Malleability of the Implicit Sense of Agency

at the Personal, Interpersonal and Socio-Political Levels



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Abstract

When we make volitional actions, we feel that we are the authors of such actions and we do not feel as if they simply happen to us. Such phenomenon is known as the sense of agency, i.e., the experience of controlling one's own actions and through such actions, events in the external world. The sense of agency can be measured implicitly via the temporal binding effect, whereby action and its outcome are perceived to occur closer in time. Temporal binding is malleable and influenced by personal and social factors, yet most research manipulates variables within the paradigm itself rather than examining antecedent psychological conditions. This thesis, therefore, investigates the *antecedents* of temporal binding across the personal, interpersonal and socio-political levels. Chapters 3 and 4 examined personal-level factors. Chapter 3 found that sensorimotor disruption impaired temporal binding only when the easier version of the motor-control task was performed first. Repetitively performing the easy motor task also attenuated the task-unrelated temporal binding effect. Both highlight the roles of motor task order as well as task novelty. In Chapter 4, gratitude journaling intervention to increase level of life satisfaction was utilised. The Control group who created daily activity lists showed stronger temporal binding effect than the Journal group who wrote letters to benefactors. Meanwhile, increase in positive mood via video watching did not influence binding magnitude. Chapter 5 explored interpersonal contexts, particularly when being coerced to harm others. Results showed that prosocial disobedience i.e., refusing to send shocks to the victim boosted the temporal binding effect compared with obeying orders to harm. Chapter 6 extended to socio-political contexts, finding that temporal binding was not modulated by states or voting behaviours in the 2024 U.S. Presidential Election. Overall, temporal binding might be a dynamic index of implicit agency, possibly shaped by personal states/traits and social situations, supporting adaptive action, learning and engagement with the world.

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Author's Declarations

I declare that the work contained in this thesis has not been submitted for any other award and that it is all my own work. I also confirm that this work fully acknowledges opinions, ideas and contributions from the work of others.

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CHAPTER 1

Theoretical and methodological overviews of agency

Definitions of sense of agency and other relevant constructs

Sense of agency

The experience of controlling one's own actions and through such actions, events in the external world is known as the Sense of Agency (Haggard, 2017; Haggard et al., 2002). Making volitional actions mean we feel that we are the authors of such actions and that we do not feel as if they simply happen to us. Such experience is crucial in everyday life because it helps us distinguish our actions and their consequences from which of others or external events. As a result, this has great implication for society in general, especially the legal system, as it underscores attributing responsibility and authorship to one's own actions and thus refraining from acts that could lead to harmful, unethical or immoral consequences (Caspar, 2021; Moretto et al., 2011). However, the sense of agency is often assumed to be a single unique one thing whilst it indeed is comprised of several components or many different constructs might even be claimed to be capturing the same thing. In fact Grünbaum and Christensen (2020) proposed sense of agency to have at least four distinct constructs. The authors critiqued that researchers treat sense of agency as if it was one unified thing while different experiments all measure in slightly different ways. The first distinction they made was between ability and phenomenal character. Ability refers to sense of agency as a cognitive function i.e., the capacity to correctly identify who caused a movement or event (a 'who' system). This can be right or wrong and measured with accuracy/reaction time. Phenomenal character refers to sense of agency as a *felt quality* i.e., what it is like to be in control which is not binarily right or wrong but only stronger or weaker and is measured with subjective ratings or implicit proxies. The second distinction Grünbaum and Christensen (2020) made was between bodily i.e., sense of agency tied to the movement itself such that 'did I move my finger or did a machine move it?' and external i.e., sense of agency being tired to the consequences of the movement out in the world like 'did my button press cause this tone?". Therefore by crossing phenomenal character v. ability with bodily v. external, this leads to four agency-related psychological constructs (Table 1).

Table 1.

Four agency-related constructs adapted from Grunbaum & Christensen (2020)

	Bodily	**External**
Phenomenal character	Construct 1: feeling of bodily activity/control	Construct 3: feeling of controlling external events
Ability	Construct 2: ability to distinguish voluntary v. passive movement	Construct 4: ability to distinguish self-caused v. other-caused external events

Grünbaum and Christensen (2020) argued that the four constructs of sense of agency (SoA) are not merely different methodological angles on a single underlying phenomenon but are more plausibly subserved by distinct cognitive and neural mechanisms. This claim rests on both conceptual and mechanistic grounds. Mechanistically, they note that predicting the sensory consequences of one's own bodily movement such as anticipated proprioceptive feedback is likely computed by a relatively fast, modular and encapsulated process operating over a fixed set of sensorimotor associations. By contrast, predicting the external, often delayed and tool-mediated consequences of an action (e.g., an engine starting after a key is turned) necessarily draws on general world knowledge, learned associations and beliefs about intervening causal mechanisms. Because these two forms of prediction rely on qualitatively different types of input and knowledge, the authors contend that they are unlikely to be implemented by the same computational mechanism, even if both are governed by broadly similar predictive or Bayesian principles.

Conceptually, if sense of agency is defined as a cognitive ability i.e., the capacity to correctly attribute an action or its effects to oneself then this ability can seemingly be exercised without any accompanying conscious feeling of agency, as in cases of nonconscious or implicit action attribution. And conversely, a felt sense of agency can arguably arise without engaging this identificatory function. If, instead, sense of agency is defined purely as a phenomenal feeling (what Kühn et al. (2013) describe as a definite background feeling or buzz of being in control) then the construct risks becoming causally inert or epiphenomenal, which is problematic for experimental study since behavioural paradigms require that the target construct play some systematic causal role in producing measurable outputs.

This theoretical bifurcation thus directly dictates which experimental paradigm is appropriate for studying a given construct. Ability-based conceptions of sense of agency are properly investigated using self-other discrimination tasks that yield accuracy or reaction-time measures, since these can be scored against an objective ground truth. Phenomenal-character conceptions of sense of agency, by contrast, cannot be assessed for correctness in this way and instead require either explicit introspective ratings of felt control or implicit indices (assumed to track the phenomenal experience nonconsciously) most notably temporal binding, the perceived temporal compression between a voluntary action and its effect and sensory attenuation which is the reduced perceived intensity of self-generated sensory outcomes. We will later review each of these measures in depth. Grünbaum and Christensen use this mapping to argue that much of the existing sense of agency literature has conflated

these paradigms without acknowledging that they may be targeting fundamentally different psychological constructs.

It is worth noting that based on Grünbaum & Christensen (2020)'s conceptual distinctions, this thesis will be focussing primarily on the phenomenal sense of agency wherein we will be using the temporal binding effect as the implicit measure and introspective report of how control the participants feel over the outcome and that the paradigm used will be manipulating an external event. Therefore, phenomenal + external sense of agency (highlighted in orange of Table 1) is the main focus of the thesis.

Sense of agency v. consciousness

Consciousness is the general state of being aware and having subjective experiences (Koch, 2018). Kircher and Leube (2003) propose a distinction between prereflective consciousness i.e., our immediate lived experience and introspective consciousness which involves reflecting on and thinking about those experiences. According to their model, conscious experience is characterised by a sense of subjective ownership or 'myness' meaning that all experiences are intrinsically given from a first-person perspective. Sense of agency and consciousness are not the same thing. Pacherie (2008) using the delusions of alien control as the key evidence suggested that patients are conscious of what they are doing (awareness of action, their 'what' and 'how' awareness) while denying they are the author of it (no sense of agency). While consciousness and sense of agency should not be used interchangeably, we suggest that consciousness acts as the necessary baseline framework so that the levels of sense of agency identified by Synofzik et al.(2008), (2013) can be understood as playing out within the scaffolding of consciousness provided by Kircher and Leube (2003). The feeling of agency is a pre-reflective phenomenon so given immediately as part of the first-person 'myness' of experience, whereas the judgment of agency depends on introspective consciousness since it requires the agent to reflect on and conceptually evaluate that experience. This gives the levels of sense of agency reviewed later (Synofzik et al., 2013; Pacherie, 2008) a place within a broader account of consciousness.

Sense of agency vs sense of control

In Pacherie (2007)'s model, sense of control is one of the main components that make up sense of agency but it is not a separate construct sitting alongside it and the sense of control is defined as the feeling that the action is unfolding according to one's intentions and that one can guide, regulate and adjust it as it proceeds. Pacherie (2007) suggests that a sense of control is necessary for a robust sense of agency but it is not identical to agency itself. The

stronger the match between what one intended and what actually happens, the stronger their sense of control and consequently their sense of agency. However the sense of agency also depends on the sense of initiation (the feeling that the action began from one's own intention) and the experience of intentional causation (the feeling that the action produced an effect) so sense of control alone cannot explain the whole experience of being an agent. It is therefore worth noting that throughout the thesis and in broader literature (Sidarus, Vuorre, Metcalfe, et al., 2017 etc.), when 'control' is referred to in measures of judgments of agency by asking participants 'how much control did you feel', this is closer to Pacherie (2007)'s narrower sense of control than to the full tripartite sense of agency. Though it is also likely that sense of control is used interchangeably with sense of agency elsewhere in this thesis (and also in sense of agency research). We are cautious that sense of control does not fully capture the sense of agency experience as a whole, especially given the hierarchical and muti-level nature of the phenomenon (reviewed later) and also how there might be different constructs that are agency-related like Grünbaum & Christensen (2020) had suggested.

Sense of agency v. self-efficacy v. responsibility

Further constructs related to sense of agency need to be defined clearly. Firstly, self-efficacy refers to the belief about one's general capacity to bring about outcomes, formed and updated over time (Bandura et al., 1999) so therefore not a moment-to-moment experience during a specific action. Self-efficacy and sense of agency could indeed be correlated such that self-efficacy as a top-down influence could influence sense of agency over specific action and that sense of agency over specific action over time could contribute to the change in self-efficacy belief. Thus, self-efficacy is more of a stable trait-like belief and it is more closely related to long-term end and the concept of global metacognition of Seow et al., (2021) (see below). However, these two constructs are also not the same and are not referred to interchangeably in the thesis.

Secondly, sense of agency and responsibility are related but distinct constructs. Responsibility is a socially and culturally grounded concept which is based on the assumption that individuals choose and control their own actions and can therefore be held to account for the outcomes those actions produce; and that "responsibility may include both moral/emotional and purely rational/utilitarian aspects" (Moretto et al., 2011, p. 1848). Responsibility rating (i.e., how much the participants feel responsible for the actions or outcomes they have caused) has also been utilised, particularly in research assessing agency in social contexts that the actions are coerced and the outcomes are social, often taxing and

immoral and thus directed at others (Caspar et al., 2016, 2021, 2022; Salatino et al., 2025). Coercive behaviours to harm others are often found to be associated with lower responsibility rating i.e., not feeling personally responsible when being coerced despite such actions being self-generated (Caspar et al., 2016, 2021, 2022). On this view, sense of agency is a psychological precondition for attributions of responsibility rather than being identical to them because a person would be aware of their causal role in bringing about an outcome (one interpretation of sense of agency) and yet felt no sense of responsibility because for instance, they had no choice or say in the matter (Dewey, 2024). Thus, in this thesis particularly chapter 5 where feeling of responsibility over the moral action was measured, this was not meant to be reducible to the agents' own sense of agency.

Theories of sense of agency

The comparator model

There are three main models which could help explain the sense of agency. One of the foremost classical theories is called the comparator model, based on the computational model of motor control (S.-J. Blakemore et al., 2002; Frith et al., 2000; Wolpert, 1997). This account suggests that the intention to perform a goal-directed action enables a representation of a motor plan i.e., an inverse model which entails the information about the desired sensory states. Afterward, there are two copies of the selected motor plan from the inverse model, one of which is the signal which the motor cortex of the brain sends to the muscles so as to enact the goal-directed action. A copy of that signal is also sent through the brain. This second efferent copy, or the corollary discharge, is the copy of the neural signal sent to other parts of the brain to help inform them of an impending movement and thus aid predict the sensory consequences of such action i.e., the forward model. The predictions about sensory feedback given the estimated state by the forward model are then compared with the actual sensory outcome of the action. If the actual and predicted sensory feedback match together, this gives rise to the sense of agency. Whereas if there is a mismatch between the actual sensory consequences and the predicted estimate, such prediction error would result in a reduction or absence of the sense of agency. The subtraction of the prediction from early stages of sensory processing would thus result in the attenuated perceptual experience, as it has been observed that self-produced stimuli is perceived to be less intense compared with externally-produced ones. Such phenomenon is referred to as sensory attenuation, explaining why, for instance, we cannot tickle ourselves because before the actual sensory outcome takes place, our sensorimotor system already anticipates it. As a result, the sense of agency seems to be driven

significantly by the sensorimotor processes, considering how the perceived intensity of self-produced outcomes is attenuated at both neural and perceptual level, distinguishing outcomes that are the results of one's own actions in contrast to externally produced ones (S.-J. Blakemore et al., 2000; Press et al., 2023).

Besides sensory attenuation, the model posits that the prediction errors arising from the mismatch between predicted and actual sensorimotor outcomes would moderate the experience of the sense of agency. Indeed, many studies have found that predictions and predictions errors moderate the experience of sense of agency. Firstly, Moore and Haggard (2008) confirmed the role of predictive process by varying the probability with which a tone would occur as a result of the manual key press as the action. The authors found that when the predictability of the effect of action was high, participants experienced the sense of agency even in trials where the tone as the action effect did not occur. Secondly, deterioration in the mechanism that underlies motor representation for intending and recognising actions amongst people with schizophrenia impaired their ability to match internal about movement direction with their action outcome (Daprati et al., 1997). As a result, Daprati et al. (1997) found that sense of agency was disrupted in people with schizophrenia since they misattributed the observed effects to oneself as author even though it was not. Thirdly, when movements and outcome feedback are delayed longer than expected, this could also modulate agency. For instance, Shimada et al. (2009) showed that any temporal discrepancy larger than 0-300ms visual-tactile stimulation window would lead individuals to reject body agency and ownership over the rubber hand. However, a review on the effect of feedback delay on agency by Wen (2019) concluded that even though delay in action-effect is often used to attenuate the sense of agency, there are many nuances at different levels that perception of causal relations might be more flexible and remain intact despite action-effect delay, whereas sensorimotor control might have been more affected. Lastly, discordance between actual movements and the sensory feedback which created prediction errors has been shown to induce participants to misattribute movements to other agent when the spatial discordance was elevated i.e., high prediction errors (Farrer, Bouchereau, et al., 2008). To summarise, predictions and prediction errors shape and influence the sense of agency as one can experience agency over absent effect based on high predictability, or lose agency because of impaired internal motor representation, action-effect delay and movement-outcome discordance. This shows the importance of prediction and prediction errors in the formation of agency experience.

It is worth noting that the comparator model might not capture fully how the motor system could influence the sense of agency because some studies show that agency could already be moderated before any action plan has taken place. Chambon and Haggard (2012) showed that participants responding to targets that were compatible with a prior subliminal prime reported greater sense of control over the subsequent colour effect compared with when the preceding prime was not compatible; even though the two types of primes shared equivalent predictive statistical relationship with the colour effect. Sidarus et al. (2017) later showed that event-related potentials components like action-locked correct-related negativity (CRN) amplitude (associated with incompatible priming trials, reflecting conflict monitoring) was also associated with agency rating. In particular, the authors showed that unresolved conflict leads to greater action CRN and consequentially a reduction in agency, even if the outcome has yet taken place. As a result, these results pinpointed the additional prospective component to sense of agency. Such prospective component of agency is influenced by dysfluency and difficulty in action selection that often stem from the subliminal or supraliminal stimuli associated with a conflicting response, thus resulting in attenuated agency over the action outcomes (Sidarus & Haggard, 2016). Notably, this component involves the mechanisms underlying action generation processes that occur before any action and independent of outcome (Chambon et al., 2014). On the other hand, the retrospective sense of agency is heavily influenced by actual effects since it relies on the comparison between the predicted and actual sensorimotor outcomes of the action as depicted by the comparator model. Therefore, prospective agency is distinguishable from the retrospective aspect of agency and that the comparator model fails to capture the prospective aspect of the sense of control.

Theory of apparent mental causation

The apparent mental causation theory adopts an opposite approach to explain sense of agency that while the previous model emphasises the role of the motor system in sense of agency formulation, this model disregards it and places the focus on top-down inferences and thoughts. Wegner (2003) suggests that the experience of conscious will results from when we infer an apparent causal pathway from our thoughts to our actions. The actual causal path, i.e., it is the motor system that results in the action, are not present in our consciousness or that we do not have conscious access to this. Our thoughts also occur from unconscious mental events without us knowing why or how they occur. All these unconscious motor system and mental events might be link to each other directly or another mental or brain

processes. Our thoughts and intentions to act and the actual actions are the only two apparent components that we are conscious of. As a result, we experience this illusory conscious will bridging between though and action based on what is apparent, not what is real. Such inference occurs based on the three similar principles derived from causal perception and attribution research: 1) priority: thought appears in consciousness just prior to the action; 2) consistency: the said thought is consistent with the actual movements and 3) exclusivity: there are no other possible alternative explanations for the cause of the action. As a result, we experience conscious will and we attribute the action to ourselves, suggesting that the experience of agency is an illusory one.

Numerous studies have indeed supported the theory of apparent mental causation that for example, people mistakenly believe to be controlling the cursor on a computer screen themselves when it is actually the experimenter who is executing control (Wegner & Wheatley, 1999). Fourneret and Jeannerod (1998) showed how limited our conscious monitoring of motor performance is, as denoted by the theory of apparent causation. In this study, participants trace lines with their right hand hidden and view through a mirror creating a visual-kinaesthetic conflict and they grossly underestimate the deviation of their hand path, reporting it as nearly straight ahead despite motor and sensory mismatches. Other studies showed the importance of top-down influences contrasting the pure motor control mechanisms approaches. Desantis et al. (2011) found that when participants were led to believe that the tone was either generated by themselves or by someone else although in fact the tone was always triggered by the participants, they showed greater sense of agency when believing they caused the tone compared with when they believed that the experimenter caused it. This highlights the importance of high-level contextual information and causal beliefs on the sense of agency, independent of the sensorimotor processes.

Cue integration theory

Both of the theories highlighted above offer seemingly opposing accounts. While the comparator model puts the emphasis on the sensorimotor processes, the theory apparent mental causation denotes the key role of environmental, contextual and external information that are external to the motor system. It is indeed the case that many studies discussed above show evidence how the sense of agency cannot be explained purely by the sensorimotor system and its comparator; but the apparent mental causation approach also has its limitation in capturing the full essence of agency. The limit lies at if the experience of agency depends entirely on the contextual and environmental cues, our agency would thus be extremely a

vulnerable system that would be susceptible to any misleading top-down influences. Later on, researchers have indeed synthesised the two competing models. It has been suggested that the sense of agency can be influenced by both sensorimotor signals as well as the external cues (from the context in which action is performed) together with the agents' higher-level cognitive processes such as thoughts, prior beliefs and intentions.

Moore et al. (2009) developed the cue integration theory wherein the dual contribution of both internal motor signals and the situational external cues influence sense of agency. The sense of agency is conceptualised as posterior i.e., the belief and experience that 'I am the agent' in the Bayesian cue integration framework. The integration of various information from both predictive sensorimotor system and top-down contextual, social cues and prior beliefs determine the strength of agency. The degree to which these cues play in the formation of agency is relative and it is influenced by their level of availability and reliability, that is, the more reliable the source of information, whether it is based on sensorimotor comparator or the external cues, the more these cues dominate the experience of agency (Moore & Fletcher, 2012). Synofzik et al. (2010) showed prime evidence supporting how such Bayesian accumulating weight of each domain give rise to agency attribution that schizophrenia patients who hold impairments in the comparator mechanisms rely more strongly and therefore adapt to external agency cues. Altogether, the sensorimotor and contextual cues integrate to influence agency in an interactive (rather than stringently additive) manner.

Schematics of the hierarchies and different levels of agency

Several lines of work are centred on the idea that sense of agency is organised hierarchically. The first and most directly relevant to the vertical structure of a single action, comes from Pacherie (2008) who proposes a hierarchical model of action specification comprising three levels of intentions and control: distal intentions, which specify the action in cognitive terms (goals and means); proximal intentions, which specify it in actional-perceptual terms (the action schemas to be implemented and their expected perceptual consequences 'that is a representation that fits the specification inherited from the D-intention while anchoring it to the situation at hand.' p.184-185); and motor intentions, which specify it in sensorimotor terms. With the D-P-M architecture of action specification, Pacherie (2008) noted that the sense of agency should not be treated as a single signal but as a compound of three distinguishable experiences each tied to comparisons made at a different level of the D-P-M hierarchy. The first is the sense of intentional causation, the feeling that one's action

caused a given effect. Pacherie (2008) grounds this in work on temporal binding effect (reviewed later) where the perceived time of a voluntary action and its effect are drawn closer together in time. The second component is the sense of initiation, which is distinguished from intentional causation. This is described as the felt binding between one's awareness of intending to move and one's awareness that the movement has actually begun, tied to P-level (parietal) prediction rather than M-level (cerebellar) prediction. Pacherie (2008) suggests that patients with parietal damage who could report that they had moved but not when they had first intended to move as evidence that these two forms of awareness can be selectively disrupted. The third component is the sense of control, which Pacherie (2008) further analyses as a compound of motor control (at the M level), situational control (at the P level) and rational control (at the D level), each resting on a comparison between desired, predicted and actual states at that level. It is worth noting also that Pacherie (2008) separates the feeling of being in control from the feeling of having to exert control such that these can trade off against one another and that this distinction helps explain why effortless action sometimes heightens the sense of agency and sometimes diminishes it (Relevant to chapter 3 of this thesis). Together, this model offers an empirical grounding for treating sense of agency as a genuinely hierarchical phenomenon rather than a single undifferentiated feeling.

Synofzik et al., (2008) created a two-step account of sense of agency wherein there is a crucial line being drawn between the non-conceptual *feeling of agency* and the *judgement of agency.* In particular, the most basic levels are consisted of sensorimotor processes which give rise to the lower-level non-conceptual feeling of being an agent, referred to as the feeling of agency. This *feeling of agency* is generated based on a gradual and plastic sub-personal weighting process of various action-related perceptual as well as sensorimotor cues. Furthermore, the feeling of agency could be understood as the background buzz of control which we feel for voluntary actions without explicitly thinking about it (Moore, 2016). Such pre-conceptual core is then processed to next step by the higher-level conceptual judgment that is largely influenced by conceptual capacities and attitudinal factors such as beliefs and desires, allowing the attribution of the sense of agency. In other words, the meta-representational judgment of agency arises in situations whereby individuals explicitly attribute agency to the self, distinguishing action outcomes caused by oneself from those of others.

As a result, low-level sensorimotor processes, particularly the comparison between actual and predicted sensory outcome as denoted in the comparator model, are responsible for the feeling of agency formation. Higher-level cognitive processes, including our ad-hoc belief

states, knowledge and contextual cues play a role in determining self-attributed agency, which is the judgement of agency. These two components of agency contribute to the overall sense of agency at different degree depending on the context (and experimental design) and that they might be distinctive in some cases. Most of the time, people need not form any causal belief or judgment about self-agency (purposefully flicking on the light switch and the light comes up etc.) and take the feeling of agency at its face value. When the situations get more ambiguous, for instance, when you accidentally or unexpectedly push a button without knowing or meaning to and thus cause the light to come on, because you are the only person in the room, you will experience sense of agency. That is because there are no other possible explanations for the observed event, irrespective of the fact that you have had no awareness of the sensorimotor processes preceding the event (De Vignemont & Fourneret, 2004). The two distinguishable levels of agency also represents the line between agency and causality as stated by Moore (2016): low-level feeling of agency is an automatic process of registering agency v. non-agency via the sensorimotor contingencies tracking process, whereas the high-level judgment of agency employs more general causality attribution processes, inferring 'I caused this and I am the author of this'.

This was later reframed by Synofzik et al., (2013) within a framework of optimal cue integration (See Figure 1). In this model, the authors deemed agency as a greatly heterogenous phenomenon and thus must be explained in a framework with both bottom-up and top-down processes, in similar vein as Moore et al. (2009)'s cue integration theory. They theorise that the sense of agency as a Bayesian integration of multiple cues weighted by their reliability, their consistency with the action having caused the outcome, and prior beliefs about causation. This offers a formal mechanism for how lower-level sensorimotor signals and higher-level conceptual signals might be combined into a single unified experience. The model holds that the experience of agency emerges from an ongoing exchange between anticipatory i.e., predictive and after-the-fact i.e., postdictive processing. At the sensorimotor level, the predictive side draws on internally generated signals which are predictions about the sensory consequences of movement, the process of selecting an action, motor output and a copy of the motor command itself. In some circumstances these internal signals alone are enough to generate a feeling of agency. In others, the system instead weighs these predictions against external information, such as incoming sensory feedback, producing postdictive feeling of agency that is reconstructed after the fact rather than anticipated in advance. Such basic, pre-reflective feeling can then give rise to an explicit, cognitively mediated judgment of agency, at which point contextual knowledge, general world knowledge and prior beliefs

play a substantial role in shaping the outcome. The relationship is not one-directional, however: judgments, beliefs, and situational context can also feed back and recalibrate the underlying sensorimotor predictions. Finally, factors such as emotional reactions, expectations of reward or punishment and the perceived value of outcomes can shift how heavily internal versus external signals are weighted, at both the sensorimotor and the cognitive level (Figure 1).

