding. Wen, Yamashita and Asama (2015) looked at the influence of arousal on explicit and implicit sense of agency. They used an interval estimation intentional binding paradigm and elicited arousal in participants by using red (heightened arousal condition) vs black (control arousal condition) action outcomes. They found participants' binding to significantly increase in the red relative to the black action outcome condition.

If arousal is underpinning the effect of speed on tone binding, then this could be consistent with the pre-activation account put forward by Waszak et al. (2012) to explain outcome binding (see **Chapter 1**). More specifically, I propose that heightened arousal engendered by ever faster clock speeds might have risen the neural activity in the perceptual units representing participants' action outcomes over and above the level of activation already caused by these outcomes' representations. This, in turn, might have caused action outcomes to reach awareness even faster than the pre-activation account normally postulates, this increasing participants' tone binding.

Although this arousal-based explanation is possible, it seems to me that an uncertainty-based explanation is more favourable. This is because, as I explain above, I have been able to demonstrate the explanatory power of Eagleman and Holcombe's (2002) framework and, thus, successfully fit it with the theoretical thread running through my thesis (i.e. the link between uncertainty and sense of agency). However, for clarity, future research could investigate these two competing accounts. This could be done, for example, by testing participants using the same experimental setup I used in **Chapter 2** while also checking their arousal levels by means of subjective ratings and objective measurements (i.e. the galvanic skin response). Both these measurements are routinely used to verify whether experimental manipulations induce arousal in participants (e.g. Wen et al., 2015). Thus, if they offer appropriate results, this would then bring further support to the influence of my manipulations on participants' arousal levels (rather than on their temporal contiguity priors).

Moreover, another means to test these two competing accounts would be to subject participants to the same manipulations I use in **Chapter 2** while also recording their brain activity. A host of neuroscience studies have already identified brain circuitry implicated in arousal (e.g. Foucher, Otzenberger & Gounot, 2004) or in perceptual uncertainty (e.g. Baumann & Mattingley, 2014). I propose that, if some of these brain areas get preferentially activated during participants' completion of my tasks, then this would shed light on what cognitive process (arousal vs perceptual uncertainty) my manipulations of the Libet clock target. By extension, such investigations would also have the potential to provide additional evidence in favour of one underlying mechanism of binding (uncertainty-based; Eagleman & Holcombe, 2002) over another (pre-activation-based; Waszak et al., 2012).

Thus far, I have commented upon the theoretical implications of my work on binding. I will now steer this discussion towards the practical implications of my results for the binding literature.

Practical implications

The most important scope of my thesis has been to shed light on what Libet clock parameters that vary between studies affect binding, so that future research can take notice of this when attempting to replicate and/or compare results across the volition literature. In light of this, my work clearly suggests that investigators should be mindful when setting up the clock stimulus. In particular, researchers should be mindful of settings concerning the *Libet clock speed* and clock *radius.* Furthermore, my results also indicate that task instructions are an important variable in determining the magnitude of binding. While clock speed is a less widespread issue (experiments typically stick to the standard speed of 2560ms per clock rotation), the Libet clock radius and instructions require an additional degree of caution, as both vary considerably across the literature (see my extensive discussions on these issues in **Chapters 3** and **4**). Therefore, I recommend both inter- and intra-study consistency in the usage of clock speed, clock radius and participant instructions, so that results between studies and participant groups (within the same study) can be reliably compared and/or replicated.

Conversely, my work demonstrates that the number of Libet clock markings and the length of the rotating object can be safely changed both between studies and participant groups, as these changes are unlikely to affect action or tone binding to a significant extent. This is important, as it suggests that the results of studies that have employed different numbers of clock markings or rotating object lengths can be safely compared. This is also important because it shows that these particular clock parameters can be creatively adapted to fit diverse experimental needs that oftentimes resurface in binding studies (e.g. to reduce boredom/fatigue, particularly in atypical populations, senior adults or children, by, for example, using *a maximal number of clock markings* and *a long clock hand*).

At this stage, although this has not been an overt aim of my thesis, curiosity might prompt us to wonder what Libet clock settings *facilitate* the intentional binding phenomenon. A visual inspection of the results reported in **Chapters 2**, **3** and **4** suggests that binding is most likely when *the Libet clock rotation speed is fast* (i.e. 1280ms), *the radius is medium* (i.e. 17.5mm) and *participants can act whenever they see fit*. Future research should take notice of this and further investigate whether this combination of settings indeed engenders binding in the context of diverse experimental aims and setups.

So far, as part of this final chapter, I have focussed on discussing and challenging my work on binding. I will now turn the lens on the other dependent variable I have investigated as part of my thesis, namely intention timing, and proceed to address the aggregated theoretical and practical implications of my clock manipulations on W judgments.

The link between my methodological work on binding and the wider agency field

In **Chapter 1** I write at length on the link between the Libet clock methodology, binding and sense of agency in general. This link has many implications, some of which are too exhaustive for this section. However, at least two of them merit further consideration. Firstly, one valid question concerns the implications of my findings for the wider field, as rarely, if ever, is binding measured in isolation (like this thesis has done). It is true that, at first sight, the impact of clock speed, radius and instructions on binding my thesis suggests could be regarded as superfluous in the context of other studies that seek *to modulate* binding in order to answer more general questions about agency. However, the main message of my work is that variations in some parameters of a widely used apparatus (i.e. the Libet clock) that measures agency can affect agency, *over and beyond* the way in which other variables might do it. Put otherwise, if a new study sets out to answer a question about agency through recourse to intentional binding and the Libet clock paradigm yet disregards the fact that some changes to the Libet clock affect binding, the influence of that study's independent variable of interest on binding may be *confounded* with that of these methodological changes on binding. This constitutes the primary reason why I advise researchers to attend to Libet clock parameters when replicating or building on previous studies, and, depending on their new work's rationale, keep them constant or modify them.

Secondly, another valid question concerns the implications of my work for what is perhaps the most long-standing debate in the binding literature – whether binding measures *causality* or *volition*. As far as this point is concerned, I feel that my investigation cannot bring a resolution, as my findings can only speak about the mechanics of the Libet clock and how they affect binding (regardless of what binding truly represents). That is, my main findings stand for *any study* that uses the Libet clock method, because the method stays the same regardless of whether a study conceptualises binding as a causality or as an intentionality index. Of course, as the preceding paragraph suggests, attention should be paid to the fact that the effect of clock speed, radius and task instructions on the temporal compression that characterises the binding effect adds to the systematic variation of any statistical model used, which, in turn, may cloud or distort the effect of other independent variables that are designed to pick apart causality from intentionality.

MANIPULATIONS OF THE LIBET CLOCK STIMULUS AND INTENTION TIMING

Theoretical implications

As part of **Chapter 5**, I embarked upon a systematic investigation of the influence of select Libet clock parameters on intention timing. To this end, I manipulated the same parameters I did in the context of intentional binding (i.e. the Libet clock speed, number of clock markings, length of the clock hand and radius of the clock face) to see what effect this would have on participants' W judgments. My four-experiment study found a linear effect of clock speed on intention timing, whereby participants' W judgments became significantly less anticipatory (relative to the actual time of their actions) as a function of an increase in speed. Moreover, my study also found the number of clock markings to influence intention timing by rendering participants' W judgments significantly less anticipatory in the 30' as opposed to the "No markings" or 5' conditions.

As previously mentioned, I deem these results to be important for the interpretation of W judgments with respect to neural measures of motor activity (such as the readiness potential or the lateralised readiness potential). Issues with W judgments have already been noted, and it has been suggested that they are influenced by various factors, including perceptual biases (the flash-lag effect, Nijwahan, 1994; the prior-entry phenomenon, Spence & Parise, 2010) and working memory effects (the representation momentum effect, Hubbard & Bharucha, 1988). These effects can artificially lengthen *the delay between the RP or LRP onset and W time*. Although I have not measured the RP or the LRP myself, the results I report here highlight how different configurations of the Libet clock can bias this delay *further* – different clock speeds and different numbers of clock markings may artificially lengthen the lag between the putative RP/LRP onset and W judgments, or between W judgments and participants' actual time of action. Because this introduces a confounding change in the temporal experience of intention, it may impact on the accurate interpretation of the link between mind and brain.

In terms of potential neurocognitive mechanisms that might underlie these results, in **Chapter 5** I postulate that the linear effect of speed on W judgments, for example, might be accounted for by a cue integration, uncertainty-based mechanism adapted from Lau et al. (2007) and Eagleman and Holcombe (2002). On this view, due to perceptual uncertainty engendered by ever faster clock speeds, participants might have estimated the time of their intentions to act to be ever closer in time to the more reliable *late* RP component the faster the clock speed was. This, in turn, might have resulted in increasingly less anticipatory (i.e. later) W judgments as a function of an increase in speed. Future research is advised to test this hypothesis as well as to investigate other theoretical avenues that might explain the second relationship between Libet clock parameters and intention timing my data demonstrate – the effect of the number of clock markings on W judgments.

Further to these theoretical implications there are also practical implications of this work to be considered.