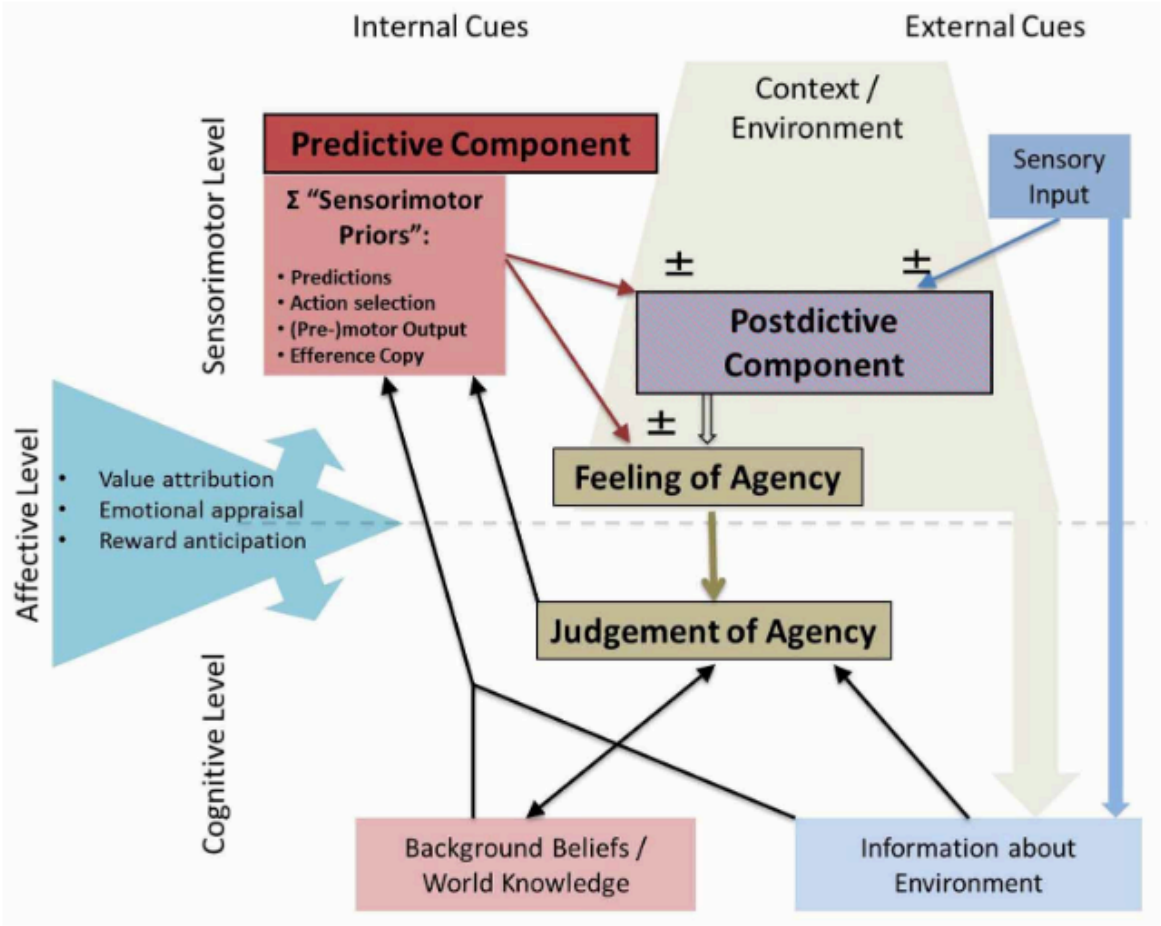


Figure 1. Synofzik et al. (2013)'s account of optimal cue integration underlying the experience of agency.

Sidarus et al. (2017) also propose an integrative and hierarchical model of the sense of agency using Pacherie (2008)'s terminology. In this model the sense of agency is built from at least three inputs at different levels of the hierarchy: a) the fluency or dysfluency of action selection itself (converting a goal into a specific action), b) the match between predicted and actual proximal outcomes (here, whether the visual cursor tracked the participant's mouse movements) and c) the match between predicted and actual distal outcomes (here, whether the target disappeared as expected). Their data show that these cues do not contribute equally. Discrepancies at the proximal outcome level had a substantially larger effect on judgements

of agency than fluency of action selection did. That said, action selection with either ease or difficulty was also shown to have some impact on the sense of agency. As a result Sidarus et al. (2017) proposed, consistently with the broader claim in Synofzik et al. (2013), that different classes of signal (internal and metacognitive signals related to action selection, as well as bodily movement and external outcome signals) are integrated according to their reliability rather than combined in a fixed or equal manner.

A second and conceptually distinct hierarchy in sense of agency appears in work on social and joint action by Schwarz et al. (2023) who introduced a paradigm for investigating the sense of agency of supervisors v. subordinates in hierarchical joint-action settings. The main finding across all experiments was that hierarchical role strongly shaped the sense of agency judgements. Participants who were assigned the role of supervisors consistently reported feeling more like having just caused the outcomes and feeling more responsible for such outcomes than their counterpart who played the role as subordinates, even when the actual opportunities for control were nearly equal between roles. Supervisors also tended to attribute outcomes slightly more to themselves than to the pair as a whole whereas subordinates were more likely to view outcomes as the product of joint action. Thus, people seem to internalise their social position in a hierarchy with leadership roles fostering a stronger subjective experience of sense of agency and responsibility, even beyond objective differences in action opportunities. This shows that the sense of agency is also hierarchical at the interpersonal level that it is important to also look at the sense of agency as a fundamentally social phenomenon. More broadly, Schwarz et al. (2023) provides a valuable framework for studying how people perceive sense of agency, responsibility and decision-making in hierarchical structures such as workplaces or educational settings. A later study by Schwarz and colleagues (2026) also extended sense of agency research into applied domain such that the authors showed how increased sense of agency is associated with pro-environmental behaviour. This therefore means that the personal sense of agency could be mapped onto broader societal level which has societal implications to foster environmental stewardship as well as halt the detrimental environmental decline.

Finally, a third line of work bridges these action-level hierarchies to higher-level metacognition, beliefs and mental health. Metacognition is known as our ability to reflect and evaluate our own thoughts and actions (Flavell, 1979) and research has shown that impairment in metacognitive abilities could be linked with poor behavioural outcomes as well as poor mental health. Stephan et al. (2016) built a hierarchical Bayesian framework in which interoception, homeostatic and allostatic control and a metacognitive layer that monitors the

performance of that regulatory circuitry are stacked on top of one another. When there is a mismatch between expected and actual bodily states, a prediction error is generated. The authors defined chronic dyshomeostasis as a persistent state in which these prediction errors remain elevated because the body repeatedly fails to match the brain's expectations. Such repeated experience of failing to regain physiological control gradually undermines the brain's sense of allostatic self-efficacy i.e., the higher-order monitoring system such that the brain does not merely monitor bodily states but also monitors its own effectiveness at regulating them. Within this framework, fatigue is interpreted as an early metacognitive signal that homeostatic regulation is failing, in other words when the brain repeatedly attempts to reduce bodily prediction errors but experiences little success, it infers that its regulatory actions are ineffective. The subjective experience of fatigue therefore reflects a perceived lack of control over bodily functioning. Initially, such loss of confidence relates only to bodily regulation but over time it could generalise to other aspects of cognition and behaviour. Drawing on theories of learned helplessness, the authors suggest that individuals may begin to develop a broader belief that they lack control over important outcomes in general. This generalised perception of helplessness can then manifest as depression. Depression is therefore conceptualised as a higher-order consequence of chronic fatigue-related failures in self-efficacy, rather than simply a parallel symptom. Stephan et al. (2016) therefore offers a direct model of how a failure at a low, regulatory level can propagate upward into a global belief with clinical significance.

In a complementary vein Seow et al. (2021) reviewed metacognition as operating across hierarchical levels of abstraction, ranging from local confidence in isolated decisions to global metacognition self-beliefs about one's abilities and skills. Unlike local confidence which concerns a specific decision, global metacognition reflects information accumulated across many experiences over longer periods of time. These broader self-beliefs play a critical role in shaping motivation, resilience, decision-making, educational achievement and mental health. Individuals with low global self-beliefs often feel less capable of influencing outcomes and may recover less effectively from setbacks. Consequently, global metacognition overlaps conceptually with constructs such as self-esteem and self-efficacy, both of which are strongly linked to depression and anxiety. Critically, Seow et al. (2021) emphasised that these levels interact reciprocally such that local confidence contributes to the formation of global self-beliefs, while global self-beliefs also feed back down to shape local confidence. They link this bidirectional structure to transdiagnostic dimensions of psychiatric symptomatology.

Taken together, here we propose how these three strands could suggest a way of organising the schematic to understand the sense of agency and how sensorimotor, personal, interpersonal and socio-political factors could moderate the sense of agency. Pacherie (2008)'s motor, proximal and distal levels of intention together with Synofzik et al. (2013)'s account of optimal cue integration with judgment of agency and feeling of agency distinction form the core sense of agency hierarchy. Whilst Pacherie (2008) differentiates the sense of agency hierarchy by processing stage, Synofzik et al (2013) differentiate it by phenomenal conceptual v. nonconceptual format and this latter distinction is helpful for understanding explicit v. implicit agency (temporal binding). Metacognitive and global-belief layers (Seow et al., 2021; Stephan et al., 2016) are placed above this core hierarchy, culminating in mental health outcomes such as fatigue, depression and anxiety that beliefs at the higher levels shape processing at the lower levels rather than the influence running in one direction only. Note that this is also in line with Synofzik et al. (2013)'s account of optimal cue integration wherein cognitive level such as background beliefs and world knowledge could also feed into the sensorimotor level that gives rise to the feeling of agency as well as the judgment of agency (See Figure 1). Schwarz et al. (2023)'s social-hierarchy and Schwarz et al. (2026)'s pro-environmental-related findings can be represented as modulating factors acting primarily on the distal/goal level, since role (leader v. follower) appears to affect attribution and judgment more than motor-level cues and how greater perceived control is linked with greater pro-environmental decisions, respectively.

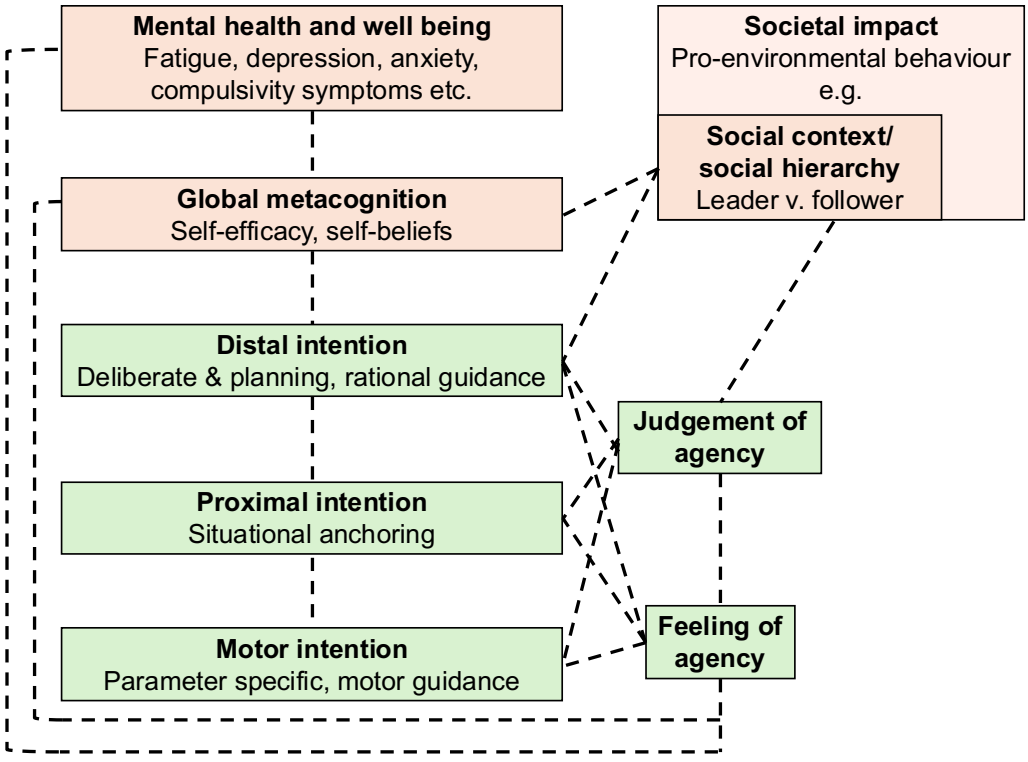


Figure 2. Graph illustrating a potential schematic proposed to help understand the sense of agency. Boxes in orange highlight the high-order factors such as higher beliefs, meta cognition and social factors that could exert top-down influences on what are placed in green boxes, which highlight the hierarchy of the core sense of agency over specific action itself. It is worth noting that the lines are in dashes which implies that these boxes/levels are still distinctive and can dissociate and that they might influence one another but this by no means implies any definite relationship between the suggested levels.

This schematic (Figure 2) helps build the theoretical reasoning of this thesis. Specifically, it motivates our focus on higher-level influences on the sense of agency, including beliefs and social factors and examines how individual differences and experimentally induced changes in these factors might influence the subsequent sense of agency over otherwise unrelated action-effect associations. Such approach is grounded in the reviewed theoretical accounts proposing that higher-level cognitive and social processes exert top-down influences on lower-level sensorimotor processing involved in the experience of agency rather than sense of agency arising solely from bottom-up motoric signals or factors that are specific to that context in which the action and outcome take place (K. A. Schwarz et al., 2023; Seow et al., 2021; Stephan et al., 2016; Synofzik et al., 2013). By investigating these higher-level influences, this thesis aims to extend current understanding of how broader cognitive and social contexts contribute to the construction and transformation of the sense of agency.

Measures of agency

We will review below the measures of agency and how each method of measuring agency would encapsulate the aforementioned four different constructs of agency proposed by Grünbaum & Christensen (2020). Table 2 shows what measures are appropriate for each specific construct. As having mentioned, the thesis is particularly focussing on the phenomenal sense of agency so the relevant implicit measures and explicit measures that are proposed to capture the phenomenal sense of agency will be reviewed.

Table 2.

Measures of sense of agency constructs adapted from Grunbaum and Christensen (2020).

Construct	Measure
Bodily + ability	manipulate the movement itself (e.g., voluntary vs. passive finger movements via mechanical devices, functional electrical stimulation,

Construct	Measure
	TMS-induced twitches) and measure with an objective accuracy score: e.g., can the participant correctly tell whether they or a device produced the movement.
External + ability	manipulate an external event (e.g., distorted visual feedback of a drawing movement, action-effect associations) and measure accuracy: e.g., correctly judging deviations or correctly identifying whether they caused an effect.
Bodily + phenomenal character	use introspective reports or implicit measures tied to the movement itself: e.g., cortical/TMS stimulation eliciting felt urges to move or sensory attenuation of self-produced tactile sensations
External + phenomenal character	use introspective scales tied to external effects: e.g., subjective how much control did you feel ratings or implicit measures, putatively temporal binding effect (temporal attraction between action and effect) + sensory attenuation (judgment about stimulus intensity + suppression of EEG signals)

Explicit measure of agency

The first type of explicit measure that is widely used targets the external + ability sense of agency suggested by Grünbaum & Christensen (2020). A number of studies have employed the action recognition judgments while concealing the action and only presenting (delayed) onscreen feedback. Participants could be asked to evaluate authorship i.e., whether the participants themselves have caused the observed outcome or someone else/the computer (Bart et al., 2019; Farrer, Frey, et al., 2008; Timm et al., 2016). These studies found that when the visual feedback is congruent to the participants' actual movement, this leads to faster judgment that the movement was self-made. Research also used virtual hand to delay either the movement of the virtual hand (delay of movement execution) or delay the expected auditory outcome (delay of outcome-related feedback) and examine if such delays influence authorship judgement (me/not me) (David et al., 2007, 2016). Authorship attribution is found to be more sensitive to delays of outcome-related feedback than delays of movement executions (N. David et al., 2007, 2016). Cultural contexts also matter in how individuals attribute authorship of outcome to self-generated actions (Bart et al., 2019). This thesis will not include this type of explicit measure.

Second type of explicit measure involves researchers asked participants to provide ratings to judge how much they felt like being in control over the actions and/or the consequential outcomes, often on an analogue scale (no control to full control) (Beyer et al., 2017, 2018; Ciaunica et al., 2024; Galang et al., 2025; Reis et al., 2022; K. A. Schwarz et al., 2018; Sidarus & Haggard, 2016). This explicit measure targets the external + phenomenal character sense of agency outlined by Grünbaum & Christensen (2020). Higher explicit judgment of feeling in control is often found when participants are working alone rather than working with a joint cursor (Dewey & Knoblich, 2014) or participants feel more control when they have initiated a joint action than when they have followed it (Grynszpan et al., 2019). The current thesis will be using this type of explicit measure to target the external + phenomenal character frequently throughout chapters.

The potential problems with explicit measures are the self-serving bias which participants might be prone to that individuals have the tendency to attribute favourable outcomes to themselves and other demand effects (Moore, 2016; Saito et al., 2015) or social desirability biases (Zhao et al., 2025). That being said, explicit measures of agency, from action recognition, authorship attribution to control/responsibility ratings offer rich and intuitive insights into how people experience agency as they require participants to reflect on this feeling. Thus, explicit measures often tap into the judgment of agency element as proposed by Synofzik et al. (2008). It should however be noted that this discussion is not intended to suggest that implicit measures especially temporal binding are not influenced by valenced outcome. Indeed temporal binding effect has been shown to vary as a function of outcome valence and moral context too (e.g., Caspar et al., 2016; Yoshie & Haggard, 2013), albeit in a more nuanced manner which will be reviewed more in-depth in chapter 2.

Further issues with explicit measures go above demand characteristics and response biases as the external + phenomenal sense of agency, as previously mentioned, is a complex and heterogenous concept and that it is phenomenally thin (Haggard, 2005). In other words, our everyday actions and their consequences are not typically and rarely reflected on and being inferred to as the results of our volition. This is particularly relevant for outcomes that are neutral such that people rarely ever reflect on how much control they feel over the act of turning on the light, for instance. Thus it might be challenging for the participants themselves to reflect on this explicit judgment of agency, not because of the self-serving bias per se but because reflection might be genuinely difficult as it is not something being done so often. Implicit measures of agency could help in this sense, without asking participants to directly refer to their experience. We will now review two widely used implicit measures of agency.

Implicit measures of agency

Sensory attenuation

The first implicit measure is the sensory attenuation paradigm. Sensory attenuation refers to the robust phenomenon whereby the sensory consequences of one's own voluntary actions are perceived as less intense than physically identical stimuli generated externally (S.-J. Blakemore et al., 1999, 2000). It is typically explained within the framework of the internal forward model: when the motor cortex issues a motor command, an efference copy (also termed corollary discharge) is generated in parallel and used to predict the sensory consequences of the movement (Stenner et al., 2015). This predicted signal is compared against the actual sensory feedback and a close match between prediction and outcome results in the dampening or attenuation of the perceived sensory signal (Blakemore et al., 1998; 1999). In this sense, sensory attenuation is thought to index a low-level sensorimotor form of prediction rather than a higher-order reflective judgement about agency.

Grünbaum and Christensen (2020) were explicit that whether a sensory-attenuation measure counts as *bodily* or *external* depends on what is being attenuated. They suggest that attenuation of a *tactile* sensation from one's own touch (Blakemore et al., 1999) was bodily (See Table 1 and 2) because it pertains to the attenuation of feedback tied to the movement itself. Meanwhile, attenuation of an auditory/visual etc. consequence (Gentsch & Schütz-Bosbach, 2011 etc. see ERPs components reviewed later) is external because the latter is attenuation of a distal, mediated event. Thus, the research reviewed below vastly measures the external sense of agency.

Methodologically, sensory attenuation has been demonstrated across behavioural and neural levels. Behaviourally, it is most commonly measured as a reduction in perceived stimulus intensity. Participants are often asked to rate the intensity or volume or brightness of the self-initiated self-generated stimuli (touch, force, sound or visual effects) (Kiepe et al., 2021; Lubinus et al., 2022; Weiss et al., 2011) and they often report that self-generated stimuli are weaker than externally generated ones of identical physical magnitude. Delay detection tasks are also useful to measure sensory attenuation wherein participants can also be asked to judge whether there has been a delay between self-generated v. externally generated movement and its visual feedback (Arikan et al., 2019; Pazen et al., 2020). For feedback that displays continuous own hand movements, participants often experience a reduced sensitivity to detecting delays between a self-initiated action and its sensory outcome compared with when the feedback displays someone else's hand (Uhlmann et al., 2020). This

indicates that the perceptual suppression impairs the comparison between continuous actions and feedback (Uhlmann et al., 2020).

At the neural level, attenuation is typically indexed by a suppression of early sensory-evoked responses: EEG/MEG studies show a reduced amplitude of the N1/M100 component for self-relative to externally-generated sounds (Baess et al., 2011; Horváth et al., 2012). fMRI studies report attenuated BOLD responses in sensory cortices during self-generated relative to externally-generated stimulation (Straube et al., 2017). Such convergence of behavioural and neural evidence and the fact that attenuation has been demonstrated across visual (Hughes & Waszak, 2011), auditory (Weiss et al., 2011) and painful heat (Beck et al., 2017; Borhani et al., 2017) domains, is generally taken as strong support for its use as an implicit marker of the sense of agency. For instance, Beck et al. (2017) found that predictable self-administered high-intensity heat-pain stimuli were perceived as less intense than unpredictable ones and Borhani et al. (2017) found that freely self-generated painful stimuli were rated as less intense than those administered under experimenter compulsion.

However, whether sensory attenuation taps the same underlying construct as other implicit measures such as temporal binding effect (reviewed below) remains contested. Dewey & Knoblich (2014) directly compared these measures within the same participants having them provide explicit agency ratings alongside tasks assessing both temporal binding (via a temporal reproduction procedure; reviewed later) and sensory attenuation (via a loudness-judgement task in which participants compared self- v. externally-produced tones to a reference tone). They found no meaningful correlations between any of the three measures. This dissociation suggests that sensory attenuation and temporal binding, despite both being classified as implicit indices of agency might reflect at least partially distinct underlying processes with explicit judgements of agency drawing on yet another more inferential source of information. The lack of correlation between sensory attenuation and temporal binding might be due to how Grünbaum and Christensen (2020) note that sensory attenuation itself is not a single construct because as having mentioned some paradigms manipulate the movement itself (a bodily measure) while others manipulate action-effects like tones (an external measure), whereas temporal binding is consistently external. Sensory attenuation may also reflect general event predictability rather than self-generation specifically (Kaiser & Schütz-Bosbach, 2018). Together, this suggests the two putatively implicit measures i.e., sensory attenuation and temporal binding effect may not be tracking a single common attribute.

Temporal binding

Libet Clock.

The more popularly employed implicit measure, which would also be used intensively in this thesis, is the temporal binding paradigm. This temporal binding effect (or intentional binding) refers to the robust phenomenon whereby the time interval between (often) a voluntary self-generated action and its outcome is subjectively compressed (further discussion on the 'voluntary', 'self-generated' aspects will be elaborated as they might not be crucial for binding effect to appear). According to Grünbaum and Christensen (2020) temporal binding effect is also indexing the external + phenomenal character sense of agency. In the seminal study by Haggard et al. (2002) which adopted the Libet clock task, participants were asked to watch a clock face with a rotating clock hand and reported at which position on the clock face was the clock hand when a certain event happened, either a button press or a sound. The three main conditions were a) a voluntary condition wherein participants pressed a button; b) an involuntary condition wherein participants' movements to press the button were caused by the transcranial magnetic stimulation (TMS) and c) control condition whereby participants did nothing and only heard the sham sound of the TMS, and these were the first events. All of these first events were then followed by a second event which was a tone after 250ms. The authors found that when participants were in the voluntary condition where they pressed the button voluntarily, the action was perceived to take place later in time whilst the tone triggered by the button press was perceived to occur sooner, creating this compression of time between action and its outcome. Importantly, such temporal compression was not observed during the TMS sham involuntary condition, even though the action was identical and so was the outcome. This leads to the conclusion that voluntary action and its outcome are bound together in time by our neural-cognitive system. Such perceptual shift has been conceptualised to enable individuals to experience the sense of their own agency and thus researchers have used the perceived time between intentional actions and their sensory outcomes as the means by which the sense of agency over voluntary actions can be studied.

Action binding and outcome binding.

Using the Libet Clock paradigm to measure the temporal binding effect allows researchers to distinguish action binding from effect binding. As previously mentioned, the temporal binding effect involves the voluntary action being perceived to take place later in time (i.e., action binding) and the effect being perceived to occur sooner (i.e., effect binding), thus action and effect are bound towards each other in time and the interval between these two events are compressed. Researchers have found that these elements could reflect

different processes and thus unrelated. A wide range of studies have found no correlation between the action and outcome shifts (Desantis et al., 2011; Siebertz & Jansen, 2022; Tonn et al., 2021). This confirms the meta-analysis of 78 studies results by Tanaka et al. (2019) who showed that action binding is dependent substantially on how much control individuals have in the action execution and planning processes, regardless of the actual sensory outcome. In other words, the action binding phenomenon derives from the activation of learned action-outcome representations via action planning and execution. For instance, in the study by Moore and Haggard (2008), participants showed action binding even in the absence of any action outcomes when the probability of outcome occurring was high. This result shows that action binding is not so modulated by the postdictive mechanisms, and it is rather dependent on the execution of actions and learned action-outcome associations. Indeed, previous research have shown that action binding decreased when participants executed actions in the states of fear, anger and sexually aroused compared with neutral states; even though outcomes of such actions are always the same (Christensen et al., 2019; Render & Jansen, 2021). Being in a stressful, negative or highly arousing state could impair the smooth attentional and perceptual processes of motor planning and execution and thus attenuate the action shift.

On the other hand, outcome or effect binding may rely more substantially on the degree to which individuals could compare the predicted onset of the effect and its actual onset. This means outcome binding is modulated to a greater extent by postdictive processes. Tonn et al. (2021) denoted that a general association between action and effect is sufficient to give rise to outcome binding without needing any specific action-effect mapping. For instance, Borhani et al. (2017) found that outcome binding occurred even in the absence of agency, when participants neither chose the outcome nor initiated the action, suggesting that causal inference as the postdictive process alone can produce a binding effect. This was demonstrated through a significant outcome binding effect in the passive-movement or instructed-choice condition, while action binding in this condition was not significant. Likewise, Desantis et al. (2011) primed participants to believe that the outcome was caused by either themselves or the confederate (but it was always caused by the participants themselves). The authors found stronger outcome binding when participants believed the outcome was self-caused even though action execution was identical across conditions. Thus, the action execution here did not play a role in forming outcome or effect binding but rather the postdictive and general mapping between the two events.