Practical implications

On the one hand, the effects of clock speed and the number of clock markings on W judgments suggest that researchers should take notice of these settings and maintain consistency in their usage and reporting across the intention timing literature. This will aid replication attempts and the inter-study comparability of results. This is particularly important in the case of *the number of clock markings*, as it varies considerably between studies (see **Chapter 5** for a more ample elaboration on this matter).

On the other hand, my work suggests that the clock radius and the length of the clock are less likely to influence W judgments, and so changes in these parameters are likely to be less of an issue. As is the case with intentional binding (see above), the implications of these findings are two-fold. Firstly, they highlight how the results of studies using different types of clock radius or different clock hand lengths can be safely compared. Secondly, they emphasise how these particular clock parameters can be creatively modified to suit diverse experimental needs.

However, in contrast to the recommendations I make above in the context of binding, in the case of W judgments I deem it inappropriate to recommend that researchers use or avoid particular clock settings when measuring intention timing in Libet-style experiments. This is because, as opposed to action or tone judgments, W judgments have *no objective W time* that can provide a measure of participant's timing accuracy. Thus, we cannot assess the validity of participants' W judgments similarly to how we assess the validity of their M, S or tone judgments. In this respect, my work's aim has *not* been to shed light upon what clock settings engender W judgments. Rather, its aim has simply been to illuminate how different clock manipulations impact on individuals' subjective timing of their intentions to act.

This chapter has, thus far, discussed my most instrumental findings. I will now continue this scrutiny of my work by focussing on some of its limitations. Afterwards, I will bring my thesis to a close with a final section that highlights the key take-home messages of my work.

LIMITATIONS AND FUTURE DIRECTIONS

One potential limitation of the research presented in this thesis is the method by which I determined the sample size. Throughout the thesis, the sample size was set by experimental precedent, rather than using a more formal approach such as power calculations. Although I obtained sample sizes that were consistent with relevant published research, it is possible that they were not appropriate. In light of this, a more formal approach may have been preferable and could, depending on the outcome of those calculations, have helped disambiguate certain findings I obtained (which hint towards the possibility that, if the sample sizes in certain experiments had been bigger, the effects may have been significant).

Moreover, I feel that a further limitation of my thesis resides with my impossibility to explain a host of *non-significant effects*. These include the lack of an effect of clock markings and clock hand length on intentional binding, and the lack of an effect of clock radius and clock hand length on W judgments. Moreover, there are also various *significant results* (i.e. the effect of radius on intentional binding and that of the number of clock markings on W judgments) whose neurocognitive mechanisms I have been unable to fully elucidate. I recommend future research to take notice of these gaps and shed light on them.

CONCLUSIONS

Sense of agency sits at the heart of human experience. It is the cognitive skill that allows us to purposefully master all facets of our lives. As such, it can be argued that sense of agency represents perhaps the foremost cognitive quality humans have been evolutionarily endowed with (Birch & Cobb, 1981), without which they would not be able to survive or thrive (Durham, 1991).

Given the importance of sense of agency, it is imperative that scholars fully scrutinise the measures used to probe it. This is something that my work has accomplished for the very first time. As a result of this, my findings provide important practical and theoretical insights into this most unique of human experiences.

Firstly, my work successfully provides a set of guidelines concerning the effect of the Libet clock method on binding and W judgments that future investigators will be able to consult. In doing so, my work also demonstrates, on the one hand, that this method constitutes *a fairly robust* intentional binding measure, which renders binding sensitive to some of its features, yet not to the extent that the effect is ever abolished altogether. This is important, taking into account that various other intentional binding measures have been developed in an attempt to tackle limitations of the Libet clock (see **Chapter 1**). Moreover, this also fits with Tanaka et al.'s (2019) view according to which the Libet clock method amplifies the magnitude of binding (in contrast to the interval estimation procedure, for example). On the other hand, as regards the usage of this method in the context of W judgments, my results echo the sentiments of other researchers by demonstrating that *the clock stimulus biases them*. However, as the volition literature has so far reported both problems with W judgments per se and with the stimulus used in their measurement (see **Chapters 1** and **5**), at this stage it remains unclear whether, when measuring intention timing, the Libet clock *or* W judgments themselves are problematic.

Secondly, my work provides important theoretical insights into the neurocognitive mechanisms that might underlie intentional binding and W judgments. More specifically, some of my results suggest that both binding and the relationship between preparatory brain activity and W judgments might be driven by a similar, uncertainty-based mechanism. As such, it remains for future research to test the endurance of both the practical and theoretical aspects of my findings in wide and diverse contexts that treat intentional binding and W judgments as dependent variables.

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