As a result, it has been advised that action and tone binding should be studied separately for studying consciousness of action, agency and volition as this provides more insight about the underlying mechanisms regulating each perceptual shift (Wolpe et al., 2013). Furthermore, this could contribute to the understanding of abnormal experience of agency (Tanaka et al., 2019). For instance, patients with Parkinson's disease have been found to have a specific lack of action binding as they experience unreliability and uncertainties in motor execution as well as deficits in sensorimotor integration (Moore et al., 2010; Saito et al., 2017). In contrast, outcome binding is reserved in patients with Parkinson's, supporting the two action and effect binding might be dissociative.

Interval estimation/interval reproduction paradigm.

Despite the Libet clock's clear advantage of separating the action binding and outcome binding, allowing researchers to understand each component separately, many researchers have raised several potential problems. The Libet Clock paradigm has been said to exert high cognitive and attentional demand while participants constantly monitoring the clock; as well as the possibility that the participants might execute actions based on certain clock hand's positions instead of in a truly self-generated and volitional manner (Engbert et al., 2007). Furthermore, the clock procedure measures temporal binding indirectly as it captures the timing of events relative to the baseline rather than the actual veridical time interval between two events (Humphreys & Buehner, 2009). As a result, researchers have developed an alternative means of measuring temporal binding which is known as the interval estimation method. In this paradigm, in the operant condition, participants are often asked to perform an action that will trigger an outcome whilst in the baseline condition, participants observe two events occur independently without executing any actions. In both experimental conditions, participants have to provide an estimate using an on-screen scale, often in milliseconds, of how much time they perceive to have elapsed between the two events. The perceived time interval between the voluntary action and its outcome in the operant condition is robustly shorter i.e., more compressed (approximately 20% of the actual interval) than the perceived time elapsed between two independent events. Such compression of time interval between action and its effect observed is the temporal binding phenomenon. Later study by Humphreys and Buehner (2010) employed similar conditions but instead of asking participants to provide a numeric estimation, participants hold down the space bar to reproduce the intervals, without reporting any number explicitly. This interval reproduction paradigm helps eliminating the response bias because it does not involve a suggestion of any range of time.

Contrasting Libet Clock and the interval estimation/reproduction paradigms, it has been found that whilst the temporal binding measured using the clock procedure often decrease at sub-second intervals (460 and 650ms), binding effect is robust for super-second intervals up to four seconds when measured using the interval estimation/reproduction paradigms (Haggard et al., 2002; Humphreys & Buehner, 2009, 2010). On the other hand, Tanaka et al. (2019)'s systematic review results revealed a larger effect size and greater sensitivity perceptual moderators in the temporal binding effect amongst studies using Libet Clock compared with those with the interval estimation/reproduction. Therefore, considering both methods' merits and downsides, the experiments in this thesis employed a mixture of both interval reproduction/estimation and Libet clock methods where appropriate to best capture the temporal binding effect and the mechanisms driving it.

Dissociation of these temporal binding measures

Psychometric properties for these temporal binding measures have been explored very little despite them being widely used methods for putatively measuring temporal binding effect. One study did use both interval estimation and Libet Clock in their within-subject design study to examine the effect of violations of the learned action-outcome associations on temporal binding (Haering & Kiesel, 2014). Experiment 4 of their results showed that a mismatch between learned and presented action outcome did not influence temporal binding magnitude with both interval estimation and Libet clock paradigms. However, this is not sufficient evidence suggesting that the two methods do not diverge or validly measure the same construct. Siebertz & Jansen (2022) is one of very limited studies that examine the validity of whether these two interval estimation and Libet clock paradigms actually measure the same underlying process. The authors found that temporal binding scores from the two paradigms did not correlate such that individuals who showed strong temporal binding magnitude in the Libet clock task were not more likely to show strong temporal binding in interval estimation. In addition whilst Libet clock paradigm generally showed good to excellent reliability, the interval estimation demonstrated only moderate reliability and was more susceptible to measurement error. De Azevedo et al. (2025) later extended on these findings and found that for Libet clock, interval estimation and interval reproduction paradigms they all have moderate to good reliability within a single testing session but their test-retest reliability across different sessions was much lower. In addition, regarding whether different tasks measure the same construct, only robust correlation observed was between the interval estimation and interval reproduction tasks while temporal binding measured using

the Libet clock task did not correlate reliably with the other tasks. This is likely due to how estimation and reproduction both require judgments of interval duration and therefore mighty rely on shared cognitive processes whereas the Libet clock task focuses on judgments about the timing of discrete events. These findings suggest that temporal binding is not a single, uniform process and that researchers should be cautious when comparing results across paradigms or using temporal binding measures to study individual differences. Any claims made in the thesis about the unitary temporal binding construct are fully acknowledged as provisional and should be interpreted with caution.

Dissociation of explicit and implicit agency

The two measures of the sense of agency have been proposed to reflect separate processes because a wide range of studies have found that the two measures often dissociate (Dewey, 2024; Dewey & Knoblich, 2014; Saito et al., 2015; K. A. Schwarz et al., 2019; Vantrepotte et al., 2022). Indeed, the implicit sense of agency measured via the temporal binding effect or sensory attenuation has been denoted to underlie the background feeling of control which people often not consciously reflect on, thus capture the feeling of agency element, as proposed by the two-step account by Synofzik et al. (2008). Complimentarily, the explicit agency often concerns participants retrospectively infer their sense of agency and reflect on the feeling of being in control or authorship attribution, which therefore reflects the judgment of agency element (Synofzik et al., 2008). We argue that these two measured do not work in opposition of each other but instead in parallel and reflecting different processes, and thus it is important to include both measures, if possible, when investigating the sense of agency phenomenon.

Neural correlates of agency

Brain regions associated with agency

Besides the above behavioural methods, neuroimaging studies have also provided valuable insights into the understanding of agency, particularly what neural mechanisms drive the formation and transformation of such phenomenon. Several brain circuits have been identified to underpin the action planning, execution and action and/or outcome monitoring processes that give rise to the sense of agency. Here, we attempt to review (not exhaustively) neural evidence underlying the different sense of agency constructs as defined by Grünbaum & Christensen (2020). We attempt to categorise the neural evidence that focusses on the external + ability sense of agency (ability to distinguish self-caused v. other-caused external

events tested via manipulating an external event e.g., distorted visual feedback of a drawing movement). Then we will review the research showing neural evidence underlying both prospective and retrospective components of the external + phenomenal character sense of agency (feeling of controlling external events).

External + ability sense of agency

Regarding the ability + external sense of agency, earlier study by Farrer et al. (2008) using fMRI showed the importance of right inferior parietal lobule (IPL), specifically right angular gyrus (AG) in monitoring the discrepancies between intended actions and the movement consequences, which then gives rise to action authorship. In similar vein, high-frequency (10 Hz and 20 Hz) repetitive transcranial magnetic stimulation (rTMS) applied to the right IPL was found to impair the detection of non-self-agency such that it impaired participants' ability to detect when an action outcome was not their own hence providing causal evidence for the right IPL's role in this sensorimotor-comparator process (Bečev et al., 2025). As a result, inferior parietal cortex (IPC) or AG seem to be involved in also the ability + external sense of agency. Additionally, the Extrastriate Body Area (EBA) which is traditionally considered a category-selective region for the visual processing of static human bodies has also been implicated to contribute to the ability to distinguish self-caused v. other-caused external event. David et al. (2007) found that EBA activity *increased* when visual feedback was incongruent with participants' self-generated movements. On this basis, the authors proposed that the EBA might underlie the sensorimotor mechanism distinguishing one's own action outcome from another's.

Prospective and retrospective components of external + phenomenal character sense of agency

Firstly, considering the frontal cortex, the following regions appear to play a crucial role in the external + phenomenal character sense of agency construct as defined by Grünbaum & Christensen (2020). A meta-analysis by Khalighinejad et al. (2016) supported the role of the dorsolateral prefrontal cortex (DLPFC) in endogenous (internally generated) actions when participants selected between multiple actions, as anodal stimulation of DLPFC increased the temporal binding involving endogenous action selection. In contrast when there was no genuine choice (a fixed action or an externally instructed action), DLPFC stimulation had no effect. Thus such findings support the prospective component of the sense of agency whereby processes occurring before action execution might already shape the experience of sense of agency and that the DLPFC could play a key role in this aspect.

The Supplementary Motor Area (SMA) and pre-SMA in the superior frontal gyrus of the frontal lobe have also been found to be associated with voluntary action and internal generation of movement. Kühn et al. (2013) using functional magnetic resonance imaging (fMRI) showed that greater activation of a cluster in the left SMA proper (extending into the dorsal premotor cortex) was associated with a stronger temporal binding effect, i.e., a greater compression of the perceived voluntary action-outcome interval. This means that the SMA might contribute to the subjective experience of the temporal flow that accompanies goal-directed behaviours, although as a correlational finding this cannot establish that the SMA plays a causal role (Kühn et al., 2013b). Complimenting this Zapparoli et al. (2020) provided causal evidence for the pre-SMA's involvement in the sense of agency, specifically its prospective component i.e., sense of agency as computed at the point of action selection, prior to the outcome, using the same temporal binding measure. When rTMS was delivered over the pre-SMA time-locked to action planning (but not when it was time-locked to the actual physical outcome of the action), participants' predictions about the temporal interval from action to consequence became significantly less precise. Because the disruption only occurred when stimulation targeted the action-planning stage and not the outcome stage, this dissociation indicates that the pre-SMA's contribution to temporal binding is specifically prospective, reflecting a role in action selection rather than in outcome monitoring. Thus, the DLPFC and pre-SMA appear to contribute to the prospective component of the external + phenomenal sense of agency experience.

Second, parietal regions of the brain have also been shown to be associated with the sense of agency, though the evidence here spans different processing stages (prospective i.e., action selection vs. retrospective i.e., outcome monitoring) and different methods (correlational fMRI v. causal TMS/rTMS). Chambon et al. (2015) found the role of the IPC in the prospective sense of agency that applying single-pulse TMS over the left IPC reduced participants' rating of control at the point of action selection, not at the time of action outcomes indicating that the left IPC causally contributes to sense of agency computed before rather than after the outcome occurs. In contrast, there is evidence of the right IPC or AG to be active in the retrospective construction of agency i.e., agency reconstructed after an outcome has occurred, via detection of a real-time mismatch between predicted and actual sensorimotor outcomes (Haggard & Chambon, 2012). Thus, the IPC appears to contribute to both prospective and retrospective components of the external + phenomenal sense of agency experience.

Moving to the right Temporo-Parietal Junction (rTPJ), adjacent to AG, also appears to play a role in the sense of agency, particularly in outcome-stage action monitoring rather than action selection itself. Using fMRI, Miele et al. (2011) found that rTPJ activity increased when participants felt out of control i.e., when they detected a discrepancy between an observed and an expected outcome suggesting that heightened rTPJ activity tracks the detection of a loss of control once an outcome has been registered, again a correlational (fMRI) finding. Other fMRI studies report a similar pattern, with increased TPJ activity associated with a reduced sense of agency measured with explicit judgment of control (Zito, Anderegg, et al., 2020; Zito, Wiest, et al., 2020), hence reinforcing the rTPJ's proposed role in retrospective, outcome-related monitoring.

Further integral neural networks also underlie the sense of agency are proposed by Seghezzi et al. (2019)'s meta-analysis including a) the left IPL and left EBA as a body-ownership-specific network, b) the left posterior insula, the right postcentral gyrus as well as the right superior temporal lobe as the sense-of-agency-specific network and finally c) the left middle insula as a shared network between body ownership and sense of agency.

Event-related potential (ERP) components

We have previously discussed sensory attenuation, which could be an index measuring the external + phenomenal character sense of agency because self-generated stimuli are often perceived to be less intense than externally-produced ones. A vast range of studies have showed that action-related visual/auditory N1 waveform amplitude is attenuated compared with those locked to externally-generated visual effects or sounds, because such sensory consequences are predictable and their representations are already activated prior to any movement execution (Gentsch & Schütz-Bosbach, 2011; Houde et al., 2002; Poonian et al., 2015). As a result, N1 suppression could be seen as a neural marker of sensory attenuation and thus by extension the external + phenomenal character sense of agency (Grünbaum & Christensen, 2020).

Caspar et al. (2016) used the auditory N1 to ask a different question not whether an outcome is self- or externally-caused but how subjective control over an already self-caused action affects its neural processing. In their design participants always pressed the key themselves so an efference copy was generated in every condition and nonetheless, N1 amplitude was smaller under coercion than under free choice. Here, they did not utilise N1 amplitude to measure sensory attenuation (because again, in both conditions the participants' actions were all self-generated) but instead N1 amplitude was used to assess outcome

processing. Therefore, N1 amplitude is not a single unidirectional marker of sense of agency such that its direction of modulation depends on what is being contrasted, such as a) self- v. externally-caused stimuli (where self-generation produces attenuation) and b) voluntary v. coerced self-generated action (where attenuation instead tracks reduced felt control over an act that remains technically self-caused).

Others also report the attenuation of the P2 component, for instance, that the suppression of P2 might relate more to the experience of agency since it was found to correlate with the sense of agency perception as well as body ownership (Pyasik et al., 2021; Timm et al., 2016). Related, Kühn et al. (2011) measured N1 and P3a to tones that either were perceived to be caused by participants' actions or not and whether these components were related to later judgment of authorship over the tones. Their findings were in line with N1 attenuation previously reviewed findings such that N1 was attenuated for congruent tones that matched with the learned action-effect association but N1 was not attenuated for incongruent tones that mismatched such action-effect mappings. The authors further found that only P3a was directly related to the judgment of agency such that P3a deflections were significantly greater for self-caused tones compared with tones that were perceived to be externally caused. N1 was not related to explicit judgment of agency. The authors concluded that the evidence for how later judgment of agency was predictable based on the earlier P3a component suggests that the judgement of agency might incorporate early information and therefore is not purely retrospective and after-the-fact evaluations over the outcomes. Vastano et al. (2020) in complimentary linked the late P300 to the temporal binding effect. The authors manipulated the fluency of action wherein participants performed actions that were either congruent or not congruent with a visual target and such actions caused a tone to occur. They found that congruent actions led to greater temporal binding effect which was associated with greater P300 evoked during the processing of congruent stimuli.

Sidarus et al. (2017) further shows that the sense of agency is supported by both prospective and retrospective neural signals indexed by distinct ERP components. While incompatible action priming increased the target-locked N2 (N2 reflecting early response conflict during action selection), N2 amplitude was not related to agency judgments. This suggests that early conflict detection by itself is not what reduces the sense of agency. In contrast, the action-locked correct-related negativity (CRN) was a key prospective correlate of agency: larger CRN amplitudes, illustrating post-decisional action monitoring and uncertainty about response selection, were associated with reduced feelings of control over subsequent outcomes. In addition, the outcome-locked feedback-related negativity (FRN)

was also negatively related to agency, indicating that unexpected or poorly predicted outcomes attenuate the sense of agency, particularly early in learning. These findings thus demonstrate that agency arises from a mix of post-decisional action monitoring and outcome monitoring signals. Early action selection conflict alone does not sufficiently moderate sense of agency.

In social contexts, researchers have shown associations between additional ERPs and the sense of agency. Considering the notion of vicarious agency wherein people feel control over the movements of others that in an experiment by Wegner et al. (2004) participants experienced the illusionary feeling of controlling the same movements that were executed by the experimenter when watching from a first-person perspective. Drawing on this, studies like Pavone et al. (2016) and Pezzetta et al. (2018) showed that when observing errors induced by a virtual avatar from a first-person perspective, participants showed greater Error-related Negativity (ERN) as well as heightened theta power. This means that the brain processes the virtual avatar's errors as if they were self-generated, and this supports the notion of vicarious sense of agency.

Neural oscillations

EEG has also been utilised to further investigate the external + ability sense of agency to understand how individuals may distinguish one's own action from which of others. Using a dual-EEG imitation paradigm, Dumas et al. (2012) found that when participants occupied the role of model (their movements were imitated by a partner, ascribing primacy of action to self), beta amplitude decreased over right fronto-central regions contrasted with rest. The authors interpret this beta desynchronisation as reflecting greater integration of sensorimotor processes, consistent with self-agency being linked to active engagement of the motor system in generating and monitoring one's own action. Conversely, when participants occupied the role of imitator (copying the partner's movements, ascribing primacy of action to the other), delta and theta amplitude increased over fronto-central and centro-parietal regions contrasted with rest. The authors propose that this low-frequency increase reflects top-down attentional control exerted over the primary motor cortex when tracking and interpreting an externally generated action. This indexes the additional monitoring demands involved in ascribing agency to another person, since the movement cannot be predicted through the same internal motor prediction mechanisms available for self-generated action. This dissociation suggests that self- and other-ascription of agency are supported by distinct oscillatory mechanisms:

sensorimotor integration for self-agency and attentional/predictive monitoring for other-agency.

Remarks and acknowledgement

The chapter above shows that the sense of agency literature is indeed complex and nuanced. This is because there are many issues with the way a) how the sense of agency is defined, b) that there are several different frameworks attempting to understand mechanisms underlying the sense of agency and c) methodological issues wherein different measures claimed to measure the same sense of agency construct do not converge. The review chapter above, whilst attempting to be as clear and thorough as possible, was by no means exhaustive and it is beyond the scope of this chapter and the thesis itself to reconcile these issues. For the sake of avoiding repetition, the rest of the thesis will be focussing on a) external + phenomenal character sense of agency as defined by Grünbaum & Christensen (2020) b) using the hierarchy framework of sense of agency (See Figure 2) which highlights how top-down influences could exert impact on sensorimotor sense of agency over otherwise unrelated action-effect and c) using explicit measure ('how control do you feel') and especially implicit measure, primarily the temporal binding effect (measured using interval reproduction, interval estimation and Libet clock paradigms) whilst acknowledging forwardly that these measures might not correlate with each other and have reliability-related issues.

CHAPTER 2

Mechanisms underlying the temporal binding effect and the malleability of agency.

What is temporal binding actually measuring?

In the previous chapter, we have discussed how the temporal binding effect has been widely used in the context of voluntary actions and thus conceptualised as a marker for the external + phenomenal sense of agency (Grünbaum & Christensen, 2020). The traditional interpretation, especially following Haggard et al. (2002), is that this binding reflects a **pre-reflective sense of agency**, which is a kind of implicit, non-conscious feeling that one is the author of an action and its consequences. This sense of agency is described as 'phenomenally thin' or 'elusive', meaning that it is not easily accessible to introspection. Researchers argue that temporal binding offers a way to measure this implicit feeling, which might not be captured by explicit self-reports. Temporal binding has long been a robust and replicable effect in research employed the method.

However, since the emergence of the binding effect, there have been a notable number of studies to demonstrate that the temporal binding effect can occur a) without intentionality, b) without a voluntary action i.e., the action is executed involuntarily or c) without any action at all. The strongest alternative proposal accounting for the binding effect is that causal belief drives the temporal binding effect. Buehner and Humphreys (2009) was the first to show that intentional action on its own, without the establishment of causal relation, is insufficient to yield the temporal binding effect between actions and outcomes. In this study, their experimental conditions were identical, both in terms of perceptual and motor demands with the only exception: participants were informed that their response to the first stimulus was merely preceding the second stimulus in the non-causal condition; whereas in the causal condition, the participants were told that their response to the first stimulus *caused* the second stimulus. As a result, only in the causal condition was the temporal binding effect between participants' actions and the consequential event observed, but not in the non-causal condition where the two events were perceived to be not causally related, despite both conditions being identical in every other aspect. Likewise, the author later established that the temporal binding effect could occur without any intentionality, because participants exhibited the binding effect even when a machine lever pressed the button to execute the action (Buehner, 2012). Buehner (2012) indicated that for temporal binding effect to take place, intentional and voluntary action is not required and thus replaceable with mere nonintentional/mechanical causation.

Findings from later studies also casted doubt on whether temporal binding effect is truly a measure of agency, considering it can occur even without intentionality and voluntary

action, which is problematic because intentional and voluntary actions are the core foundation of the agency concept (Pacherie, 2008). Notable study by Suzuki et al. (2019) further disregarded the role of intention in binding using Virtual Reality (VR) and Augmented Reality (AR) that shorter intervals i.e., greater temporal binding effect were found in both active/voluntary action and fake-hand conditions relative to the no-hand or control condition. Tellingly, the binding effect was not significantly different between the active and fake-hand conditions, suggesting that intentional action was not necessary to produce the binding effect. Kirsch et al. (2019) also found equivalent magnitude of temporal binding effect for involuntary and voluntary actions after controlling for temporal predictability. Without subtracting baseline values, participants perceived their voluntary actions to occur later than involuntary movements but only in the unpredictable session, whereas such difference disappeared in the predictable condition. Their findings indicated that the difference in perceived timing was due to predictability, not intention. Kirsch et al. (2019) proposed that their findings supported the causal inference theory as stated by Buehner (2012) wherein two events are bound together in time if they are believed to be causally related. They further proposed another framework to account for temporal binding, which is the multisensory integration theory. This account suggest that the brain is able to combine information from multiple sensory sources, including vision, proprioception and auditory stimuli. Importantly, the brain only merges the sensory modalities which are likely to come from the same source. Such combination forms a coherent and unified perception of our body within the world. Thus, based on this multisensory integration framework, the temporal binding effect is the outcome of such multisensory integration processes wherein an action and its effect are integrated together and represented as if they were both a single meta-event (Cao et al., 2020; Kirsch et al., 2019; Klaffehn et al., 2021; Tonn et al., 2021; Wolpe et al., 2013).

Altogether, these results warrant caution when interpreting temporal binding strictly as a proxy for the sense of agency since the binding effect seems reflect a general process of causality or multisensory grouping, regardless of intentionality and voluntariness. However, temporal binding has been continued using as an index for implicit agency because of the following reasons.

Firstly, a recent study by Wiesing and Zimmermann (2024) attempted to directly replicate the previously discussed research by Suzuki et al. (2019) whose findings posit that intentionality does not drive the binding effect. Wiesing and Zimmermann (2024) noted that results of Suzuki et al. (2019) showed a clear regression to the mean which could have

potentially founded the temporal interval estimations. In their first experiment, they presented the temporal intervals block-wise instead of trial-by-trial basis and found systematically greater compression for active than fake-hand trials, contrasting Suzuki et al. (2019). In their second experiment, they conducted a direct conceptual replication of the experimental procedures and not only did they not replicate the results, Wiesing and Zimmermann (2024) instead found even more pronounced temporal binding effect in active trials compared with the fake-hand trials. These findings thus support the possibility that intentionality still plays a crucial role in the temporal binding effect.

Secondly, we shall revisit the vicarious sense of agency as mentioned in the previous chapter that people could experience the sense of being in control over someone else's movements when observing from the first-person perspective (Wegner et al., 2004). This is because of the mirror neuron system that allows us to infer others' intentions and states by using our own sensorimotor and mental representations (Rizzolatti et al., 1996). As a result, temporal binding effect has also been found for others' produced actions (Pascolini et al., 2021; Poonian & Cunnington, 2013; Wohlschläger et al., 2003). Additionally, such evidence showing how temporal binding effect could occur without action i.e., observed actions by others (or machines, see below) is also inconsistent with the previous claim that motor stimulation is a prerequisite for temporal binding to occur (Antusch et al., 2021b). As a result, causal inference theorists could use this pattern to argue that the temporal binding effect is not a measure of sense of agency because if temporal binding can occur from observed action alone without any motor involvement by the observer, this suggests temporal binding reflects the formation of a causal belief about the action-effect relationship rather than any agentic or motor process specific to the observer.

However, temporal binding effect might not be likely to be reducible to either causality or agency alone or that there is no in-between. The mirror neuron account above does not straightforwardly extend to vicarious temporal binding effects observed for machines or artificial agents which typically have very different effectors from human hands. Instead, in human-machine interaction studies, the relevant mechanism appears to be intentionality attribution i.e., the extent to which an observer attributes mental states and an intentional stance to the artificial agent which itself arguably is a form of causal belief. Many studies found that temporal binding effect was present when participants were observing a robot or artificial agent, but *only* when the participants perceived the robot agent to have intentionality i.e., attributing mental states and intentional stance towards to the robotic agent (Navare et al., 2024; Pascolini et al., 2025). When participants did not perceive these robotic

agents as having intentionality, no temporal binding was observed for the robots' actions even though it was clear that the robots' actions produced the outcomes, demonstrating a clear causal action-effect link. These findings suggest that intentionality, which could be understood here as a form of causal belief directed at the observed agent, can modulate temporal binding effect. Thus this supports how causality and sense of agency can interact and how temporal binding effect can be sensitive to this graded interactive causal-agentive process rather than to mere presence of any causal link (Hoerl et al., 2020).

Thirdly, in response to the multisensory integration theory account, we do not refute the possibility of the brain combining sensory inputs in a statistically optimal way and thus fusing them into a single percept if they are perceived to be relate to each other. However, the multisensory integration process on its own cannot account fully for the temporal binding effect. Recalling the study by Moore and Haggard (2008) who found action binding even in the absence of any outcomes, this makes the account of integrating multiple sensory inputs irrelevant because in this scenario, only one event/input was present. Lastly, in a study by Weller et al. (2020), participants reported sense of agency for events following nonactions i.e., deliberating not doing anything and because of not acting, this caused an outcome to occur. It was found that temporal binding effect between nonactions and the outcomes was indeed present and robust, even if only one event took place (i.e., the outcome). As a result, the multisensory integration theory could not explain fully the mechanisms driving the temporal binding effect, not at least to the same extent as intentionality or causality (Klaffehn et al., 2024).

It is irrefutable that causality plays an integral role for the temporal binding effect to occur. However, using temporal binding as a measure of sense of agency is complicated by the fact that the sense of agency itself is best understood as a form of causal belief (Hoerl et al., 2020). Neither causal belief nor sense of agency is a simple binary (yes v. no) and both have varying degrees and the two can interact, for instance, a stronger sense of agency may inflate the perceived strength of the causal relation between action and outcome and this in turn may be further modulated by self-relevance or authorship. As mentioned above (with the vicarious agency in human-machine interaction), temporal binding may therefore be sensitive to this graded interactive causal-agentive process (Hoerl et al., 2020). In response to such criticism, perhaps the best way forward for using the temporal binding effect to measure agency depends on the experimental design, whilst keeping the role of causal belief in mind. That is, temporal binding could serve as a proxy for implicit sense of agency only if the design allows causality and agency to be separated. In particular, one must design the

experiment so that self-attributed, voluntary action-effect causality is contrasted against a condition that preserves the same causal relationship between events but removes or varies the agentic component (e.g., voluntariness or self-attribution). For instance, considering the classic key press-tone design, if one wishes to use temporal binding between key and tone as a measure of sense of agency, the operant condition must be one in which participants believe their own key press causes the tone. Meanwhile the control condition must retain a comparable causal relationship between the two events. For instance, in the control condition participants can be led to believe that the occurrent of the first tone would causally trigger the second tone, or that the second tone only occurs because the first tone has been played. Such design of the control condition keeps the causal relationship between the two events but without the voluntary action from the participants. Another possible control condition could be that the same key press and tone occurring together but without the participant's voluntary by applying TMS like Haggard et al. (2002) or participants' index finger being pressed down by the experimenter instead like in Borhani et al. (2017), rather than removing causality altogether by making the two events independent. If the two events in the control condition are simply independent the comparison no longer isolates agency from causality such that it removes causality entirely and so cannot exclude the alternative explanation that binding is driven merely by causal belief rather than agency. The two events being bound and their causal relationship should therefore be held constant across the operant and control conditions, with only the agency-relevant dimension (e.g., voluntariness, self-attribution) varying between them. Only under this kind of design could one be confident that temporal binding is attributable to intentional agency, above and beyond causal belief alone (Borhani et al., 2017; Poonian et al., 2015; Scott et al., 2022).

Notably, this criterion is not consistently met in existing temporal binding paradigms. In the widely used version of the interval reproduction task the operant condition compares key press-tone binding against a baseline condition comprising tone-tone binding (Howard et al., 2016; Humphreys & Buehner, 2009, 2010; Pascolini et al., 2025) (Experiments 3 and 4 in the current thesis, following standard practice in the field). Here the two conditions differ not only in the agentic dimension but also in the identity of the events being bound, making it unclear whether any difference in temporal binding reflects agency, causality or simply the change in event type. This limitation should be borne in mind when interpreting findings from such paradigms including those reported later in this thesis.

Malleability of agency

Having established that the temporal binding effect remains a valid proxy for the implicit sense of agency while keeping in mind the importance of causal belief and the need to control for causality carefully, we will now review the factors that modulate the magnitude of binding effect, highlighting its flexibility, plasticity and sensitivity to both non-social/personal and social/interpersonal contextual cues.

Non-social cues at personal level

Firstly, we consider the factors that modulate agency at the action selection, planning and execution stages. Motor stimulation is also proposed to be a prerequisite for binding to occur as it has been shown in the absence of actual motor movements, temporal repulsion (opposite of temporal binding) occurs (Antusch et al., 2021b). Freedom to choose amongst a number of action alternatives has been found to increase temporal binding effect that individuals showed the strongest magnitude of the temporal binding effect when they had the maximum of number of action choices available compared with medium level of choice or no choice at all (Barlas et al., 2017; Barlas & Obhi, 2013). Although, Antusch et al. (2021) noted that temporal binding effect might be robust and not easily disrupted by removing autonomy over action and timing, as long as the actions align with internal goals and values. The authors found that temporal binding effect occurred in all full-autonomy, partial-autonomy or no-autonomy conditions (Antusch et al., 2021a). Deliberate rule breaking could also influence temporal binding that participants experienced lower temporal binding effect (at 100ms interval) which reflects cognitive conflict during rule breaking (Galang et al., 2025). As previously mentioned, generating actions when being in a fearful or angry states could also attenuate the action binding effect (but not outcome binding) (Christensen et al., 2019). Actions performed with high physical effort or under great cognitive load also decreased temporal binding (Howard et al., 2016). These findings show how/why/when actions are selected or planned and executed could influence the perceptual shift in the temporal relationship between action and its outcome (Bonicalzi & Haggard, 2019; Chambon et al., 2014; Sidarus, Vuorre, & Haggard, 2017a).

Secondly, regarding the outcome component of the temporal binding effect, we must also consider what, how and/or when the effects or outcomes of actions occur could modulate temporal binding. For instance, considering the predictability of the identity of the outcome, in the study by Desantis et al. (2011), participants performed two possible actions i.e., left or right key press which then caused one of two action outcomes, either corresponding to their

choice or decided randomly. The predictability of outcome identity and the congruency between the outcome identity associated with the chosen action and the actual outcome identity did not influence outcome binding. On the other hand, Huang et al. (2024) showed that manipulation of outcome identity must be more specific for it to modulate the binding effect. In particular, the authors found significantly larger outcome binding when the sound stimulus was self-related that to strangers, which highlights the influence of self-associated information on the temporal binding effect. Other studies on predictability of outcome also suggest more nuances such that Majchrowicz and Wierzchoń (2018) found that identity-unexpected (oddball) action outcomes produced enhanced rather than reduced temporal binding, similar to how Cui et al. (2018) (see below) reported higher temporal binding for unpredictable fearful stimuli. At first glance this seems inconsistent with accounts in which better-predicted outcomes bind more strongly. However, the predictability manipulated in the above reviewed studies pertains to the *identity* of the outcome whereas other research suggesting predictable outcomes lead to stronger binding focuses more on predictability of *when* the outcome occurs (Moore & Haggard, 2008 etc.). Thus, the direction of the effects in which predictability operates depends on whether it is about a) which outcome will occur or b) when an outcome will occur. Temporally predictable outcomes and identity-unpredictable outcome might both enhance binding. As to explain for why this might be the case, Majchrowicz and Wierzchoń (2018) suggested that learned action-outcome associations preactivate the sensory regions that register an anticipated effect, lowering the detection threshold and shifting its perceived timing earlier. It is possible that both temporally predictable outcomes and identity-unpredictable types of outcome are more easily registered against this already-elevated activation hence producing an even larger temporal shift and thus stronger temporal binding.

Next, the valence of the outcome is also particularly crucial in modulating binding, although the evidence here is mixed. Yoshie and Haggard (2013) found that the temporal binding effect was attenuated for negative outcomes compared with positive or neutral outcomes. Similarly, predictable fearful stimuli as the outcomes reduced the magnitude of the temporal binding effect (Cui et al., 2018). However, not all negative outcomes weaken the binding effect. Other studies found the opposite patterns such that Di Costa et al. (2018) found that the temporal binding effect was stronger following negative than positive outcomes. Negative outcomes also enhanced action binding on the next trial, but only when participants could use the error to improve future choices. Majchrowicz et al. (2020) also found similar results that during a probabilistic learning task, self-initiated actions resulting in

losses produced a boost in temporal binding effect compared to trials that followed wins. Cui et al. (2018) themselves reported that while predictable fearful outcomes reduced temporal binding, causing unpredictable fearful stimuli, in contrast, strengthened the temporal binding effect. Along this line, Jenkins and Obhi (2022) found that temporal binding effect was stronger for actions producing mistakes compared with those perceived as correct. The findings above suggested that when actions produce negative outcomes or mistakes that could help individuals improve subsequential behaviours, avoid further downfalls and thus strengthening learning, this would lead to stronger sense of agency, as indicated by greater temporal binding. In contrast, it is possible that negative outcomes as observed in Yoshie and Haggard (2013), whose valence of each trial was already predictable in advance and thus would not contribute to reinforcement/improvement, might therefore attenuate temporal binding effect. More broadly, this suggests that the direction of the valence effect might therefore depend less on valence per se and more on factors such as predictability, error monitoring or the reinforcement-learning context in which the outcome occurs. Thus, negative outcome is too simplistic a category to explain these findings on its own. Altogether, looking at the valence and predictability of outcomes the temporal binding effect appears to be very sensitive to contextual and task-specific factors but the current evidence does not support a single coherent account of the direction in which these factors operate.

Social cues at interpersonal level

What we have reviewed so far occurred at the personal level i.e., not within the social contexts. Humans are social species and thus our actions are often executed in social environment wherein there would be other agents present. Indeed, the temporal binding effect has been shown to be influenced by various social cues in different types of interpersonal interactions. Working with others in a joint action which serves a common goal also gives rise to the temporal binding effect, and such phenomenon is termed sense of joint agency (Obhi & Hall, 2011). Vogel et al. (2021) found that when believing that one was interacting with another person, self-produced actions caused a social face-like stimuli (versus a mere physical effect), the temporal binding effect was more prominent. Similarly, results by Silver et al. (2025) indicated that temporal binding was greater in social cooperative interaction compared with non-social context, and that social temporal binding effect was stronger at shorter action-effect intervals compared with non-social condition.

In addition, how our action outcomes are perceived by others would also moderate binding. Specifically, Chen et al. (2024) investigated the effect of social feedback on sense of

agency and the authors found stronger temporal compression between voluntary action and its outcome interval when the social feedback was negative. Likewise, negative outcomes after following social advice from others were also found to lead to greater temporal binding magnitude (Reis et al., 2022). These results are in line with what was previously discussed about binding system aiding the detection of negative events and thus allowing individuals to learn from adverse outcomes (Cui et al., 2018; Jenkins & Obhi, 2022; Tanaka & Kawabata, 2021) that the binding system also helps agents to learn and avoid producing future undesirable social consequences.

Lastly, it is insightful to investigate how one's actions outcomes could influence other people contexts, because our actions often have social consequences. If one's action causes a social outcome, for instance, eye gazing, this could also produce the temporal binding effect (Stephenson et al., 2018). Wang and Zhou (2024) showed that when individuals made actions to uphold social norms and not purely out of actual personal interest, the temporal binding effect was more pronounced. In extreme social contexts where people might be coerced to execute actions that would lead to harmful consequences towards others, the temporal binding effect is attenuated. In these scenarios, such attenuation in binding is considered to reflect a dissociation of the self from the immoral behaviours, or that the attribution of authorship is not made to the self but to the coercer (Caspar et al., 2016; Niu et al., 2023).

Antecedents of temporal binding

From the end of Chapter 1, we have reviewed a potential schematic (Figure 2) which helps build the theoretical reasoning of the review below. In particular, it motivates our focus on higher-level influences on the sense of agency, including beliefs and social factors, to examine how individual differences and experimentally induced changes in these factors might influence the subsequent sense of agency over otherwise unrelated action-effect associations. Such approach is grounded in the reviewed theoretical accounts proposing that higher-level cognitive and social processes exert top-down influences on lower-level sensorimotor processing involved in the experience of agency rather than sense of agency arising solely from bottom-up motoric signals or factors that are specific to that context in which the action and outcome take place (Seow et al., 2021; Stephan et al., 2016; Synofzik et al., 2013). As humans are generally understood to be fundamentally agentic, it is plausible that both a) the stable dispositional traits we carry with us and b) the immediate states in which we are situated could shape how the sense of agency over a 'simple' voluntary and sensorimotor action and its outcomes is attributed and experienced.

Such a theoretical framework highlighting the top-down influences on sensorimotor sense of agency thus motivates the review below. The aim of this review is to identify different examples of studies which investigated how higher-level influences such as background beliefs, trait, knowledge and information about the environment/contexts etc. could influence the feeling of agency (like Synofzik et al. (2013)'s account of optimal cue integration underlying the experience of agency). In turn, these studies help us understand **whether broader psychological factors (states and individual differences) could moderate the temporal binding effect as a measure of the feeling of agency, even over action-effect association that are otherwise unrelated to those broader factors.** This is relevant because the empirical chapters (except for Chapter 5) will adopt similar approach. As a result, the review focusses on the rather overlooked body of research that assess the factors surrounding the temporal binding effect without interfering with or manipulating the paradigm at all.

Crucial to note, the review below is a narrative review. Studies included in this review (Appendix A) were identified primarily through searches of PsycINFO and Google Scholar using combinations of terms including 'temporal binding' or 'intentional binding' with 'individual differences', 'trait', 'state', 'personality', 'belief', 'clinical', 'clinical condition', 'control' supplemented by hand-searching the reference lists of key papers (Aarts & Van Den Bos, 2011; Cavazzana et al., 2014, 2017; Edwards et al., 2025; Hascalovitz & Obhi, 2015; Obhi et al., 2012; Soral et al., 2021). Searches were not bounded by publication date and were conducted up to June 2025. Studies were included if they measured temporal binding as an index of implicit sense of agency in relation to stable individual differences factors including long-term conditions/personality/traits; or an experimentally induced psychological/physiological state. Studies that manipulated the temporal binding paradigm itself (varying action-effect identity pairing, predictability of outcome, contingency or feedback validity etc.) were excluded because such manipulations could confound agency-related variance with perceptual, causal or multisensory processes, and have already been comprehensively reviewed elsewhere (see Hughes et al., 2013; J. W. Moore & Obhi, 2012; Tanaka et al., 2019; Villa et al., 2022), whereas no prior review has focused specifically on the antecedents of temporal binding with the paradigm held constant.

We will now look at studies that a) measure inter-individual differences in the temporal binding effect without manipulating the action or effect components, i.e., simply measure between-subject differences in the magnitude of temporal binding, or b) manipulate only the psychological background or context preceding the task used to capture the binding

effect, i.e., the antecedents of temporal binding. To meet criteria for inclusion, studies had to use a standard temporal binding paradigm (either interval estimation, interval reproduction or Libet clock) that was not itself experimentally varied. For instance, studies that manipulated action-effect delay, contingency or feedback validity as an independent variable were excluded.

At each of the two levels of analysis i.e., personal v. interpersonal, we categorised studies into focussing either on a) long-term and stable individual differences variables such neuropsychiatric disorders, personality traits, beliefs and age and b) short-term, temporary states (mood valence, arousal, externally administered drug/alcohol). With regards to the former, selected research on stable traits, chronic neurological/psychiatric conditions and other long-term individual differences variables would involve recruiting participants of interest, assigning them to groups and comparing the magnitude of temporal binding across these groups. Notably, as previously mentioned, these studies do not involve manipulating any parameters of the temporal binding paradigms utilised as they straightforwardly ask groups of participants to perform the tasks to measure binding effect and then contrast the level of binding between groups. By doing so, the researchers could observe the difference in how temporal perception of voluntary action is influenced by whichever personality, long-term traits, beliefs and conditions/disorders a group of participants might have compared with their counterparts who do not. Concerning the later subset of studies investigating states, such research entails experimental manipulation of only the psychological backgrounds preceding the performance of the binding task. In other words, researchers manipulate the states in which participants are before completing the temporal binding paradigm and measure if moderating these states would influence the subsequently measured temporal binding effect. Altogether, whether concerning the long-term inter-individual differences factors or the manipulation of temporary states, these studies reviewed here share in common that they do not manipulate the action and outcome components of the temporal binding paradigm, and they have been used by the authors as a putative marker of the implicit sense of agency. As a result, taken together they can offer a starting point for considering:

- How the temporal binding effect, as a marker of the implicit agency, varies at the group level between individuals differing in particular traits or conditions.
- Whether being in certain temporary psychological or physiological states could be similarly associated with shifts in the magnitude of temporal binding within individuals.

It is important to note that the link between these traits/states and the sense of agency is itself partial and putative rather than established because many other factors will differ across the individuals/conditions compared and thus we shall take care not to conflate the trait/state label with the construct of sense of agency.

Personal factors: Individual differences – long-term conditions/personality/traits

Neurological and psychiatric disorders

Some research has been done in clinical populations wherein participants completed temporal binding task without further manipulations, and the individual differences are highlighted by contrasting the magnitude of binding effect between clinical groups and control population. In general, these studies involved neuropsychiatric disorders would entail demonstrating the differences in the magnitude of the temporal binding effect between healthy control individuals and patients with specific neurological and/or psychiatric disorders. The studies that met our criteria had a) no experimental manipulation applied to the temporal binding paradigms and b) direct comparison of the binding magnitude between healthy controls and patients with disorders and conditions of interest.

Schizophrenia spectrum receives much attention as we identified a number of studies comparing the difference in temporal binding effect between people with schizophrenia and control. This is understandable considering how the condition is starkly characterised by atypical perceptions of agency falling within the positive symptoms i.e., hallucinations and delusions. These studies all typically demonstrate that patients with schizophrenia show an abnormally high level of action-effect binding compared with nonclinical population (Franck et al., 2005; Haggard et al., 2003). It is proposed that individuals with schizophrenia have impaired internal sensorimotor predictions and thus they have to rely more on extern sensory cues, affecting their ability to integrate external auditory, visual information and bodily somatosensory stimuli (Shergill et al., 2005). Such impairment is often leading to the delusion of control as they cannot credibly tell the difference between internally or externally generated outcomes, the tendency to over-associate actions and outcome and thus the abnormal increase in the temporal binding effect. It is worth noting that the problems with predicting the outcomes of actions might be most prominent in later stages of the disorder wherein worsened dopamine dysfunction in the patients introduces noise to the neural system's error detection signals and thus resulting in unreliable predictions (Moccia et al., 2024; Peterburs et al., 2013). Graham-Schmidt et al. (2016) showed more complex relationship between temporal binding effect and the subjective experience of agency. The

authors highlighted the role of having passivity symptoms i.e., delusions of control amongst people with schizophrenia. Individuals *with* passivity symptoms had impaired temporal perception of voluntary action, whereas those *without* passivity symptoms can differentiate between active v. positive movements but mistakenly attribute others' action outcomes to themselves. Lastly, by recruiting healthy participants who showed high schizotypy, Moore and Bravin (2015) found that having schizophrenic symptoms could indeed alter one's capability of intention awareness as shown by the Libet Clock paradigm. In other words, high schizotypy individuals do not have reliable internal predictions of knowing *when* they themselves intend to act, suggesting an altered sense of agency. Along similar line, one study looked at the Corticobasal Syndrome i.e., a rare progressive neurological disorder characterised by the degeneration of brain cells in the cerebral cortex and basal ganglia - causing problems with movement, speech etc. They found that like people with schizophrenia, corticobasal syndrome patients had stronger action binding in the most affected hand (Wolpe et al., 2014). Their findings thus also highlighted how abnormal integration of sensory cues could lead to an excessive perception of temporal binding between action and its outcome as depicted by the over-dependence on external sensory information rather than internal predictions.

Next, to date, there are also studies focusing on patients with Parkinson's disease, which is a condition accompanied by degenerating dopamine-producing neurons that causes disruptions in voluntary motor movements. Firstly, Moore et al. (2010) found that patients with Parkinson's disease who had taken dopaminergic medications showed highest level of temporal binding effect (compared with when they did not take their medication and to healthy groups). Thus, patients with Parkinson's disease do not merely experience altered agency but this rather depends on the dopaminergic medications and how they affect the striatal reward prediction error signals. Being on dopaminergic medications thus boosts the learning of action-effect associations and therefore increase the temporal binding effect. In complimentary, Ricciardi et al. (2017) further assessed Parkinson's with Impulsive Compulsive Behaviours (ICB) in contrast to without ICB and control. The authors found that Parkinson's patients with ICB showed stronger action binding than control whilst there was no difference in temporal binding between patients without ICB and control. Their findings highlight ICB as a possible side-effect of dopaminergic medications because ICBs are associated with abundant stimulation of the dopaminergic receptors in the limbic system, and this atypically promotes the excessive associations of action-effect. However, Saito et al. (2017) found that compared with healthy participants, patients with Parkinson's disease had

reduced action binding (but tone binding reserved). This highlights that long-term dopaminergic depletion, without the development of ICB, still leads to a weakened sense of agency, as illustrated by reduced action binding, especially when it comes to initiating voluntary motoric movements.

There are further studies focussing on temporal binding effect in patients with Tourette syndrome (a neurological condition causing tics i.e., involuntary sounds and movements) and Motor Conversion Disorder (or functional neurological symptom disorder – also a neurological condition causing tremors and paralysis which impairs the patients' movement or senses) (Ricciardi et al., 2017; Zapparoli et al., 2020). Both of these showed that patients with Tourette syndrome and those with motor conversion disorder experienced a reduced temporal binding effect compared with healthy control. In contrast to the neurological conditions mentioned above, involuntary movements amongst patients with Tourette syndrome and motor conversion disorder are often the results of increased internal monitoring of the tics. As a result, these patients focus intensively on the details of their movements, which makes them become excessively aware of the sensory feedback. Meanwhile, note that typically the motor system naturally provides only a subtle or "thin" feeling of control that one does not need to consciously monitor every motoric action. The heightened awareness amongst patients with Tourette and motor conversion disorder backfires this automatic pre-reflective sense of agency that one's motor system would typically produce (Stenner & Haggard, 2016). As a result, this might disrupt the normal integration of actions and their outcomes and therefore produces a reduction in the temporal binding effect. One last notable neurodevelopmental condition studied is Autism Spectrum Disorder, which often affects how people on the spectrum could socially communicate, interact or process sensory information. Sperduti et al. (2014) found that adults with autism have reduced temporal binding effect compared with control. This is consistent with the previous research on autism suggesting that being high on the autistic spectrum is associated with impaired action planning and monitoring as shown via dysfunctional pre-SMA activity during action planning processes. Altogether, these highlight how having impaired or abnormal internal action monitoring and planning as shown in patients with Tourette, motor conversion disorder and autism could lead to a weaker temporal binding effect.

Finally, we also found two studies focussing on psychiatric disorders, namely Anorexia Nervosa and Major Depressive Disorder. Engel et al. (2022) examined whether implicit sense of agency measured using temporal binding effect differs across anorexia nervosa patients, recovered anorexia nervosa patients and healthy control but the authors

found no difference in temporal binding across the three groups. Their results showed the importance of including different neurological or psychological conditions that could moderate the feeling of control differently. The authors denoted that the specific mechanisms underlying the temporal binding task might not be impaired amongst patients with anorexia nervosa in contrast to people with schizophrenia. In particular, whilst anorexia nervosa patients do experience the deficit in control, such feeling refers more to the negative attitudes towards self-images rather than the feeling of control over action and sensory outcome, which is best characterised by temporal binding. In contrast, patients with major depressive disorder undergo persistent feelings of sadness and loss of control, and thus Vogel et al. (2024) found that they had a reduced temporal binding effect. Feeling hopeless, loss of control and persistent sadness thus substantially impair how one with depressive disorder experiences the sense of agency and therefore weaken the causal association between one's own action and its effect, ultimately resulting in the attenuation of temporal binding measured amongst individuals with major depressive disorder.

Therefore it is tentatively possible that misattribution of agency associated with delusional symptoms or dysregulation of the dopaminergic system might be linked to increased temporal binding, whereas impairments in action monitoring and planning have been associated with reduced binding. However, these remain proposed explanations for observed group differences rather than mechanisms that have been directly established.

Personality, stable traits, age

Now, we focus on studies that investigate long-term individual difference factors in healthy population which assess how the temporal binding effect varies from one person to another based on certain personality factors or traits. Cavazzana et al. (2014) was the first to examine if there were individual differences in the temporal binding effect across age groups. The authors found that young children of 10 years of age had weaker temporal binding effect, particularly action binding, compared with adults with mean age of 23 years. Note that action binding reflects the predictive process of the motor control system. This means that children are likely to have this predictive system yet fully developed compared with young adults. The authors later expanded their findings by including a group of elderly participants with mean age of 70 years (Cavazzana et al., 2017). Not only did they replicate the results that young children had weaker binding effect compared with adults, but the researchers also found that elderly participants also had reduced binding effect compared with adults. There was no difference in the magnitude of binding effect between children and elderly participants.

Another study looking at aging as the individual difference factor in temporal binding by Mariano et al. (2024) and the authors confirmed that older adults showed attenuated temporal binding effect compared with younger adults. These studies controlled for the difference in general temporal perception as different age groups did not differ in time perception in the baseline condition. The disparities in the binding effect observed were driven primarily in the context of participants executing voluntary actions.

Explanation for the differences in temporal binding effect observed across age group is based on the modulation of the internal clock i.e., composed of a pacemaker and accumulator enabling organism to anticipate and adjust to rhythmic phenomena (Buonomano & Laje, 2010). It has been demonstrated that temporal binding is indeed associated with the rate of the internal pacemaker: the internal clock slows down briefly after a voluntary action is made and then speeds up again (Wenke & Haggard, 2009). Children and older adults might have the internal pacemaker accelerate to a greater extent than which of young adults whose internal dynamic clock slow down more (Cavazzana et al., 2017). As a result, the reduced temporal binding effect observed in young children and older adults could be the result of the increase in speed of the internal pacemaker.

Personality is another stable individual difference factor that could have top-down influences as proposed in the hierarchal model reviewed in Chapter 1 (See Figure 2) which might moderate the temporal binding effect. Moore (2023) looked at the borderline personality disorder (BPD) trait which is characterised by interpersonal, affective and identity instability and the author found that higher BPD trait was associated with weaker temporal binding effect. Another notable personality trait is narcissism, typified by a grandiose sense of self-importance and belief in self-superiority. This overt emphasis on the self would thus mean that individuals high on this personality trait would be likely to experience the sense of self-agency strongly and therefore show heightened temporal compression between self-generated action and its outcome. Indeed, Hascalovitz and Obhi (2015) found that participants with high and moderate narcissistic personality had stronger temporal binding effect, particularly tone binding, compared with their counterpart scoring lower on the narcissistic personality scale. Render and Jansen (2019) further expanded on this and found that the types of narcissistic personality played a role in modulating the temporal binding effect. In particular, the authors found that individuals with high level of vulnerable narcissism (fragile self-esteem, feeling inadequate but with the tendency to feel victimised) showed weaker temporal binding effect compared with those who scored lower on vulnerable narcissism. Those who were high on grandiose narcissism with a sense of superiority and

arrogance etc. did not show significant difference in temporal binding compared with individuals who scored low on grandiose narcissism. This pinpoints the importance of looking at the various aspects of a personality trait as each subset might influence the implicit sense of agency differently.

Other scholars look at further personality factors that are linked with how possessing these traits could potentially moderate one's sense of agency. A notable long-term and stable individual difference factor is people who are long-term meditators. Mindfulness practice in meditation entails cultivating the awareness of present mental states, including thoughts and intentions. Lush et al. (2016a) found evidence supporting mindfulness meditators had more accurate metacognition related to intention because they reported earlier judgements of the timing of an intention (W's) to move than non-meditators. In a different study, the authors also found that long-term Buddhist meditators had stronger temporal binding effect, especially outcome binding, than control (Lush et al., 2016b). The authors proposed that meditators occupy more reliable sensorimotor representations of the predicted outcomes of an action. These findings are thus consistent with the notion that practising mindfulness in the long run often promotes close attention to concrete experience. Note that Jo et al. (2014) earlier did not find any significant difference in temporal binding effect between mindfulness meditators and matched controls. That said, Lush and colleagues (2016b) conducted a meta-analysis of the two studies and found sensitive evidence supporting an enhanced outcome binding in meditators compared with non-meditators.

Being susceptible to hypnotising seemingly contrasts the characteristics of being mentally aware of one's states and intentions which meditators uphold. Individuals high on hypnotisability are more likely to attribute control and agency to the hypnotist or some features of the hypnotic suggestions in lieu of self-attribution. Accordingly, sensitivity to hypnotic suggestion often alters the sense of agency. Indeed, in the same study, Lush et al. (2016a) showed that high hypnotisability individuals had later W times i.e., delayed awareness of intention. It is therefore proposed that hypnotisability and mindfulness meditation lie at opposite ends of the metacognition continuum related to the conscious awareness of intention (Lush et al., 2016a). Lush et al. (2017) later showed that high-hypnotisable individuals had reduced effect binding in the posthypnotic condition compared with the voluntary condition, even though the two conditions were identical with the only exception that the former involved hypnotic suggestion prior to the completion of the temporal binding task. The authors suggested that the reduced binding stemmed from

involuntariness and the diminished influence of motor intentions. These findings resulting from trait differences in hypnotisability thus help highlight the important role of awareness of intention in the temporal binding effect, suggesting that the binding system could indeed be a marker of the implicit agency.

From studies focussing on trait difference in hypnotisability and meditation, we observe that believing whether one voluntarily acts and causes an outcome is central to the sense of agency and thus might moderate the magnitude of temporal binding accordingly. Along this line, the belief in free will i.e., the extent to which we are free agents could influence one's perception of being in control over an action and behaviour (St Quinton et al., 2023). Aarts and Van Den Bos (2011) showed evidence supporting the role of free will belief in how one experiences the sense of agency in everyday life that participants who believed strongly in free will also showed greater temporal binding effect. In contrast, participants with disbelief in freewill (i.e., everything we do is all determined by external factors that we cannot control, like god's will) showed weaker binding. Once again, this provides the possibility of how one experiences sense of agency, particularly pre-reflective agency without explicit inference, could be moderated by upholding top-down beliefs in free will as such.

To conclude, these studies are valuable because they highlight the potential roles of top-down, higher-order socio-psychological stable beliefs and traits could moderate the temporal binding effect, used here as a putative measure of the feeling of agency, even over action-effect associations that are otherwise unrelated to those broader factors.

Personal factors: states

Alongside long-term traits and conditions, a few studies looking at the short-term factors that precede the temporal binding effect by manipulating experimentally the states in which individuals are in. These studies have given evidence that the cognitive and psychological backgrounds after being manipulated could then moderate the subsequently measured binding, without interfering with the binding paradigms themselves but rather utilise them as means to measure a general implicit sense of agency. Regarding arousal, in two studies, participants were randomly assigned to high arousal v. neutral group and the level of arousal was induced by having the participants watching the video or images that produced the desirable states of arousal (Render et al., 2024; Render & Jansen, 2021). Participants performed the Libet Clock both before and after viewing the clips/images so that their temporal binding effect pre- and post-manipulation of arousal state could be measured. Render and Jansen (2021) found that being in a high arousal state reduced the temporal

binding effect, whilst being in a lower arousal state led to a stronger magnitude of binding. Render et al. (2024) found that high *sexual* arousal state attenuated the action binding effect. Similarly, in the study by Soral et al. (2021), the group of participants who had to solve an impossible task which induced the feeling of learned helplessness had significantly weaker magnitude of temporal binding measured afterwards compared with those who only performed the temporal binding Libet Clock task. Lastly, Obhi et al. (2013) employed a within-subject design and found that after recalling a depressive episode experienced in life, participants showed reduced temporal binding effect compared with before recalling i.e., no prime or simply thinking about previous day's activities.

To reiterate, most studies like Christensen et al. (2019) or Yoshie and Haggard (2017) manipulated the valence of the outcomes which participants' actions caused so as to induce the state of anger, fear or positive/negative outcomes then measure the binding effect between the actions and these outcomes. The studies reviewed above manipulated the *psychological backgrounds* that preceded the temporal binding tasks, without needing to implement any manipulation to the action or outcome components of the temporal binding paradigms. The manipulation of the psychological states (induced sexual arousal, depressive states, learned helplessness) preceding the binding tasks might be associated with reductions in the magnitude of the subsequent action-effect binding being measured, itself unrelated to those states.

Additionally, besides behavioural manipulations to change psychological states, we found studies modulating the states in which participants were in prior to completing temporal binding tasks by administering hormone, drugs, alcohol or even local anaesthesia. In particular, female participants who had been administered 0.5 mg of testosterone showed a significant increase in action binding effect compared with being administered placebo (participants were tested on two separate sessions, one week apart and the study employed a repeated-measures, double-blind, placebo-controlled design) (Van Der Westhuizen et al., 2017). Thus, testosterone, a steroid hormone associated with power and dominance, might enhance the sense of agency and, by extension, the temporal dimension of action-effect perception that has been proposed to reflect it. Secondly, being temporarily under the influence of substances could increase the magnitude of binding. In particular, being administered Ketamine leads to stronger total binding effect, driven by action binding (Moore et al., 2011). Therefore, ketamine administration in healthy volunteers produces aberrant bodily experiences associated with schizophrenia, which causes these individuals to rely on external sensory cues and thus atypically over-bind the action towards its outcome (Franck et

al., 2005; Haggard et al., 2003). Ethanol administration i.e., being under the influence of alcohol also leads to stronger temporal binding (De Pirro et al., 2020). The authors noted that the effects of alcohol observed were similar to those of dopaminergic medications in Moore et al. (2010). Since alcohol increases dopamine levels in the meso-striatal dopamine system by inhibiting GABAergic and instead disinhibiting dopaminergic neurons, being under the influence of alcohol increases the learning association between action and its outcome and ultimately the magnitude of binding. Whilst the mechanisms underlie the influence of ketamine and dopamine on temporal binding are different, they both boost the temporal binding effect to a perhaps an excessive extent that causes an abnormal experience of agency. Finally, Driller et al. (2023) found that having local anaesthesia applied to index finger enhanced the temporal binding effect. In this study, it is indisputable that participants were still well aware that a) they were the authors of the outcome and b) they could still control their bodily movements. As a result, this finding pinpoints the importance of body ownership that tactile sensory feedback is not a prerequisite for the stable temporal binding effect to occur. Furthermore, whilst the findings appeared counterintuitive, enhanced temporal binding effect under local anaesthesia could reflect the compensatory mechanism of the binding system that helps individuals make sense of the self-generated and its outcome by binding them further together in time.

Altogether, by manipulating the antecedent states, these studies showed how malleable temporal binding effect might be malleable to the contextual cues in a manner broadly consistent with what would be expected if it were sensitive to one's momentary experience of agency. Extreme states particularly those induced pharmacologically could be associated with atypical agency-related experiences and thus might correspond with shifts in binding magnitude. These findings raise the possibility that both markedly reduced and markedly increased binding compared with temporal binding effect shown when participants were not under the pharmacological influence might be associated with disruption to the sense of agency. The temporal binding effect might be an useful lens on the pre-reflective, phenomenally thin experience of agency as it shifts in response to a changing context although this remains an interpretation to be tested further rather than a settled conclusion.

Interpersonal factors

Recalling that we are inherently social species that that we often act in social contexts, which makes our sense of agency susceptible to social external cues. That said, limited studies focussing on how these cues at the interpersonal level moderate one's general sense of

agency using temporal binding effect as a standalone measure. Obhi et al. (2012) emphasised the role of interpersonal power in changing one's magnitude of binding. In this repeated-measured study, after recalling an episode of having low power over others, participants showed a significantly attenuated temporal binding effect compared with no prime or high-power condition (recalling an episode of having high power over others). Their study showed the possibility that feeling low social power over others could reduce the implicit sense of agency. Next, a few studies have shown that negative emotions and states as a result of undesirable *social outcomes* could impair one's sense of agency. Similar to recalling a depressive episode (Obhi et al., 2013), being in a state of heightened psychosocial stress also reduces the sense of agency as measured via the temporal binding effect (Edwards et al., 2025). Participants in this study were either assigned to psychosocial stress condition whereby they were in a stimulation interview for their dream job or the control condition. The former group showed significant reduction in the magnitude of temporal binding post-stress induced compared with pre-stress induction, whilst the control group showed no changes in binding. Likewise, recalling an episode of being socially excluded also leads to an attenuated temporal binding effect (Malik & Obhi, 2019). Interestingly, this reduction of temporal binding effect as a result of social exclusion extends to vicarious ostracism that merely observing someone else being socially excluded also leads to reduction in magnitude of binding (Y. Sun et al., 2023). Taken together, negative affect derived from social experience, including psychosocial stress and social exclusion (both self-experienced and observed) could be associated with a loss of control and thus putatively impair the implicit sense of agency.

One notable study, perhaps the only to date to utilise the temporal binding effect as a predictor rather than the outcome, is by Spaccasassi et al. (2023). In this study, participants first completed the interval estimation task before any experimental manipulation. Participants then watched 36 short videos with three different scenarios: Intentional Harm (the agent in the video deliberately harmed victim) v. Accidental Harm (agent unintentionally harmed victim) v. Neutral (no harm occurred). The authors tested if magnitude of the temporal binding effect would predict how one would make moral judgment about these scenarios, that is, whether or not what the observed agent in the video had done was permissible. The results suggested that participants with stronger temporal binding effect were more likely to view accidental harms caused to others as morally impermissible. The authors proposed that we use knowledge about ourselves to infer the mental states of others (i.e., theory of mind). Thus, feeling more morally responsible for one's own actions would increase the likelihood to adopt similar moral principles when judging the behaviours of other

people. This could open ways to new directions of research wherein the temporal binding effect as an index of implicit agency could also reflect the sense of self-responsibility. Such pre-reflective phenomenon might help us understand actions of others and navigate the social world that we live in.

Methodological assessment

We will have some assessment over possible relevance of the methods used (e.g. type of binding task, context) in the studies reviewed above. Of all the studies reviewed, interval estimation and Libet clock were used most widely. As having reviewed in Chapter 1, the Libet clock task separates binding into action and tone (or effect) component whereas interval estimation typically produces a single global interval judgement. This difference in what each task could detect might explain some apparent inconsistencies in the literature rather than reflecting true differences between conditions. For example, Saito et al. (2017) found that Parkinson's patients showed impaired action binding but preserved tone binding and had they used interval estimation this dissociation might not have emerged. Similarly several personality and trait studies (etc. Aarts & Van Den Bos, 2011; Hascalovitz & Obhi, 2015) reported effects that were specific to tone binding with no change in action binding. This suggests that when a study using interval estimation reports no effect this could reflect either a true absence of any binding effect or a true effect in one component (e.g. action or tone binding) that might have been masked when only a single combined interval judgement is measured. Therefore, while bearing in mind that the Libet clock and interval estimation might not measure the same thing as having reviewed, it is possible that using Libet clock could reveal more insights into which components (action v. tone) might be more relevant when studying the trait/state factors of interest.

Design type might explain the pattern of consistency across areas. Studies of manipulated psychological states (social exclusion, power, stress, moral judgement) used interval estimation in within-subject designs where each participant acted as their own baseline. These studies appeared to report fairly consistent effects in the expected direction. By contrast clinical and personality studies used between-subject designs with the Libet clock to compare separate groups of patients and controls. Between-subject comparisons (albeit necessary especially when it comes to stable trait factors) are vulnerable to individual variability in temporal perception and many other relevant factors (on/off medication, comorbidity), which could partially explain why findings in clinical populations were more mixed (e.g. no group difference in anorexia, nuances in PD findings, see also point below).

Finally, related to point above, context and subgroup specificity also matter as several studies show that binding effects depend heavily on additional contextual factors within the same clinical population. In Parkinson's disease, temporal binding was stronger on medication than off medication (J. W. Moore et al., 2010) and was only altered in patients with comorbid impulsive-compulsive behaviours not in Parkinson's patients generally (Ricciardi et al., 2017). Regarding narcissistic personality, while Hascalovitz and Obhi (2015) found that those scoring higher on narcissistic trait had stronger tone binding compared with their counterpart scoring lower on the scale, Render and Jansen (2019) found that individuals with high vulnerable narcissism showed weaker action binding effect compared with those who scored lower on vulnerable narcissism. Grandiose narcissism did not appear to moderate temporal binding magnitude. Thus it is important to investigate the subgroups and subsets of the population and variables of interest as they might each modulate temporal binding effect differently.

Caveats

It is worth noting that whilst the studies reviewed here did not manipulate action-outcome pairs and look at trial-by-trial differences and that the current thesis adopts similar approaches, the Chapter 5 (Experiment 7) of this thesis did entail trial-by-trial manipulation. In particular, Experiment 7 looks at how temporal binding effect changes across different trial types (different action choices result in different outcomes). Given that context has already been shown to influence sense of agency and the temporal binding effect, it follows that all experimental conditions of Chapter 5 might be shaped by the alternative trials or possibilities available to participants (obeyed v. disobeyed conditions) and are thus likely evaluated in relation to one another rather than in isolation. As a result, it would be inconsistent with the logic developed throughout in the review above and other chapters of the thesis to treat either condition in chapter 5 as reflecting a default or normal trait-like level of temporal binding.

Crucially, the approach that the studies reviewed here and the current thesis (apart from Chapter 5) adopted which attempts to hold the action-effect pairing constant by not manipulating action or outcome elements has inherent issues. Firstly, we must acknowledge the problem of going from effects seen at group-level to assuming they will map onto individual differences or what Hedge et al. (2020) referred to as the 'reliability paradox'. Hedge et al. (2020) demonstrated that tasks capable of producing robust and replicable group-level effects (e.g., the Stroop effect) could nonetheless show poor test-retest

reliability when used to index individual differences. This is due to how the properties that make a task reliable at the group level i.e., a strong and consistent effect present in almost all participants work against the between-subject variability that individual differences research depends on. Using the Stroop task as an illustration, they showed that even a test-retest reliability considered 'good' (ICC = .64) was sufficient to misclassify around 28% of individuals between the top and bottom half of the distribution across two testing sessions and would require sample sizes roughly 2.5 times larger than if measurement were noise-free in order to reliably detect a true underlying correlation with an external variable such as a clinical or trait measure.

The temporal binding paradigms which the studies reviewed here main used, namely interval estimation and the Libet clock, were developed largely on the basis of their capacity to produce robust group-level effects rather than any formal assessment of their psychometric suitability for individual differences work. Thus the reliability paradox raises the possibility that these measures might be similarly poorly suited to indexing stable trait-like individual differences in temporal binding even where the underlying group-level effect is well replicated. This concern is reinforced by Eisenberg et al. (2019) who found that across a large battery of 37 behavioural tasks and 22 self-report surveys task-derived measures showed substantially lower test-retest reliability than survey measures (mean ICC = 0.45 v. 0.80 respectively). Therefore even reliable and factor-analytically derived task scores were largely unable to predict real-world outcomes. As reviewed in Chapter 1, the limited existing psychometric evidence available for temporal binding measures also shows this pattern. Siebertz and Jansen (2022) and De Azevedo et al. (2025) both found no significant correlation between temporal binding scores derived from the interval estimation/interval reproduction and Libet clock paradigms. In particular, individuals showing strong binding on one task were no more likely to show strong binding on the other. Crucially, De Azevedo et al. (2025) further showed that although participants showed consistent temporal binding within a given session, the magnitude of their binding effect often changed from one testing session to another. Altogether this suggests that the well-replicated group-level temporal binding effect reported throughout the studies reviewed here might not straightforwardly translate into a reliable or valid index of individual differences. This has important implications both for the interpretation of prior work linking temporal binding to individual traits/states and for the current thesis's own reliance on these binding measures (and how different binding tasks also used interchangeably in this thesis) for individual-difference

interpretation. This issue will be revisited in the General Discussion regarding its implications for interpreting the thesis's findings.

Secondly, a related issue is that no single measure of temporal binding can be judged in isolation because its value is always relative rather than absolute. Most obviously, the task version used will itself affect the temporal binding value obtained because studies use different action-outcome intervals. This means a given score is only meaningful in relation to the specific task that produced it and thus cannot be reliably compared across studies using different designs. More importantly, to establish whether specific magnitude of binding effect as being lower or higher than 'usual' this requires having some clear expectation to compare it against: either a theoretical prediction of what the value 'should' be or an empirical estimate of what it would 'normally' be based on a large enough sample. Without this kind of benchmark it is therefore challenging to say whether an individual's score reflects genuine atypicality or just ordinary variation. This idea is related to work on normative modelling in psychiatry and neuroscience (Marquand et al., 2019). Marquand et al. (2019) propose that judgements about whether someone deviates from expected functioning only make sense once a normative reference has been built from a large and representative sample (similar to a paediatric growth chart) and once the expected range of variation is properly estimated. Since no such normative data exist for temporal binding measures, any claim that a person or group shows unusually strong or weak binding is really only a comparison against the small sample used in that particular study rather than against any established population reference. This adds to the reliability concerns raised above since a measure that is both unreliable at the individual level and lacking any normative reference point is doubly limited in what it can tell us about individual differences.

Concluding remarks

The studies reviewed suggest that the temporal binding effect is sensitive to a wide range of higher-level psychological factors, spanning from stable individual differences such as clinical conditions, personality traits, age and beliefs to temporary states such as sexual arousal, depressive states, learned helplessness or states that are pharmacologically induced; as well as interpersonal factors such as social power and exclusion; none of which involve any manipulation of the action-effect pairing itself. This convergence across otherwise very different manipulations and populations is consistent with the possibility that temporal binding might reflect a general, pre-reflective sense of agency shaped by top-down cognitive

and social factors rather than arising solely from the local bottom-up sensorimotor features of a given action-effect pair.

However, as the methodological assessment and caveats above make clear, this remains an interpretation rather than an established conclusion. The evidence reviewed here is predominantly correlational because the link between any given trait, state or condition and the construct of agency is itself putative and the reliability paradox raises genuine doubt about whether group-level effects, however consistently replicated, translate into a measure suitable for characterising stable differences between individuals. Related, in the absence of any normative reference for what a typical binding value should be, claims that a particular group or individual shows atypically strong or weak binding can only be evaluated relative to the specific sample and task version used but not against any established population benchmark. These limitations do not undermine the value of the studies reviewed but they do call for caution in how their findings are interpreted and extended. With this in mind, the present thesis adopts a similarly non-manipulated approach to the action-effect pairing across its empirical chapters (with the exception of Chapter 5 as noted above). We treat temporal binding not as a settled index capable of classifying individuals but as one useful, imperfect and still-developing lens through which the antecedents of the implicit sense of agency can be examined. The reliability and normative concerns raised here are returned to in the General Discussion in light of the thesis's own findings.

Overview of the thesis

This thesis aims at understanding the malleability of sense of agency. Notably, the psychological backgrounds that precede the temporal binding effect are manipulated to understand how moderating the antecedents would influence the magnitude of the temporal binding effect. In the current thesis, these factors were mapped onto three levels of analysis, namely personal, interpersonal and socio-political levels.

At the personal level, both a) states and b) the individual difference factors i.e., long-term traits and beliefs are investigated. Chapter 3 focusses on how modulating the prior states in which participants were in could influence the subsequently measure of temporal binding by introducing disruption to motor control. Specifically, throughout four experiments (two pilot experiments and Experiment 1 & 2), the motor control tasks which participants perform could have a high degree of disturbances in the movement trajectories, leading to sensorimotor control disruption. This allows the examination of whether engaging motor control task with high level of perturbances would moderate the subsequently measured temporal binding effect. Chapter 4 delves into the many individual differences factors that pertain to personality, traits and beliefs alongside certain states-level variables such as mood. The first two experiments of Chapter 4 (Experiment 3 and 4) establish the correlational links between individual difference factors and the magnitude of the temporal binding across participants. The results from the first two experiments show that *satisfaction with life* as the stable trait and level of *positive mood* reported as the temporary state factor seem to be correlated with the magnitude of the temporal binding. The following two experiments (Experiment 5 and 6) aim to establish causal relationships between these variables and the temporal binding effect. In particular, in two separate experiments, participants' level of life satisfaction (Experiment 5) and positive mood (Experiment 6) would be manipulated to help understand whether changes in these trait and state would lead to subsequently changes in the temporal binding effect as the marker of implicit agency.

At the interpersonal level, Chapter 5 (Experiment 7) investigates whether being coerced to harm others influences the temporal binding effect. Specifically, the agent-experimenter relationship would be a primary variable of interest as the chapter focusses on whether identifying with the experimenter could modulate sense of agency. Alongside agency, factors such as attentional control, cognitive conflict processing, prosocial

disobedience, empathy and responsibility in the coercive and morally charged context would also be investigated utilising both behavioural and neuroimaging methods (EEG).

Lastly, at the socio-political level, Chapter 6 (Experiment 8) maps the sense of agency onto the broadest spectrum of control i.e., the social-political level. In this chapter, the primary goal is to understand how individuals report perceived level of political efficacy during the US Presidential Election and importantly, how individuals experience the sense of agency as measured both via temporal binding effect and explicit agency in response to such a milestone political event. Since the election outcome is often decided by voters in the particular states, participants were recruited based on which residential states they resided in. Which candidate they voted for was also a crucial independent variable to be examined with its interaction with state group categories. Political efficacy was measure before the election outcome was announced and explicit political agency (how much participants felt like they had personally influenced the presidential outcome) was measured after the election. Temporal binding was measured both before and after the announcement of presidential outcome to assess whether such electoral outcome would result in changes in the level of implicit sense of agency.

CHAPTER 3

Personal Control: Sensorimotor Disruption and the Sense of Agency

The experience of losing control (i.e., control deprivation) has long been recognised as a critical psychological state, often resulting in cognitive, motivational, and emotional impairments that form the core of learned helplessness (LH) syndrome (Maier & Seligman, 1976; Seligman, 1975). At the heart of this phenomenon lies a disrupted connection between action and outcome. When individuals repeatedly encounter situations in which their behaviours fail to yield expected consequences, their ability to perceive themselves as agents, that is, as being capable of causing change in their environment, erodes. This learned helplessness is detrimental because it creates a generalised expectation of absence of control, a loss of human agency and thus one would be unable to effectively cope with newly encountered challenges. Such perception is thus central to experience of sense of agency as reviewed in Chapter 1 (See Figure 2) by work of (Seow et al., 2021; Stephan et al., 2016). As one of the studies reviewed in the introduction Chapter 2 that focus on the state factors at the personal level of control, Soral and colleagues (2021) had shed light into how prolonged control deprivation impaired the implicit sense of agency measured via the temporal binding effect. By using a behavioural helplessness training paradigm whereby the task was objectively unsolvable (whilst participants were told otherwise), the authors exposed a group of participants to action-outcome non-contingency. Afterwards, participants' implicit sense of agency manifested in temporal binding effect was measured, notably in a subsequent block i.e., not related to the helplessness task itself. The results suggested that participants in the helplessness group, after the prolonged period of control deprivation, showed weaker magnitude of the temporal binding effect (which was measured separately) compared with the control group who did not perform the unsolvable task. Furthermore, the longer this control-deprived episode, the lower the temporal binding effect participants of the control deprivation group exhibited.

Soral et al. (2021) contributes significantly to the understanding of the psychological antecedents of the temporal binding effect as the proxy for implicit sense of agency, that is, how negative human experiences and states of mind could impair the time course of unrelated and subsequent goal-oriented activities and outcomes. Their study showed that the temporal binding effect can be influenced by preceding experiences, suggesting it is sensitive to broader psychological states rather than solely to the immediate action-outcome relationship being measured. In contrast, as having been reviewed in earlier chapter, the literature on temporal binding often revolves around the temporal binding between an action and outcome on which the experimental manipulation was directly implemented, but not on the temporal binding being measured separately.

This chapter expanded this finding by focussing on the deprivation of sensorimotor control. It could be argued that a loss in sensorimotor control is different from which of cognitive control (BHT task in Soral et al. (2021) was a cognitive decision-making task). Meanwhile, impairing sensorimotor control movements might influence individuals differently, considering sensorimotor control requires more bottom-up and sensory inputs than executive-cognitive control (S. H. Scott, 2016). That being said, within the broader context of psychological studies on bottom-up embodied cognition, for example, it has been shown that the perceived steepness of hills and distances of landmarks, estimates of jumping height, and even selection of target versus non-target objects, can all be influenced by the presumed efforts and limitations of the motor system (Craighero, 2022; Gallivan et al., 2018). Therefore, it makes sense that if the motor system falls apart, this would influence other high-order processes, including sense of agency (Li & Lindenberger, 2002). We expected that a period of sensorimotor deprivation might lead to similar subsequently impaired temporal binding as in Soral et al. (2021).

Regarding the possible mechanisms of how disruption in motor system might impairs agency, based on the comparator model (reviewed in Chapter 1), the experience of agency is influenced greatly by brain's ability to predict the outcomes of actions via internal forward models. When the outcomes do not match with these predictions, the individuals would be likely to experience a loss of agency and thus feel as if they are not in control of their actions (Gentsch & Schütz-Bosbach, 2015). Thus, if the absence of agency is the result of being unable to control what one would normally would, we expect that introducing disruptions to a simple task which normally would cause little or hardly any difficulty in effectively executing the action could interfere with one's sense of agency. For instance, imagine trying to click on somewhere on your computer screen with the mouse, which is an easy and almost automated task that we do not normally think twice about how to execute it. The mouse suddenly moves in its own trajectory, and you lose your ability to control the mouse's movements. This is when your motor system is alerting you of the mismatch between action prediction and outcome, and you no longer experience agency over the mouse. If such sensorimotor perturbance persists, these seemingly easy motor control tasks might induce similar sense of helplessness. Such sensorimotor control deprivation could consequentially impair the implicit sense of agency measured by the temporal binding effect (for instance, patients with Parkinson's disease and their disturbances in sense of agency due to difficulty in action initiation and control of voluntary motor movement (J. W. Moore et al., 2010)).

In this line of motor disruption and sense of agency, Sidarus et al. (2017) adapted a computer game wherein the participants had to move a box along a horizontal bar to catch targets and avoid distractors. Turbulence was introduced to the box movement (participants controlled the box using the cursor) as the movement of the box deviated unpredictably, hence creating action-outcome mismatch and this attenuated the subjective explicit sense of agency. A later study by Wen and Haggard (2020) also introduced disturbances in motor control movement and examined whether such motor disruption affected the detection and attribution of agency. In their study, participants moved a white circle on the screen and aimed to touch a red cross with the white circle by moving the mouse. The motor movements to control the white circle were disrupted that there were disturbances introduced to the trajectory, which resulted in large prediction errors i.e., discrepancies between actions and outcomes. In particular, the movement of the white circle was rotated clockwise by either 90° angular (high disturbance) or 30° angular bias (low disturbance). Thus, in the 90° angular bias linear disturbance condition, the movement of the white circle greatly disturbed the comparator, which therefore resulted in poor motor control. However, the authors found that this severe disturbance left the computation of regularity intact because participants could still detect visibly the regular relationship between the action (i.e., the mouse movement) and the outcome (i.e., the actual position of the white circle). As this regularity detection was preserved in the 90° linear transformation condition, the sense of agency was still high (measured by asking participants to detect out of many white circles, which circle they had just controlled). One of the primary conclusions of the study was that participants were able to experience agency due to the regularity detection irrespective of the large prediction errors between action and outcome.

This chapter of the thesis explores such research with further lines of enquiries. Wen and Haggard (2019) stated that participants were still significantly less accurate at detecting which dot they could control between the 90° and the 30° linear disturbance conditions, which were operationalised to be the sense of agency. The authors thus suggested that regularity detection could not be the sole factor in formulating sense of agency. Indeed, participants' performance was poor in the 90° linear transformation condition, and the authors suggested that the internal model was not adapted as participants did not learn the novel movement-visual feedback mapping. As a result, we argued that with this prolonged large prediction errors, regularity detection would not be sufficient to maintain a robust sense of agency, and such disturbance might induce learned helplessness, which could then impair agency. In this chapter, we adapted Wen and Haggard (2019)'s motor control paradigm but

made the 90° linear disturbance condition even more challenging to induce learned helplessness for sensorimotor control, relatively like in Soral et al. (2021). To do so, for every trial of the high-disturbance motor block, the white circle which participants had to control had its trajectory was rotated clockwise by a random angular value between 90-270° relative to the actual mouse trajectory (note that this angular value for each trial remain the same throughout the whole trial and only changed for the next).

Additionally, the agency measured in Wen and Haggard (2020) was based on accuracy of detection as participants were asked if they could detect which dot they felt that they could control, rather than how control they felt, or the measure of implicit agency i.e., temporal binding. Therefore, a natural extension of their finding would be to understand how such perturbance in motor control influence the temporal binding effect as an index of implicit sense of agency.

Pilot experiments

Two preliminary pilot experiments (each with N = 41) were conducted to establish whether after having engaged in a motor task with high level of sensorimotor control deprivation would reduce the subsequently measured temporal binding effect. We adapted the reaching task of Wen and Haggard (2019) and compared if the white circle moved in 90-270° and the 30° linear disturbances would produce comparable level of the temporal binding effect – we argued that this would not be the case. Considering the large prediction errors between the two linear conditions, and in line with Soral et al. (2021), it was expected that prolonged period of motor disturbance in 90-270° motor task would detrimentally decrease the temporal binding effect. Meanwhile, performing the 30° linear disturbance task would not have an impact on the subsequentially and independently measured magnitude of temporal binding.

To test this, the two pilot experiments employed a within-subject block A-B-A-B-A design. Participants first completed the interval reproduction task (A) to establish temporal binding baseline. They then performed a block of motor control task (B), followed by another block of the interval reproduction task to measure temporal binding (A) after the motor task and so on. In Pilot 1, participants completed a 30° motor control block and a 90-270° motor control block. In Pilot 2, participants completed a full-control motor block (i.e., no angular rotation) and a 90-270° motor control block. Contrary to expectations, results from the two pilot experiments all showed that both temporal binding effects following the 90-270° angular disturbance and 30°/full-control motor task reduced significantly compared with the

baseline binding effect; and that the magnitude of the temporal binding effects following each motor control block did not differ from one another. In other words, temporal binding effect appeared to simply reduce over time.

Notably, we conducted further analyses to explore whether the order in which participants performed the motor tasks influenced their temporal binding effect following the end of each motor task. Here, we found interesting patterns. Although the interaction effects between motor task order and temporal binding magnitude were not significant, likely due to limited sample size, in both pilot experiments, we consistently found that participants who started with the 90-270° first had their magnitude temporal binding effect sustain over time. Meanwhile, participants starting with the easy version of the motor task showed significantly reduced binding after performing the 90-270° highly disturbance motor task in relation to baseline binding (For full reports of the two pilot experiments, see Appendix B).

These unanticipated findings led us to consider whether the motor task order in which participants started with might account for how temporal binding effect unfolded following each version of the motor task. Because the two pilot experiments were not sufficiently powered for the task order as the between-subject factor in the analysis, Experiment 1 was sufficiently sampled to test this hypothesis of 'being thrown in the deep end' i.e., whether starting with the difficult version of the motor control task first preserved the sense of agency. Moreover, in lieu of the interval reproduction paradigm, Experiment 1 employed the Libet Clock paradigm to measure the temporal binding effect. Libet Clock allowed us to quantify action binding (i.e., how later in time the action is perceived to occur) v. outcome binding (i.e., how earlier in time the outcome is perceived to occur) separately as reviewed in previous chapters. Results from Experiment 1 confirmed this hypothesis: starting with 90-270° disruptive motor control task helped preserve the magnitude of action binding throughout the experiment.

Since temporal binding following the motor control task was only measured once in the first experiment, Experiment 2 utilised many alternative blocks of motor control – interval reproduction task to assess how the temporal binding effect was timely affected by the nature of the motor task over the course of time. We found that repetitively performing the easy motor task also attenuated the temporal binding effect, highlighting the detrimental effect of monotonous task repetition. Altogether, these showed the intricate relationship between sensorimotor control and the implicit sense of agency.

Experiment 1

In this experiment, we aimed at addressing a few lines of inquiry. Firstly, we now included the Block Order (starting block with the Full-Control motor task v. starting with the 90-270° motor task) as the between-subject independent variable to test the hypothesis stemming from the two pilot experiments' findings. This would allow us to assess the 'being thrown in the deep end' notion whereby starting with a difficult task might play a role in keeping the implicit sense of agency intact. Meanwhile, for individuals performing the easy motor control task first, such block order would make it too challenging to adapt to the new and greatly disruptive motor control variant. Secondly, the two pilot experiments showed the tendency of temporal binding effect to attenuate in the latter half of the experiment. That is, temporal binding measured using the interval reproduction paradigm decreases from the first to the second half of the experiment, possibly due to participants becoming better at replicating the intervals (Silver et al., 2025). For such reason, we utilised the Libet clock paradigm here to measure temporal binding effect as there has not yet been presence of evidence showing similar attenuation of temporal binding measured using Libet clock. Additionally, Solet et al. (2021) found that there was no evidence of fatigue or temporal learning in the binding effect measured by Libet task over time.

A further advantage of the Libet Clock task was the possibility to disentangle action and effect binding since these could be driven by separate mechanisms. To recap, action binding refers to the percentual shift in the time of action towards the outcome and the tone binding reflects the shift of the outcome towards to action that has caused it (Yoshie & Haggard, 2013). We could thus quantify the contributions of action shift and tone shift to the effects of sensorimotor disruption and/or adaptation, depending on the motor control task order, on the subsequent sense of agency. Based on the pilot findings that starting with the difficult version of the motor control task first preserved the sense of agency, our main hypothesis was that for participants in the Starting with 90-270°, there would be no statistically significant difference in magnitude of temporal binding effect following each motor block. Meanwhile, it was expected that participants starting with the easy full-control motor task first would have significantly weaker temporal binding after performing the difficult version than binding after performing the easy motor task. No specific predictions were made about whether motor task order would affect action binding and tone binding differently. This was because the pilot study used an interval reproduction method, which gives a single overall score for temporal binding rather than separate action and tone

estimates. We acknowledge this meant that the pilot findings could not be used as a straightforward basis for predicting what would happen on the Libet clock measures thus we made no strong directional predictions in advance regarding differentiating action v. tone binding.

Method

Participants

A priori power analysis was conducted using G*Power (Faul et al., 2009) to determine the needed sample size for a mixed ANOVA with a two-level within-subject factor (action/tone binding after full control task v. after 90-270°) and a between-subject factor (Starting with full motor control v. Starting with 90-270° motor block). The power calculation targeted the Group × Condition interaction effect as this was the key effect of interest. For the conventional medium effect size f =.25 (J. Cohen, 2013) with α =.05, power =.80, a sample of 84 was required. It is worth noting that the assumed effect size was not derived from the pilot experiments or previous studies and thus the true effect size might differ. If the true effect is smaller than medium, the study might be underpowered to reliably detect it which risks overestimation of effects that reach significance (Brysbaert, 2019). Furthermore, while the power calculation was conducted for the mixed ANOVA interaction, the primary analyses employed mixed-effects models and this deviated from the planned ANOVA analyses. Power estimation for specific predictors within mixed-effects models is considerably more complex than for omnibus ANOVA tests and cannot be directly derived from the G*Power calculation reported here (Brysbaert, 2019). Therefore we acknowledge that the study might be underpowered for some of the mixed-effects model estimates and that the observed interaction effects may not be reliably detected based on this power analysis alone. The G*Power calculation should therefore be understood as an approximation and all results are interpreted with this caveat in mind.

86 participants were recruited (*M age* = 24.64, *SD* = 7.66, 23 Men, 1 Non-binary) who were compensated with either course credits or payment of £12/hour. Participants were randomly assigned to starting with full control or starting with 90-270° first motor control task, with 42 participants (*M age* = 24.10, *SD* = 7.47, 15 Men, 1 Non-binary) in the 90-270° First and 44 participants (*M age* = 25.16, *SD* = 7.98, 8 Men) in the Full-Control First condition (See Table 3 for more demographic characteristics). All experiments in this study were granted ethical approval by the School of Psychology Ethics Committee at the University of East Anglia.

Table 3.

Participant demographic characteristics of Experiment 1(N = 86)

Variable	Category	n	%
Household income	Less than £20,000	21	24.4
	£20,000-£39,999	16	18.6
	£40,000-£59,999	12	14
	£60,000-£79,999	7	8.1
	£80,000-£99,999	6	7
	£100,000+	6	7
	Prefer not to say	18	20.9
Ethnicity	White British	45	52.3
	Other White background	8	9.3
	African	4	4.7
	Caribbean	1	1.2
	Chinese	2	2.3
	Another Asian background	10	11.6
	Mixed/Multiple	9	10.5
	Other	7	8.1
Religion	Christianity	19	22.1
	Islam	2	2.3
	Hinduism	2	2.3
	Buddhism	1	1.2
	Atheism	36	41.9
	Agnosticism	17	19.8
	Other	9	10.4

Importantly, for all confirmatory and exploratory analyses in this experiment, the final sample excluded two participants with extreme outliers for temporal binding effect, thus final N = 84 (See data exclusion of Results for details about the exclusion criteria). This is the dataset to be used for all analyses of this experiment.

Materials

Our laboratory-based experiment was programmed using PsychoPy (v2023) coded using Python to manipulate the intensity of disruption in motor control, and to measure the temporal binding effect using the Libet Clock. The experimental tasks were conducted with a Dell 1909W monitor (the experiment was run full-screen, size of the monitor was 19 in. with 1440 x 900 resolution and refresh rate at 60 Hz), a keyboard, and a computer mouse.

Motor Control Task

We adopted the Motor Control Task paradigm from Wen and Haggard (2020) which was designed to measure prediction errors in each experimental condition when people attempted to move the stimulus. In this task, participants started moving the mouse which activated a white circle 12.5-mm (45 pixels) to appear on the screen. This white circle moved relatively in accordance with the mouse movement executed by the participants. However, while the onset, offset, and velocity of the motion of the circle corresponded to the mouse movement, the trajectory was distorted by different degrees of disturbances (Figure 3A). In the full-control condition, there was no disturbance to the trajectory of the circle at all. Thus, in the full-control condition, participants had full control over the white circle whose movements were identical to how the participants controlled the mouse. Meanwhile, in the 90-270° condition, the motion of the white circle was distorted so that the circle moved clockwise by a random angle ranging between 90° and 270° compared with the corresponding mouse movement made by a participant. This randomised angular value between 90 to 270 remained constant during the entire trial and only changed in the next trial. That is, if the angular value was 90° for a trial in this block, the circle would rotate by 90° for the entire trial.

Thus, participants had absolute sensorimotor control over the white circle in the full-control condition, whereas the 90-270° angular clockwise transformation would cause great level of perturbation in mismatch between mouse movements and the actual position of the white circle. The goal of the task was that participants must aim to control the white circle so that it touched a red cross on the screen. The 9.0-mm (38 pixels) red cross was randomly presented at one out of five possible locations for each trial: left/right top corners, left/right bottom corners, or the centre of the monitor.

Ideally, to succeed in the 90-270° angular bias task, participants should adjust their reach angle to compensate for the bias, that is, to counteract an angular clockwise transformation of θ°, participants should move the mouse θ° counterclockwise from the target direction (θ could be randomly between 90° and 270°, and this value remained constant for each trial and only randomised trial-on-trial basis). For example, if the red cross appeared in the top-left corner of the screen, and the white circle's movement was rotated 90° clockwise, participants would need to move the mouse toward the bottom-left (i.e., 90° counterclockwise from the target) for the circle to reach the cross (See Figure 3B). This type

of adjustment would be extremely disorienting, especially when the rotation was randomised per trial, and would demand strong visuomotor adaptation.

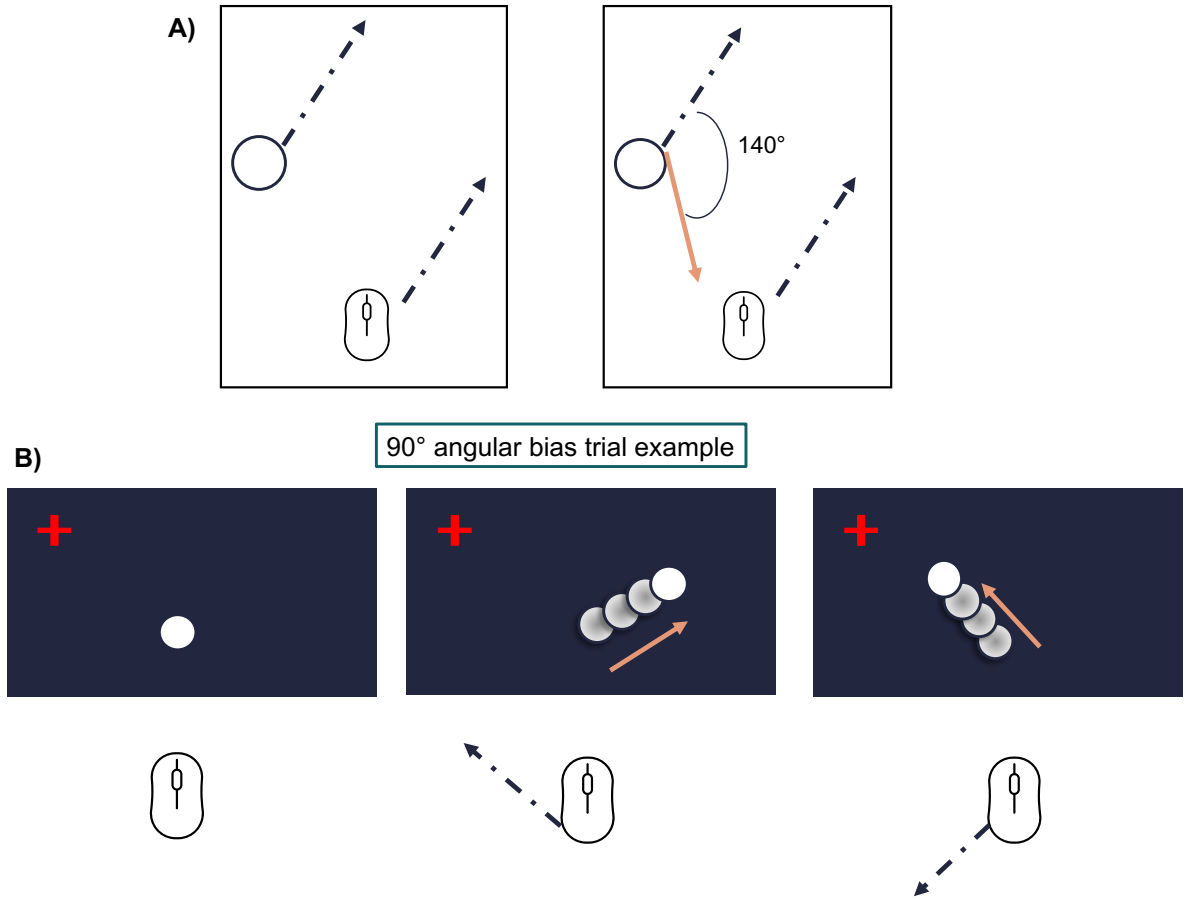


Figure 3. A) Illustration of the angular clockwise transformation of the white circle compared with the actual mouse movement in the full-control condition (left) and in the 90-270° angular bias condition (right). The dashed arrows in black represent the mouse movement trajectories, while the orange arrows represent the clockwise angular bias rotation of such trajectories in which the actual position of the white circle would end up for each movement. B) Example of a trial in the 90-270° angular bias condition wherein the angular bias was 90°. The cross appeared in the top-left corner of the screen, and the white circle's movement was rotated 90° clockwise. Participants would need to move the mouse toward the bottom-left (i.e., 90° counterclockwise from the target) for the circle to reach the cross.

Libet Clock

To measure the temporal binding effect, we employed Libet Clock and followed the similar paradigm by Haggard et al. (2002) and used similar Python script provided by Siebertz and Jansen (2022). The task was comprised of four conditions, with two termed

Operant (Operant Action and Operant Tone) and the other two Baseline conditions (Baseline Action and Baseline Tone). The Libet Clock task involved a clock face with a clock hand rotating in a clockwise direction, at the speed of 2560ms/full rotation (See Figure 4). The clock face circle had a diameter of 9.5° and the clock hand was 4.1° in length. The tone used was a sine wave tone with 100ms duration at 1000Hz.

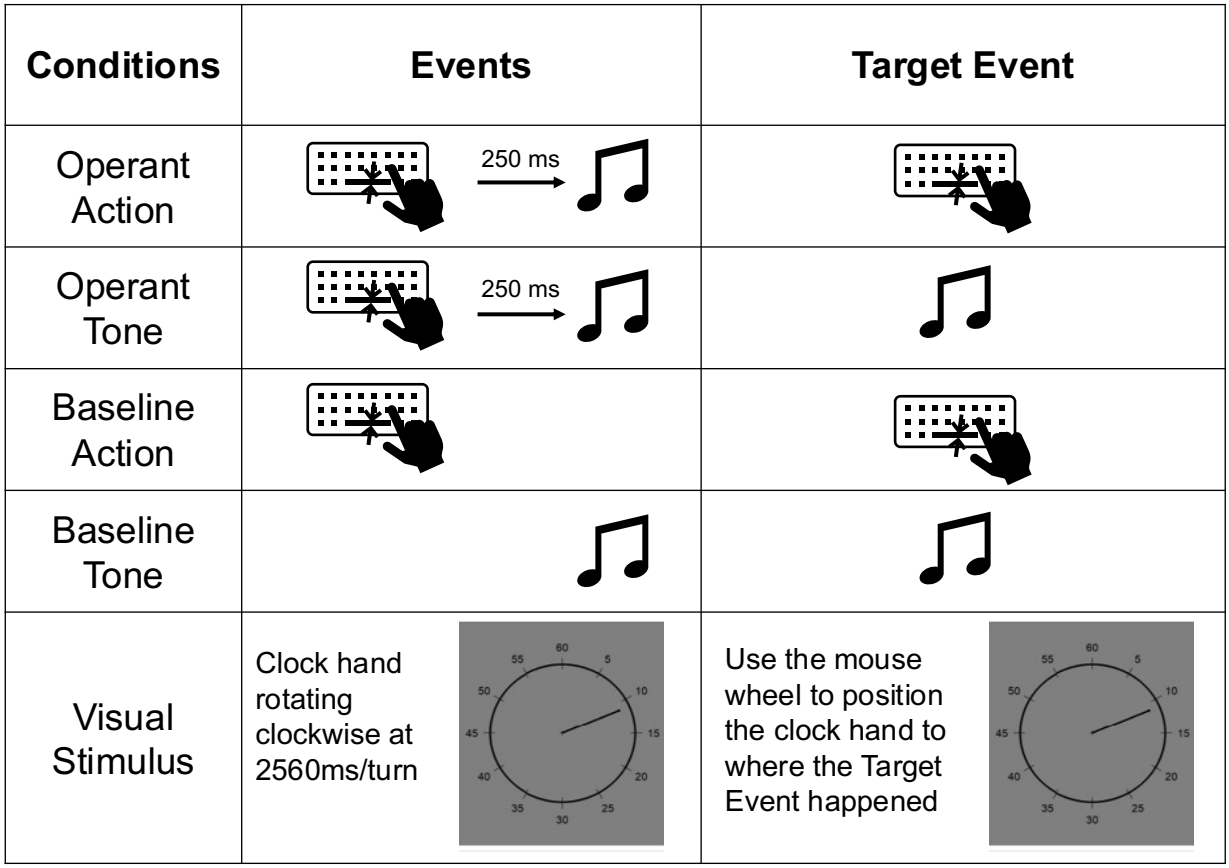


Figure 4. Illustration of the Libet Clock paradigm adapted from Siebertz and Jansen (2022).

Design

In this experiment, our primary between-subject independent variable was the Block Order with two levels: starting with full control v. starting with 90-270° angular bias motor control task. If participants started with the full-control motor task first, they would then complete a Libet Clock block to measure the temporal binding effect, which was then followed by the block of 90-270° motor control task, and finally they would complete another block of Libet Clock task to measure binding. This design would be similar for their counterpart who started with the 90-270° motor control task first, except for that the order of the motor task was switched around. Thus, all participants completed the experiment was in

blocks of A-B-A-B whereby the motor control task started first, followed by a block of Libet Clock task and so on (Figure 5).

The two blocks of Libet Clock were identical throughout the experiment. For each block of the Libet Clock task itself, there were four sub-blocks (Baseline Action, Baseline Tone, Operant Action, Operant Tone). These were presented in a pseudorandomised approach using a 4x4 Latin Square where each possible sequence was presented for across all participants. Using the Libet Clock, we could calculate the action binding and tone binding effects separately, thus the dependent variables were action binding after full-control motor task, action binding after 90-270° motor control task, tone binding after full-control motor task and tone binding after 90-270° motor task. We would also calculate a single dependent variable which would be the overall temporal binding score by combining action and tone binding together.

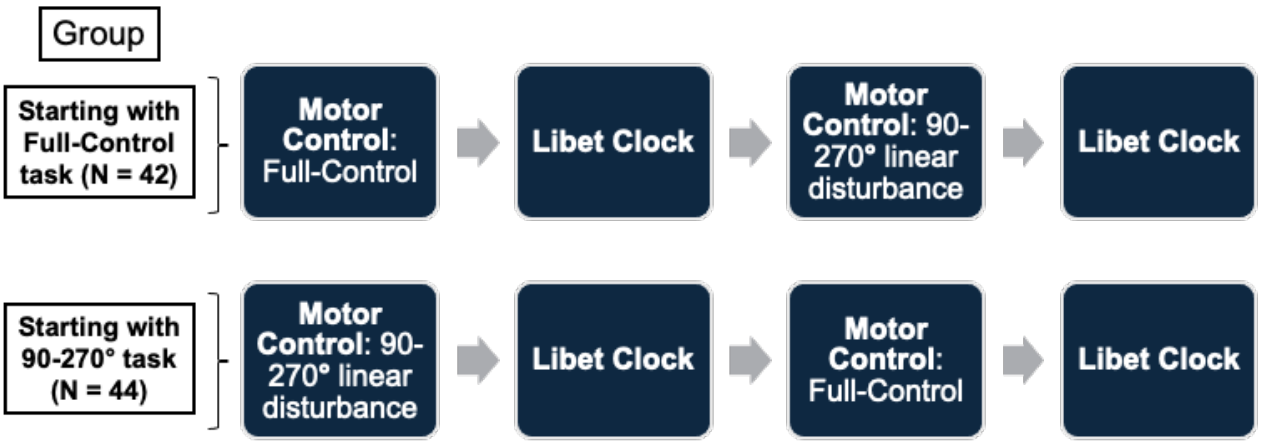


Figure 5. Outline of the experimental design of Experiment 1.

Procedure

Participants signed up for the experiment via SONA system or the University of East Anglia's paid panel. On the day of the experiment, they were first presented with the information sheet that briefly described the purpose of the experiment and how their data was handled. The researcher then verbally explained the structure of the entire experiment (A-B-A-B-A format, whereby A was the Motor Control task and B was the Libet Clock task, see Figure 5) and what needed to be done for each of these tasks. Participants were randomly assigned to either start with the full-control or the 90-270° motor control block, which was followed by a block of Libet Clock; and then a block of motor control task again (the condition which they had yet done) and ended with the last block of Libet Clock.

For the motor control task instructions, participants were informed that at some point, the white circle would move differently from how they moved the mouse. It was explained that this was all intended, rather than there were any fabrications or technical issues with the mouse itself. Participants then commenced the motor control task. Participants were reminded again of the primary goal of this task, which was to control the white circle using the computer mouse so that it touched the red cross within 10s. Once having aligned the circle in the centre of the cross and double-clicking the mouse to lock the circle's position, participants would have a successful trial, and they were informed so: 'Well done! You've reached the cross' before the next trial commenced. On the other hand, if the white circle was not placed in the centre of the cross within 10s; the trial was deemed unsuccessful and feedback was also provided 'Oops, time's up!'.

Participants first had 5 practice trials of the motor-control 'touch the cross' task. During the practice trials, there were no disruptions of the mouse movement i.e., the white circle moved exactly like how the mouse was moved, allowing participants to be accustomed to the task and what a successful trial looked like. It is worth noting the relevance of such practice trials, because it would have meant that all participants had learned first that they could fully control the task and as having mentioned above, all participants were forewarned of an upcoming loss of control to watch out for rather than the sensorimotor disruption being totally unexpected.

During the actual motor control trials, as mentioned above, all participants partook in two experimental conditions with each involving different levels of disturbance. The disturbance conditions were either 90-270° linear disturbance or full-control (control condition). In the former, the circle was spatially rotated clockwise by a trial-by-trial randomised angular degree between 90 and 270° relative to the actual mouse movement, participants had extremely poor control over the stimuli. Meanwhile, in the full-control motor condition, participants had absolute control over the white circle without any angular interference in the movement trajectory of the white circle. Each type of trial was repeated for 40 times as a block, resulted in a total of 80 trials with two corresponding blocks. Fewer successful touches indicated larger prediction errors and poorer execution of motor movement control. The 90-270° motor block took approximately 10 minutes to be completed, the full-control block took shorter amount of time about 5 minutes.

At the end of each motor control block, participants were asked to rate their level of frustration: "On a scale of 1 to 7, where 1=*'Not frustrating at all'* and 7=*'Extremely frustrating',* how would you rate the level of frustration you experienced while performing

the task?" – with higher scores indicating more frustration experienced. We also included an explicit measure of agency by asking individuals to respond to the question 'On a scale of 0 to 100, how much control did you feel over the white circle?' with 0 = '*No control'*, 100 = '*Full Control'* on a slider which did not have a marker at the start and the marker only appeared once the participants had clicked on the scale. The explicit measure of the feeling of being control was asked at the end of each experimental block of the motor task and was displayed below the frustration rating.

For Libet Clock task that followed each of the motor control block, this remained identical throughout the experiment and all participants completed each block of this task, in which there were four sub-blocks: two Operant Blocks (Operant Action and Operant Tone) and two Baseline blocks (Baseline Action and Baseline Tone). At the beginning of each block, we presented the participants with instructions about what to do in the upcoming block and what target event (the spacebar pressing i.e., action or the tone occurring i.e., outcome) was which participants should keep track of.

For the Operant blocks, in each trial, participants were instructed that they would press the space bar which would trigger a 'beep' tone. In the Operant Action block, the target event would be the key press and in the Operant Tone block, the target event was the tone. At the start of each trial, the clock hand started rotating from a random position on the clock face. Participants must wait for the clock hand to rotate a minimum of one full rotation before taking action i.e., pressing the space bar, after which they could press the space bar at will whenever they would like. Importantly, they were told not to press the space bar at a predetermined or a specific clock hand position. After pressing the space bar, there was a delay of 250ms until the tone appeared. The hand clock then continued to rotate for a further randomised interval between 1500ms and 2500ms after the tone occurred. The clock disappeared and after 1000ms, it reappeared again on the screen. The clock hand was stationary in a randomised position (between 45° and 60°, either clockwise or counterclockwise) from the actual hand position of the target event i.e., key press (action) or the tone (outcome). Participants were then asked, by using the mouse wheel, to adjust the clock hand position to wherever on the clock face that they thought the target event had occurred. Once they had placed the clock hand position to where they perceived the target event to have occurred, they were asked to press the space bar to confirm and after 100ms, the next trial started.

The two Baseline blocks, Baseline Action and Baseline Tone, followed similar procedure but in each of these respective blocks, only one event occurred. For trials in the

Baseline Action, participants were instructed to press the space bar, but there would be no Tone in this block. In the Baseline Tone condition, participants were told that they would not have to do anything, and the tone would automatically occur at a random time. Indeed, after the clock hand had rotated a full rotation of 2560ms, from this point onwards, the tone then occurred randomly from 2810ms to 4810ms. Similar to the Operant Blocks, participants were asked to adjust the clock hand position to wherever on the clock face they thought to have pressed the space bar for the Baseline Action block or to when they perceived to have heard the tone for the Baseline Tone block.

There were certain criteria based on which a trial would be deemed invalid and thus repeated to ensure that the participants were performing the task correctly. Firstly, for the two Operant blocks and the Baseline Action blocks, if the participants pressed the space bar before a full rotation of the clock hand from where it initially started, meaning pressing the space bar too early, this would make the trial void, and it would be repeated. Secondly, for these same blocks, if the participants did not take any action i.e., pressing the space bar within 10s after the onset of the trial, this trial became invalid. Lastly, for all four blocks, after being prompted to adjust the clock hand position to where the target event had occurred and the clock hand was not moved by the participants, this trial would also become invalid, and the participants must repeat this trial. There were two practice trials for each of the four blocks, resulting in 8 practice trials. For the main trials, participants completed 15 trials for each block, totalling 60 trials for each of the Libet Clock task. Since participants completed the Libet Clock twice during the experiment, this resulted in a total of 120 trials.

At the end of the experimental blocks, participants were asked to respond to demographic questions including age, gender, household income, ethnicity, and religion. Participants could skip any questions which they did not wish to answer. Having completed the questionnaires, they were debriefed, and course credits or rewards were granted. On average, the entire experiment lasted for 60 minutes.

Results

All mixed-effects models' analyses were conducted using R version 2023.03 with R packages tidyverse (Wickham et al., 2019), lme4 (Bates et al., 2015), lmerTest (Kuznetsova et al., 2017) and plots were created using ggplot2. All other analyses were conducted in SPSS version 31.

Calculating binding from the Libet Clock

Firstly, for each trial, we calculated Judgement of Error (JE) i.e., the temporal difference which was the spatial discrepancy between the actual and estimated hand clock positions of the target event (which could either be the key press or the tone occurring). Thus, if JE > 0; the target event was perceived by the participants to occur later in time than it actually did i.e., a clockwise shift of the clock hand position. Meanwhile, if JE < 0; this means that the target event was subjectively perceived to have happened sooner than it actually did i.e., a counterclockwise shift of the clock hand. For each participant, action binding was calculated by subtracting the mean JE in Baseline Action condition from the mean JE in Operant Action:

$$Action\ Binding = Mean\ JE_{\ Operant\ Action} - \ Mean\ JE_{\ Baseline\ Action}$$

Here, positive values of action binding indicated action binding effect that the key press was perceived by the participants to have occurred later in the Operant Action condition compared with the Baseline Action condition. We also calculated tone binding for each participant by subtracting the mean JE in Baseline Tone condition from the mean JE in Operant Tone:

$$Tone\ Binding = Mean\ JE_{\ Operant\ Tone} - \ Mean\ JE_{\ Baseline\ Tone}$$

With the tone binding effect, this would mean that the tone binding values would be negative since participants would perceive the tone to take place earlier in the Operant Tone condition compared with the Baseline Tone condition. Having calculated Tone Binding and Action Binding, we could then calculate the Total Binding effect:

$$Total\ Binding = (-\ Action\ Binding) + Outcome\ Binding$$

The total binding scores represents the overall sense of binding across both action and outcome, showing how action and tone were bound together closer in time. The more negative the total binding score, the greater implicit sense of agency (Haggard et al., 2002).

Data exclusion

For each participant, we excluded trials where z-scores of JE > ± 3SDs from the mean JE of that participant, resulting in a total of 1.4% trials removed. At participant level, we calculated z-scores for four measures: action binding and tone binding after full-control motor task as well as action binding and tone binding after 90-270° motor task. Participants with z-scores > ± 3SDs for any of the above four mean values of action/tone binding of all participants would be removed. Two participants were excluded with extreme z-scores of at

least one of the four measures above. This leaves the final sample of 84 participants, with 42 participants in each starting order group.

Manipulation check

Basic manipulation checks were conducted to ensure that the manipulation of motor constrains worked as expected. Results showed that in the 90-270° motor control task, participants did indeed report significantly lower explicit control rating and greater level of frustration compared with which of the full-control motor task.

Regarding motor performance i.e., "success" (reaching target), percentage of successful trials wherein the cross was touched by the white circle was significantly lower in the 90-270° motor block compared with which of the full-control motor task (See Table 4). These results suggested that the 90-270° angular disturbance might have induced motor control disruption.

Table 4.

Descriptive means and SE and comparisons in self-reported frustration, control rating and success rate between the motor control conditions

Measure	Condition	Mean	SE	*t*(83)	*p*	*d*
Frustration	Full-control	1.82	0.14	-20.62	<.001	-2.25
	90-270°	5.48	0.15			
Explicit control	Full-control	90.19	1.37	21.31	<.001	2.33
rating	90-270°	31.69	2.79			
Percentage of	Full-control	98.87	0.23	21.92	<.001	2.39
Successful trials (%)	90-270°	32.95	2.99			

Note. All tests were two-tailed.

Further analyses were conducted to assess if Group (Starting with Full-Control task v. Starting with 90-270° motor task) influenced frustration, explicit rating and motor control task performance (See Table 5 for descriptive statistics). We conducted several mixed-effects models with a) Group (Between-subject factor: Starting with Full-Control task v. Starting with 90-270° motor task), b) Condition (full-control motor task v. 90-270° motor task) and c) Group × Condition interaction as the fixed effects and with participant as random effect. In the first model, frustration was entered the model as the outcome variable:

Frustration ~ Group + Condition + Group × Condition + (1 | participant).

Group was treatment-coded with Control as the reference category. Significance was assessed via Satterthwaite's method. Variance inflation factors (VIF) were inspected to assess multicollinearity. VIF values for Group, Condition and their interactions were ≤ 2.65 which suggests no problematic multicollinearity. Results suggest that frustration Condition was a significant fixed effect that Frustration reported in the 90-270° condition (M = 5.48, SE = .15) was significantly greater than which in the full-control condition (M = 1.82, SE = .14) β = 3.43, $t(84) = 13.90$, $p < .001$. We also found a significant main effect of Group such that participants in the Starting with 90-270° motor task group (M = 3.27, SE = .15) reported significantly less feeling frustrated than participants in the Starting with Full-Control task group (M = 4.02, SE = .15) β = -.97, $t(160.95) = -3.52$, $p < .001$. There was no significant interaction effect β = .45, $t(84) = 1.30$, $p = .198$.

In the second model, Explicit Control rating was entered the model as the outcome variable, with similar fixed effects as above, similar Group coding and Satterthwaite's method:

Explicit Control rating ~ Group + Condition + Group × Condition + (1 | participant).

VIF values for Group, Condition and their interactions were ≤ 2.76 which suggests no problematic multicollinearity. Results suggest that frustration Condition was a significant fixed effect that Explicit Control rating reported in the 90-270° condition (M = 31.69, SE =2.79) was significantly less than which in the full-control condition (M = 90.19, SE = 1.37) β = -65.33, $t(84) = -17.60$, $p < .001$. Group did not have a significant main effect β = 7.81, $t(164.98) = 1.96$, $p = .052$. There was a significant interaction effect of Group × Condition β = 13.67, $t(84) = 2.60$, $p = .011$. In particular, the two groups did not differ in explicit control rating for the full-control motor task $t(169) = -1.93$, $p = .055$. However, for the 90-270° condition motor task, the Starting with Full-Control task group (M = 21.0, SE = 2.86) reported significantly less feeling of control than the Starting with 90-270° motor task group (M = 42.4, SE = 2.86) $t(169) = -5.31$, $p < .001$.

Lastly, regarding motor performance i.e., "success" (reaching target), in the third mixed-effects model with similar parameters except for percentage of successful trials being entered as the outcome variable:

% Successful trials ~ Group + Condition + Group × Condition + (1 | participant).

VIF values for Group, Condition and their interactions were ≤ 3 which suggests no problematic multicollinearity. Results suggested that Condition was a significant fixed effect that percentage of successful trials wherein the cross was touched by the white circle was significantly lower in the 90-270° motor block (M = 32.95, SE = 2.99) compared with which of the full-control motor task (M = 98.87, SE = .23) β = -71.55, $t(168)$ = -17.33, p <.001. These results suggested that the 90-270° angular disturbance might have induced motor control disruption as the intended manipulation. The Group fixed effect was not significant β = -.24, $t(168)$ = -.06, p = .954 nor was the interaction effect Group × Condition β = 11.25, $t(168)$ = 1.93, p = .056.

Table 5.

Descriptive means and SE and comparisons in self-reported frustration, control rating and success rate between the motor control conditions and between two Starting order groups

Group	Measure	Condition	Mean	SE
Starting with Full Control task	Frustration	Full-control	2.31	0.20
		90-270°	5.74	0.20
	Explicit control rating	Full-control	86.3	2.86
		90-270°	21.0	2.86
	Percentage of Successful trials (%)	Full-control	99.0	2.95
		90-270°	27.4	2.95
Starting with 90-270° task	Frustration	Full-control	1.33	0.20
		90-270°	5.21	0.20
	Explicit control rating	Full-control	94.1	2.86
		90-270°	42.4	2.86
	Percentage of Successful trials (%)	Full-control	98.80	2.95
		90-270°	38.5	2.95

Overall, the manipulation checks confirmed that the 90-270° angular disturbance successfully disrupted motor control, as shown by significantly lower success rates, reduced explicit control ratings and greater frustration compared with the full-control condition. The one notable order effect was on explicit control ratings, where participants who performed the full control task first subsequently reported feeling less in control during the 90-270° condition than those who encountered it first.

Performance in 90-270° - learning

To assess whether learning occurred within the 90-270° motor block, we compared the percentage of successful trials in the first half versus the second half of the block. Percentage of successful trials was higher in the second half (M = 40.12%, SE = 24.35.) compared with the first half (M = 27.71%, SE = 24.12), $t(82) = 4.69$, $p < .001$, $d = .51$, suggesting that participants' motor performance improved later half compared with first half within the same block, consistent with a learning/adaptation process.

Temporal binding effects after motor control tasks – Confirmatory analyses

Action binding.

A linear mixed-effects model on mean magnitude of action binding (in milliseconds) was conducted with a) Group (Between-subject factor: Starting with Full-Control task v. Starting with 90-270° motor task), b) Condition (within-subject factor: Following the full-control motor task v. Following the 90-270° motor task) and c) Group × Condition interaction as the fixed effects and with participant as random effect:

Action binding ~ Group + Condition + Group × Condition + (1 | participant)

Group was treatment-coded with Control as the reference category. Significance was assessed via Satterthwaite's method. Variance inflation factors (VIF) were inspected to assess multicollinearity. VIF values for Group, Condition and their interactions were ≤ 2.65 which suggests no problematic multicollinearity. Results suggest that action binding of the Starting with 90-270° motor task group (M = 25.1, SE = 5.59) did not differ from which of the Starting with Full-Control task group (M = 26.8, SE = 5.59) β = -12.12, $t(160.97) = -1.21$, p =.230. There was also no significant main effect Condition that action binding following the full-control task (M = 28.7, SE = 5.09) did not differ from action binding following the 90-270° condition (M = 23.2, SE = 5.09) β = 5.55, $t(165) = .87$, p =.389.

However, there was a significant interaction effect β = 27.54, $t(84) = 2.18$, p = .032. For participants starting with full-control task first, action binding following the 90-270° motor task (M =15.5, SE = 6.3) was significantly attenuated compared with action binding following the full-control task (M =34.8, SE = 7.96) β = -19.32, $t(84) = -2.16$, p = .034. For the starting with 90-270° motor task group, their action binding did not statistically differ between after having completed the full-control task (M = 22.7, SE = 7.96) and after the 90-270° motor task (M = 30.9, SE = 6.3, p = .366) (Figure 6).

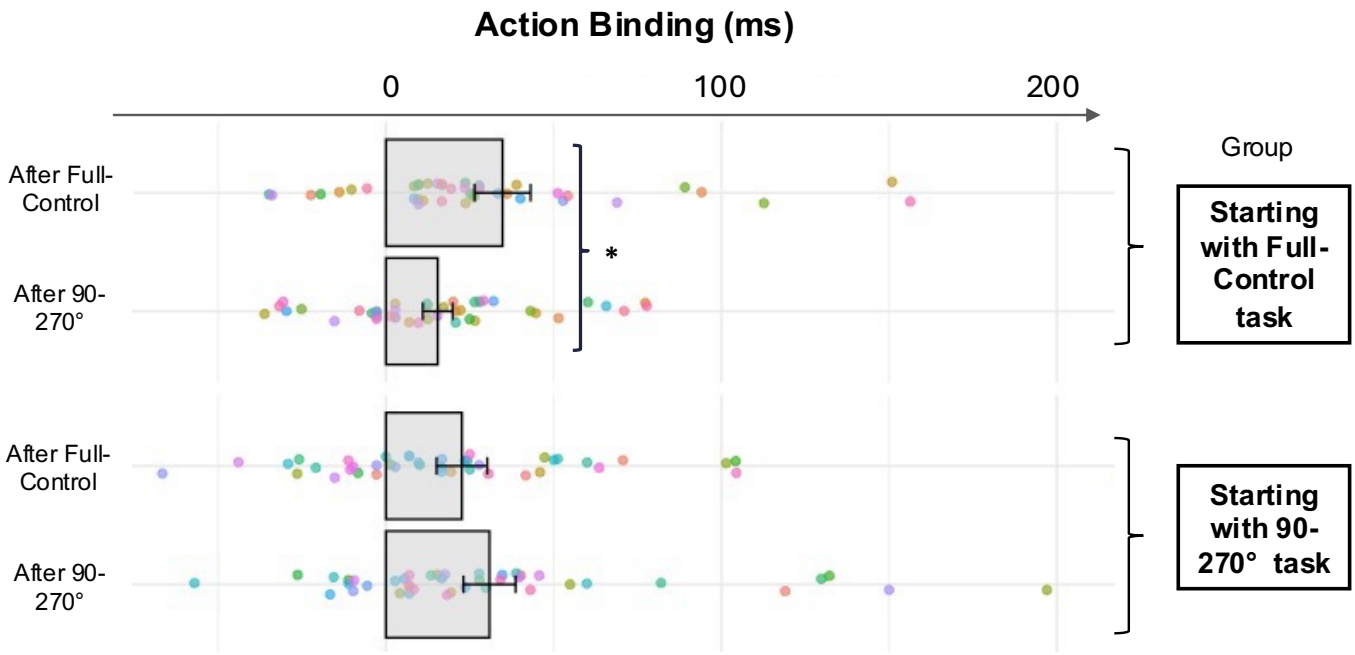


Figure 6. Bar graph showing the mean action binding (ms) across conditions (After full-control motor task, After 90-270° angular bias motor task) for participants who started with the full-control motor task first (top) and for participants starting with the 90-270° angular bias motor task first (bottom) in Experiment 1. Error bars represent standard errors. Dots represent individual data. Statistically significant differences have been denoted with asterisks on the graphs with $*p < .05$.

Here, we must acknowledge that numerically, magnitude of action binding of the second block was generally weaker compared with the first block of action binding. Thus, we conducted a linear mixed-effects model on mean magnitude of action binding with a) Group (Between-subject factor: Starting with Full-Control task v. Starting with 90-270° motor task), b) Condition (first block of action binding v. second block of action binding) and c) Group × Condition interaction as the fixed effects and with participant as random effect to check if time might explain for the results:

Action binding ~ Group + Condition + Group × Condition + (1 | participant)

Group was treatment-coded with Control as the reference category. Significance was assessed via Satterthwaite's method. VIF values for Group, Condition and their interactions were ≤ 2.65 which suggests no problematic multicollinearity. Results indeed suggested that there was a significant effect of time that participants' second block of action binding was weaker than which of the first block β = -19.32, $t(84)$ = -2.16, p = .034. This means that the second block of action binding's magnitude (M = 19.06, SE = 4.38) dropped off from the

first block of action binding (M = 32.84, SE = 5.67), regardless of what motor control block condition it had followed. Nevertheless, there was no main effect of Group β = -3.90, t(160.97) = -.39, p =.699 and no significant interaction effect β = 11.10, t(84) = .88, p =.383. This suggests that the general temporal attenuation of action binding did not differ significantly between groups. It must be fully acknowledged that action binding magnitude generally attenuated across the session but it is likely here that time alone might not fully explain why the two starting order groups showed different patterns in action binding; and the observed difference might be attributable or more sensitive to the nature of the motor control task that preceded it and demonstrated in the initial LMM model.

Tone binding and total binding.

We conducted a similar linear mixed-effects model for tone binding as the outcome with a) Group (Between-subject factor: Starting with Full-Control task v. Starting with 90-270° motor task), b) Condition (within-subject factor: Following the full-control motor task v. Following the 90-270° motor task) and c) Group × Condition interaction as the fixed effects and with participant as random effect:

Tone binding ~ Group + Condition + Group × Condition + (1 | participant)

Group was treatment-coded with Control as the reference category. Significance was assessed via Satterthwaite's method. VIF values for Group, Condition and their interactions were ≤ 2.14 which suggests no problematic multicollinearity. We found neither a significant main effect of Condition β = 2.35, t(84) = .20, p =.839 nor the main effect of Group β = -4.52, t(107.61) = -.20, p =.845 on tone binding. There was also no significant interaction effect β = -5.82, t(84) = -0.36, p =.772.

Likewise, a similar linear mixed-effects model was run on the total binding effect as the outcome:

Total binding ~ Group + Condition + Group × Condition + (1 | participant)

Group was treatment-coded with Control as the reference category. Significance was assessed via Satterthwaite's method. VIF values for Group, Condition and their interactions were ≤ 2.21 which suggests no problematic multicollinearity. We did not find significant main effect of Condition β = 21.67, t(84) = .1.46, p =.147 ; main effect of Group β = 7.61, t(117.33) = .30, p =.764 and we did not observe significant interaction effect β = -33.36, t(84) = -1.59, p =.115 (See Table *6* for descriptive statistics).

Table 6.

Mean and SE of the action binding, tone binding and total binding effect in each experimental condition.

Group	Condition	Mean action binding (ms)	SE action binding	Mean tone binding (ms)	SE tone binding	Mean total binding (ms)	SE total binding
Starting with Full-control motor task	Full-control	34.79	7.96	-103.12	15.07	-137.90	18.32
	90-270°	15.47	6.34	-100.76	17.79	-116.23	17.87
Starting with 90-270° motor task	Full-control	22.66	7.96	-107.63	15.07	-130.29	18.32
	90-270°	30.89	6.34	-111.09	17.79	-141.98	17.87

Exploratory analyses

We conducted correlational analyses (with application of the Benjamini-Hochberg FDR procedure) to examine the associations between action binding, tone binding, level of frustration, the percentage of successful trials and explicit control rating following each motor control task (Table *7*). Level of frustration and percentage of successful trials in each motor condition were not significantly associated with the magnitude of neither action binding nor tone binding effect following the respective motor control task. Action and tone binding also did not correlate significantly with explicit control following either motor control conditions (Table *7*). On the other hand, explicit control was significantly correlated with the percentage of successful trials in the 90-270° condition. This suggests that the more successful trials wherein the cross was touched, the more likely participants were to report feeling more in control in the highly disruptive motor task. Explicit agency was also negatively correlated with level of frustration, all illustrating that the explicit sense of agency was modulated by the outcome valence of the disruptive 90-270° motor task (Table *7*).

Table 7.

Experiment 1's correlation analyses between action binding, tone binding effect, percentage of successful trials, feeling of frustration and explicit control ratings with FDR corrections.

	AB after Full-control	AB after 90-270°	TB after Full-Control	TB after 90-270°	Frustration after Full-Control	Frustration after 90-270°	% Successful Trials in Full-Control	% Successful Trials in 90-270°	Explicit Control Rating after Full-Control	Explicit Control Rating after 90-270°
1. AB after Full-control	1.00	.19 (p = .325)	-.18 (p =.325)	-.10 (p = .730)	.24 (p = .126)	.02 (p = .966)	.09 (p = .788)	.04 (p = .928)	-.11 (p = .695)	-.08 (p = .894)
2. AB after 90-270°	-	1.00	.15 (p = .529)	.15 (p = .486)	.07 (p = .894)	-.04 (p = .928)	.17 (p = .415)	-.13 (p = .645)	-.06 (p = .903)	-.09 (p = .788)
3. TB after Full-Control	-	-	1.00	.76*** (p < .001)	.04 (p = .928)	.00 (p = .979)	-.02 (p = .966)	.00 (p = .979)	.01 (p = .979)	-.00 (p = .979)
4. TB after 90-270°	-	-	-	1.00	.02 (p = .966)	.04 (p = .928)	-.07 (p = .894)	-.04 (p = .928)	-.03 (p = .951)	.03 (p = .954)
5. Frustration after Full-Control	-	-	-	-	1.00	.27 (p = .075)	.10 (p = .730)	-.07 (p = .894)	-.53** (p = .009)	-.28 (p = .058)
6. Frustration after 90-270°	-	-	-	-	-	1.00	.21 (p = .229)	-.54** (p = .009)	-.13 (p = .645)	-.53** (p = .009)
7. % Successful Trials in Full-Control	-	-	-	-	-	-	1.00	-.04 (p = .928)	-.12 (p = .670)	-.33* (p = .015)
8. % Successful Trials in 90-270°	-	-	-	-	-	-	-	1.00	.03 (p = .966)	.56** (p = .009)
9. Explicit Control Rating after Full-Control	-	-	-	-	-	-	-	-	1.00	.28 (p = .062)
10. Explicit Control Rating after 90-270	-	-	-	-	-	-	-	-	-	1.00

Note. AB = action binding; TB = tone binding; *p < .05; **p < .01; ***p < .001

Discussion

In this experiment, participants performed two blocks of motor control task with each block varied in difficulty i.e., full-control v. highly disruptive 90-270° rotation motor block; and each motor block was followed immediately by a block of Libet Clock task to measure temporal binding effect. Results suggested that temporal binding effect following each type of motor task did not differ from one another. However, we found the significant effect of starting order of the motor task on temporal binding, in particular action binding. For individuals starting with the full-control motor task first, action binding was attenuated after completing the high-disturbance reaching task with 90-270° angular bias compared with action binding following the full-control task. On the contrary, participants starting off with the highly challenging 90-270° angular bias motor control task first had the magnitude of action binding consistent across the experimental conditions. This confirmed the hypothesis that being thrown in the deep end helped preserve the sense of agency, particularly action binding.

It is possible that for the group of participants who started with the difficult motor task first, after already adapting and creating new internal models from learning movement errors, switching back to non-disturbance motor reaching task might have been perceived as comparatively easy (given that they had already been exposed to a far more demanding perturbance from the get-go). Meanwhile, performing the easy task and switching to the difficult version of such motor task disrupted the straightforward action-outcome contingency estimates that the motor system had already established for this task. This could be because the angular transformations in the 90-270° were overwhelmingly disruptive, making the estimate of how to compensate for such perturbation difficult to learn, thereby impairing action binding following the completion of the high-disturbance task. It is worth noting that this effect appears to be perceptual rather than purely motoric in nature because actual task performance did not differ significantly between groups, thus the order effect was not driven by differences in how successfully participants managed to touch the cross with the circle. Explicit feelings of control (not from the primary analyses) appeared to also be sensitive to task order such that whilst the two groups did not differ in their explicit control ratings during the full-control motor task, participants who started with the easy task reported significantly lower feelings of control during the 90-270° condition compared with those who had started with the difficult task first. This somewhat mirrors the pattern of temporal binding measuring

implicit sense of agency. The transition between motoric states shifts might have influenced the experience of agency rather than changing objective motor execution itself.

However, this interpretation should be treated with caution, as exploratory analyses further showed that explicit control ratings and temporal binding magnitude were not significantly correlated with one another. A speculation is that the explicit control rating was directly about the touch the cross motor tasks (specifically how control they felt over the white circle) whilst the temporal binding effect was meant to capture the implicit sense of agency for separate unrelated action-outcome sequence. Thus temporal binding effect might have reflected the more general psychological states carried over from the motor tasks rather than sense of agency over the motor task itself. Explicit rating and temporal binding effect might be capturing distinct aspects of the sense of agency and should not be treated as interchangeable indices of the same underlying construct (Dewey & Knoblich, 2014; J. W. Moore et al., 2012; Saito et al., 2015).

It should also be acknowledged that the 'thrown in the deep end' hypothesis represented a reversal of the initial prediction (that after being in a period of sensorimotor disruption would reduce subsequently temporal binding effect for unrelated action-outcome). This revised prediction was based on pilot findings from a small and likely underpowered sample (N = 40/pilot experiment). Importantly, we did not include pre-manipulation baseline binding because we considered the length of completing three Libet clock blocks (each block of Libet Clock is comprised of 4 different sub-blocks, taking around 20 minutes to complete) to be too long and excessive for participants to perform in one experimental sitting (equating to an hour of Libet clock and 20 minutes of motor control blocks). We fully acknowledge that pre-manipulation Libet clock to measure binding before any motor control task would have beneficial to assess changes in binding in relative to baseline. That is, not having pre-manipulation baseline in the current experiment limits the strength of conclusions that can be drawn because it was not possible to confirm that the two groups were equivalent in their baseline temporal binding before exposure to the motor tasks. These limitations mean the findings should be interpreted cautiously and the hypothesis treated as preliminary pending replication with a larger sample and a full baseline measure.

Finally, in this experiment, the temporal binding was only measured once after each motor block. This prevented the assessment of how the 90-270° angular bias motor control task could influence the temporal binding system dynamically as individuals perform this motor task over time. As a result, in the next experiment, we employed a block design to better capture this relationship between the performance of the high-perturbance

sensorimotor control task and the subsequently measured temporal binding. In other words, the block design could allow us to understand how the binding system might 'online' update after each block of exposure to the highly disruptive task. This could be done by measuring temporal binding at multiple blocks over time to track how the motor control task affected agency over these blocks.

Experiment 2

In the second experiment, we utilised many alternative blocks of motor control and interval reproduction task that measured temporal binding to assess how the temporal binding effect was timely affected by the nature of the motor task over the course of time. In other words, having repeated paired blocks of motor control and temporal binding allowed us to observe how the temporal binding effect dynamically update over the time following the exposure to a short period of the motor control task. It is worth noting that this experiment employed the interval reproduction paradigm rather than the Libet clock for practical reasons. Because this design required many repeated binding measurements across the session, the Libet clock would have been impractical as it requires four separate estimation blocks to produce a single binding score, making it too time-consuming for a multi-block design. The interval reproduction method by contrast provides a single binding estimate per block, making it better suited to capturing how temporal binding shifts across successive exposures to the motor task.

Participants were randomly assigned to two groups. The Control group completed an alternating paired blocks of 1) full-motor control task with no mouse-trajectory disruption and 2) interval reproduction task that immediately followed the end of the motor control block. The Disrupt group completed the experiment in similar format with the exception that the motor control blocks they perform would all have the 90-270° angular disruption in the mouse trajectory. To clarify, the Control group of participants would only complete the full-control motor control task, and the Disrupt group would only complete the 90-270° motor control task throughout the experiment. As participants in this experiment completed only one type of motor task throughout rather than switching between easy and difficult versions as in the first experiment, there were no strong directional predictions about how temporal binding would change across blocks. The main aim was instead to explore how temporal binding of participants who had repeated exposure to disrupted motor conditions shaped over time compared with which of participants who had repeated exposure to full-control, easy task.

Method

Participants

A priori power analysis was conducted using G*Power to determine the sample size needed for a mixed ANOVA 2 (between-subject Control v. Disrupt group) × 7 (within-subject: baseline temporal binding v. 6 blocks of temporal binding). The power calculation targeted the Group × Block interaction effect as this represented the primary effect of interest which was whether temporal binding changed differentially across blocks as a function of Group. For the conventional medium effect size f =.25 (J. Cohen, 2013) with α =.05, power =.80 a sample of 86 participants was required. However, several limitations of this calculation should be acknowledged. Firstly, similar to Experiment 1, the assumed medium effect size was not derived from prior studies and if the true interaction effect is smaller, the study may be insufficiently powered with the associated risk of overestimating significant effects (Brysbaert, 2019). Secondly, power for a 2×7 interaction is considerably more demanding than for simpler designs and a medium effect size assumption may be too optimistic for such complex interaction. Finally, whilst the power calculation was conducted for a mixed ANOVA the primary analyses ultimately employed mixed-effects models, also deviating from the originally planned approach. As having discussed in Experiment 1, power estimation for specific predictors within mixed-effects models is considerably more complex than for omnibus ANOVA tests and cannot be directly derived from the G*Power calculation reported here (Brysbaert, 2019). Thus, we acknowledge that this experiment might be underpowered for some of the mixed-effects model estimates and that the observed interaction effects might not be reliably detected based on this power analysis alone. The G*Power calculation should therefore be understood as an approximation and all results would be interpreted with this caveat in mind.

We recruited 95 participants (*M age* = 20.18, *SD* = 3.80, 11 Men, 6 Non-binary) who were compensated with course credits. Participants were randomly assigned to either the Control group or the Disrupt group, with 43 participants (*M age* = 19.56, *SD* = 1.95, 4 Men, 2 Non-binary) in the former and 52 participants (*M age* = 20.68, *SD* = 4.78, 7 Men, 4 Non-binary) in the latter group (See Table *8* for more demographic information).

Table 8.

Participant demographic characteristics of Experiment 2 (N = 95)

Variable	Category	n	%
Household income	Less than £20,000	19	20.0
	£20,000-£39,999	18	18.9
	£40,000-£59,999	14	14.7
	£60,000-£79,999	11	11.6
	£80,000-£99,999	5	5.3
	£100,000+	10	10.5
	Prefer not to say	18	18.9
Ethnicity	White British	54	56.8
	Other White background	15	15.8
	African	3	3.2
	Another Black/African/Caribbean	2	2.1
	Bangladeshi	2	2.1
	Chinese	2	2.1
	Another Asian background	7	7.4
	Mixed/Multiple	6	6.3
	Other	4	4.2
Religion	Christianity	26	27.4
	Islam	3	3.2
	Hinduism	1	1.1
	Buddhism	1	1.1
	Judaism	3	3.2
	Atheism	39	41.1
	Agnosticism	15	15.8
	Other	7	7.4

For all confirmatory and exploratory analyses in this experiment, the final sample excluded 10 participants, with 9 participants showing extreme outliers for temporal binding effect in at least one of the seven temporal binding blocks and 1 participant failed to reproduce the intervals covarying monotonically with the actual action-tone intervals, thus final N = 85 (See data exclusion of Results for details about the exclusion criteria). This is the dataset to be used for all analyses of this Experiment 2.

Materials

Interval Reproduction Task

We employed the Interval Reproduction task to assess participants' implicit sense of agency, which was measured by how participants perceived the time interval between a voluntary action (key press) and the action outcome (a tone) (Humphreys & Buehner, 2010). In this task, each trial was comprised of two events. The first event was a key press 'y' on the keyboard. After a random interval between 500ms and 1500ms (increment of 100ms), the key press would trigger an auditory tone. This tone lasted for 100ms with a volume of 1kHZ. The 500-1500ms inter-event interval was triggered as soon as the 'y' key was pressed.

Motor Control Task

The motor control tasks (either the Full-Control motor task for the Control group of participants or the 90-270° motor control task for the Disrupt group) were similar to Experiment 1.

Design

In this experiment, our primary dependent variable was the magnitude of the temporal binding effect which was measured by the reproduction interval paradigm. The independent variables were the between-subject factor of Group (Control v. Disrupt) and the within-subject factor of Blocks (Baseline Temporal Binding and 6 Blocks of Temporal Binding following a block of the motor control task) (Figure 7). The dependent variable was the temporal binding magnitude for the baseline and each subsequent block, which was calculated as the mean error of estimation (i.e., perceived/reproduced interval minus actual interval) within that block. Negative reproduction errors indicated more underestimation of the intervals and thus lower mean error of estimation (more negative) indicates greater temporal binding effect i.e., greater implicit sense of agency.

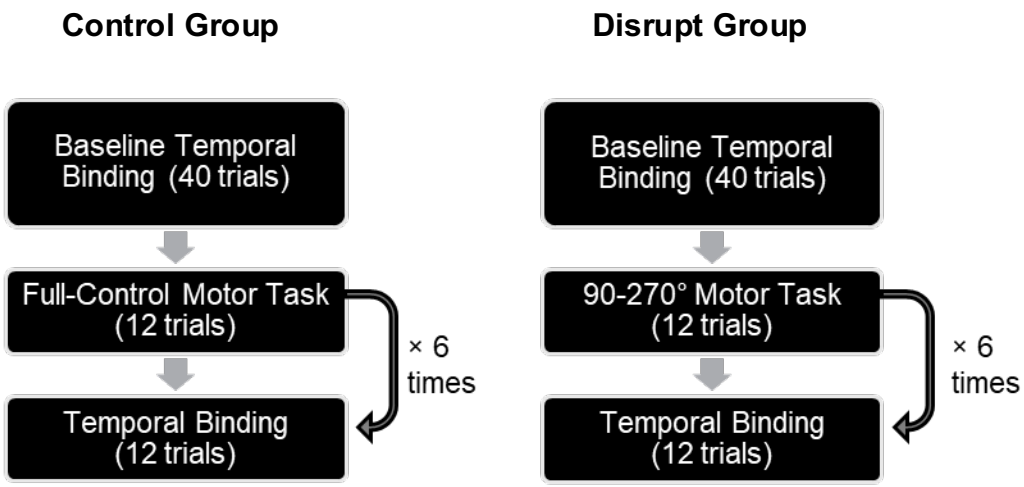


Figure 7. Outline of the experimental design of Experiment 2.

Procedure

In this experiment, having been given instructions, participants proceeded to the first interval reproduction task. Participants were instructed to press 'y' on the keyboard with their index finger. They were told that by pressing 'y' on the keyboard, this would trigger a tone but after a delay. The interval delay between the key press and the tone was randomised between 500ms and 1500ms with an increment of 100ms for each trial. After 1000ms, participants were asked to make response intervals by holding down the space bar with their index finger for the duration which they perceived the delay interval to have lasted. Afterwards, a fixation cross was presented for 1000ms, and the trial ended. All participants first completed a block of the interval reproduction paradigm with 40 trials (Operant key press – tone) to establish the baseline temporal binding effect.

Afterwards, they engaged in the first block of the motor control task which was comprised of 12 trials. Participants in the Control group would perform the full-control motor task whereby there was no disruption at all in the mouse trajectory movements, whereas participants in the Disrupt group would perform the 90-270° motor control task. Having completed the block of the respective motor task, participants moved onto a block of 12 trials of the interval reproduction task to measure the temporal binding effect, which immediately followed the end of the motor control block. All participants repeated this cycle of a motor control block (12 trials) followed by a temporal binding block (12 trials) for 6 times in total. As a reminder, the Control group of participants would only complete the full-control motor control task, and the Disrupt group would only complete the 90-270° motor control task throughout the experiment. At the end of each motor block, we also measured feeling of frustration and explicit agency by asking how much control participants felt over the white

circle. By the end of the experiment, all participants were asked to fill out questionnaires measuring demographics, similar to the previous experiment. On average, the entire experiment lasted for 45-50 minutes.

Results

All mixed-effects models' analyses were conducted using R version 2023.03 with R packages tidyverse (Wickham et al., 2019), lme4 (Bates et al., 2015), lmerTest (Kuznetsova et al., 2017) and plots were created using ggplot2.

Data exclusion

To control for outliers in the interval reproduction blocks, we calculated the z-scores for each trial of every participant and eliminated the trials a) whose z-scores exceeded ±3, and b) trials with reproductions of less than 100ms, resulting in 2.1% of total trials removed. We then calculated z-scores for all participants based on all participants' mean of each block for temporal binding of all seven blocks (baseline block and six blocks following each motor task). Individuals with z-score(s) of their mean temporal binding effect exceeded ±3 of at least one block across all seven blocks were excluded. There were 9 participants whose z-scores of errors of estimation which had exceeded the thresholds ($z(s)>\pm3$) for at least one of these seven blocks and so these 9 participants were excluded.

Further following exclusion criteria was applied, consistently with previous studies involving the same methodology (Pascolini et al., 2021, 2025) that participants not producing reproductions covarying monotonically with veridical intervals (i.e., $r < .4$) were excluded. One participant in the Disrupt group (with $r = -.25$) failed to meet the criteria. Thus this leaves the complete dataset with N = 85 (N = 41 for Control group; N = 44 for the Disrupt group).

Manipulation checks

Several mixed-effects models were conducted to examine how Group (between-subject: Control vs. Disrupt) and Block (within-subject: Blocks 1-6, treated as continuous) influenced behavioural outcomes including Frustration rating, Explicit Control rating and % successful trials. The models included random intercepts and random slopes for block at the participant level to account for individual differences in temporal trajectories. Group was treatment-coded with Control as the reference category. Significance was assessed via Satterthwaite's method. VIF values were inspected to assess multicollinearity. Firstly, for frustration as the outcome variable:

$$Frustration \sim Group + Block + Group \times Block + (1 + Block \mid Participant)$$

VIF values for Group, Block and their interaction were ≤ 2.46, suggesting no problematic multicollinearity. Results showed that there was a significant main effect of Group such that the Disrupt group (M = 4.94, SE = .18) reported significantly higher frustration than the Control group (M = 2.18, SE = .19), β = 3.56, t(83.4) = 13.47, p < .001. The main effect of Block was not significant, β = .03, t(84.3) =.52, p =.602 that frustration did not change across blocks in the Control group. However, there was a significant Group × Block interaction, β = -.23, t(83.8) = -2.96, p = .004 such that frustration significantly decreased across blocks in the Disrupt group but remained stable in the Control group (see Table 9, Figure 8).

Secondly, for Explicit Control rating as the outcome variable:

Explicit Control Rating ~ Group + Block + Group × Block + (1 + Block | participant)

VIF values for Group, Block and their interaction were ≤ 2.23, suggesting no problematic multicollinearity. Results suggested that there was a significant main effect of Group such that the Control group (M = 82.1, SE = 3.43) reported significantly higher explicit control rating than the Disrupt group (M = 33.9, SE = 3.30), β = -50.66, t(83) = -11.20, p< .001. The main effect of Block was not significant, β = .40, t(83) = .53, p = .597. The Group × Block interaction was also not significant, β = .71, t(83) = .67, p = .503, suggesting that explicit control ratings remained stable across blocks for both groups.

Table 9.

Descriptive means and standard errors (SE) of temporal binding (TB) effect, rating of frustration, explicit control rating and % successful trials of the two Disrupt and Control groups across blocks.

Control group	Mean TB	SE	Mean Frustration	SE	Mean Explicit Control rating	SE	Mean % Successful trials	SE
Baseline	-267.26	41.55						
Block 1	-261.21	49.02	2.03	0.18	78.22	3.17	98.78	2.15
Block 2	-184.69	49.21	2.24	0.22	81.85	3.57	98.98	3.59
Block 3	-159.79	49.03	2.08	0.23	84.02	3.71	96.55	4.14
Block 4	-145.97	49.32	2.11	0.25	83.63	3.88	98.58	4.31
Block 5	-128.68	48.05	2.21	0.27	84.10	4.03	98.37	4.41
Block 6	-163.98	47.22	2.21	0.28	80.05	4.33	98.78	4.60

Disrupt group	Mean TB	SE	Mean Frustration	SE	Mean Explicit Control rating	SE	Mean % Successful trials	SE
Baseline	-299.70	40.11						
Block 1	-259.22	47.32	5.79	0.17	28.93	3.06	12.88	2.07
Block 2	-244.20	47.50	5.00	0.21	35.27	3.44	23.49	3.47
Block 3	-215.75	47.33	5.07	0.22	34.93	3.58	31.82	4.00
Block 4	-260.90	47.61	4.91	0.24	32.23	3.74	34.28	4.16
Block 5	-207.57	46.39	4.67	0.25	35.14	3.89	35.99	4.26
Block 6	-226.14	45.58	4.64	0.26	36.73	4.18	38.07	4.44

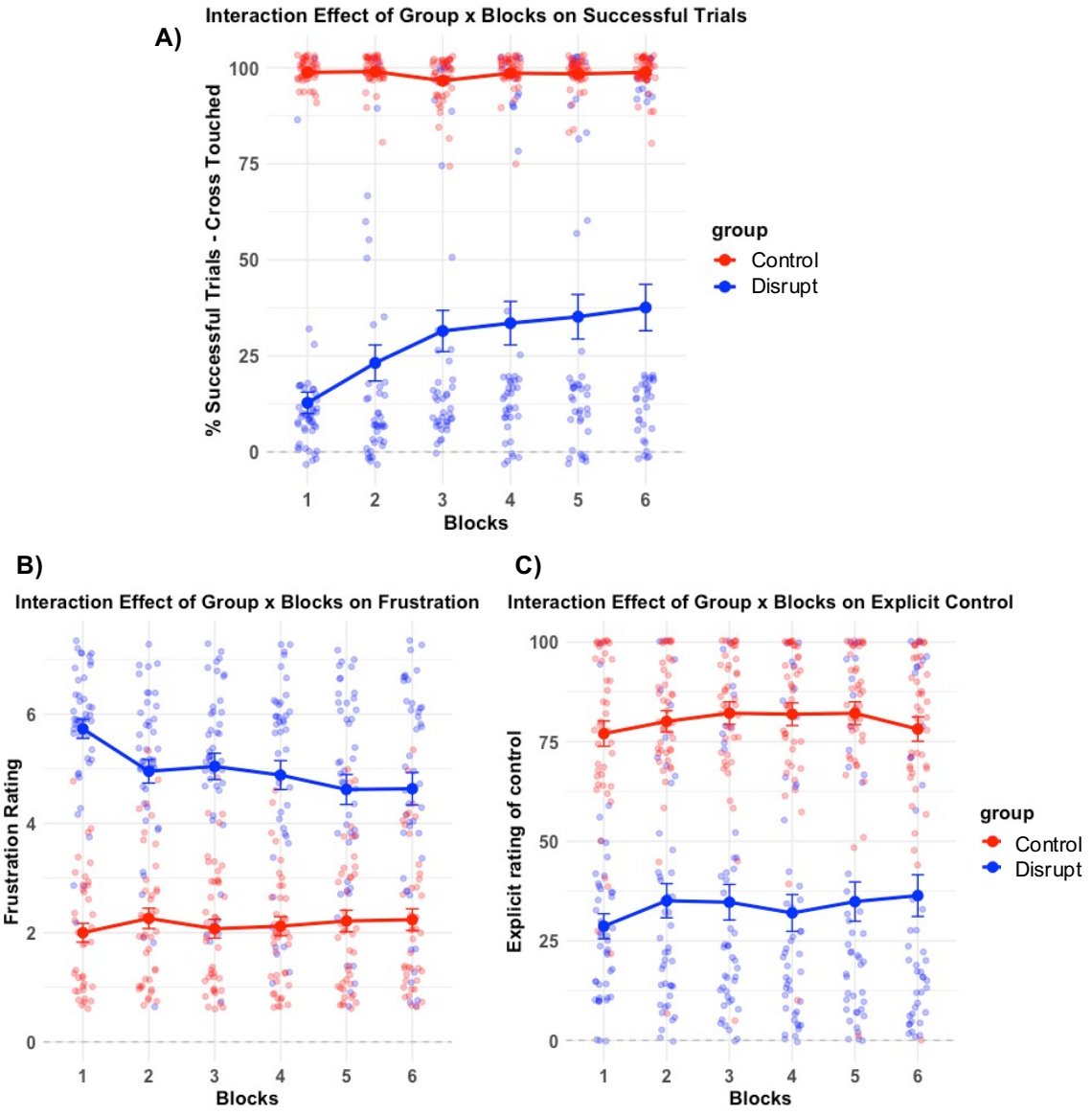


Figure 8. Line charts showing the progress of A) percentage of successful trial (%); B) Frustration rating and C) Explicit control rating across the 6 motor control blocks for Control group (in red) and the Disrupt group (in blue). Error bars represent standard errors. Dots represent individual data.

Lastly, for % successful trials as the outcome variable:

% Successful Trials ~ Group + Block + Group × Block + (1 + Block | participant)

VIF values for Group, Block and their interaction were ≤ 2.12, suggesting no problematic multicollinearity. Results showed a significant main effect of Group such that the Control group (M = 98.3, SE = 3.35) showed significantly higher success rates than the Disrupt group (M = 27.8, SE = 3.23), β = -87.48, $t(83) = -27.05$, $p < .001$. The main effect of Block was not significant, $\beta = .01$, $t(83) = .01$, $p = .994$. Importantly, there was a significant Group × Block interaction, $\beta = 4.84$, $t(83) = 4.24$, $p < .001$ indicating that while the Control group maintained near-ceiling performance throughout, the Disrupt group showed a meaningful increase in successful trials across blocks (see Table 9, Figure 8). This result suggested that the 90-270° motor task, though disruptive, was learnable and that participants of Disrupt group gradually adapted over time.

Temporal binding – primary analyses

We conducted a linear mixed-effects model wherein temporal binding (in milliseconds) was the outcome variable and was left in its raw unit to preserve interpretability. Note that temporal binding effect included the baseline binding prior to any motor control block (which helps control for baseline group differences) as well as temporal binding following each of the 6 motor control blocks. Block was entered as a continuous centred numeric predictor (block was centred around its mean prior to analysis; 0 corresponds to the midpoint of the experiment, with one unit representing one block) and a quadratic term ($block^2$) was included to assess the non-linear trajectory. Continuous predictor variables (Frustration, Explicit Control ratings and % Successful Trials) were standardised (z-scored) prior to analysis to facilitate interpretation and model convergence. The model included random intercepts and random slopes for block at the participant level to account for individual differences in temporal trajectories, following recommendations by Barr et al. (2013):

Temporal Binding ~ Group × (*Block + Block² + Frustration + Explicit Control rating + % Successful Trials*) + (*1 + Block | Participant*)

Group was treatment-coded with Control as the reference category. Significance was assessed via Satterthwaite's method. Here, correlation coefficients between predictors were assessed and correlations between Frustration, Explicit control rating and % successful trials were high (≤ .858) (Table 10). However, VIF values for Frustration and Explicit Control rating and their interactions with Group were within acceptable ranges (VIFs ≤ 5.52). Elevated VIF values for % Successful Trials (VIF = 26.10) and % Successful Trials × Group interaction (VIF = 26.23) reflected the intended experimental manipulation of the design, specifically, a ceiling effect on % successful trials in the Control group (M ≈ 98%) rather than problematic multicollinearity but the results were interpreted with caution.

Table 10.

Correlation coefficients between temporal binding and main fixed effects of Experiment 2 (N = 85)

	Temporal Binding	Group	Block	Block²	Frustration	Explicit Control rating	% Successful Trials
Temporal Binding	.	-.105*	.117**	-.057	-.135**	.117**	.115**
Group		.	.057	.014	-.684***	.702**	.817***
Block			.	0.000	-.081	.038	.100*
Block²				.	-.016	-.010	.029
Frustration					.	-.813***	-.800***
Explicit Control rating						.	.858***
% Successful Trials							.

Note. * $p < .05$; ** $p < .01$; ***$p < .001$

The random effects showed substantial between-participant variability in baseline temporal binding (random intercept variance = 74884, *SD* = 273.65) and moderate variability in individual trajectories across blocks (random slope variance for block = 690, *SD* = 26.27). The small negative correlation between random intercepts and slopes (r = -.15) suggests that participants with stronger baseline binding showed a slightly faster attenuation over blocks though this relationship was weak.

Regarding fixed effects, there was no significant main effect of Group (β = 94.96, $t(314.20) = 1.02$, $p = .307$) so the Control and Disrupt groups did not differ significantly in overall temporal binding magnitude when averaged across all blocks. For Control group, Block showed a significant positive linear effect $\beta = 31.29$, $t(123.58) = 5.16$, $p < .001$, such that temporal binding attenuated (became less negative) as blocks progressed. There was also

a significant negative quadratic effect of block amongst Control group β = -10.82, t(343.71) = -4.37, $p < .001$, indicating that amongst participants of this group, this attenuation was not linear but rather decelerated over time and this suggests a pattern of initial attenuation followed by partial stabilisation. Importantly, there was a significant Group × Block linear interaction β = -26.10, $t(131.98) = -3.00$, $p = .003$ that the linear attenuation of temporal binding over blocks was significantly weaker in the Disrupt group compared with the Control group. There was also a significant Group × Block² interaction $\beta = 10.47$, $t(340.97) = 3.08$, $p = .002$ which indicated that the curvature of the trajectory also differed between groups such that the Disrupt group showed less deceleration in attenuation, suggesting a more sustained temporal binding effect across the experiment (Figure 9).

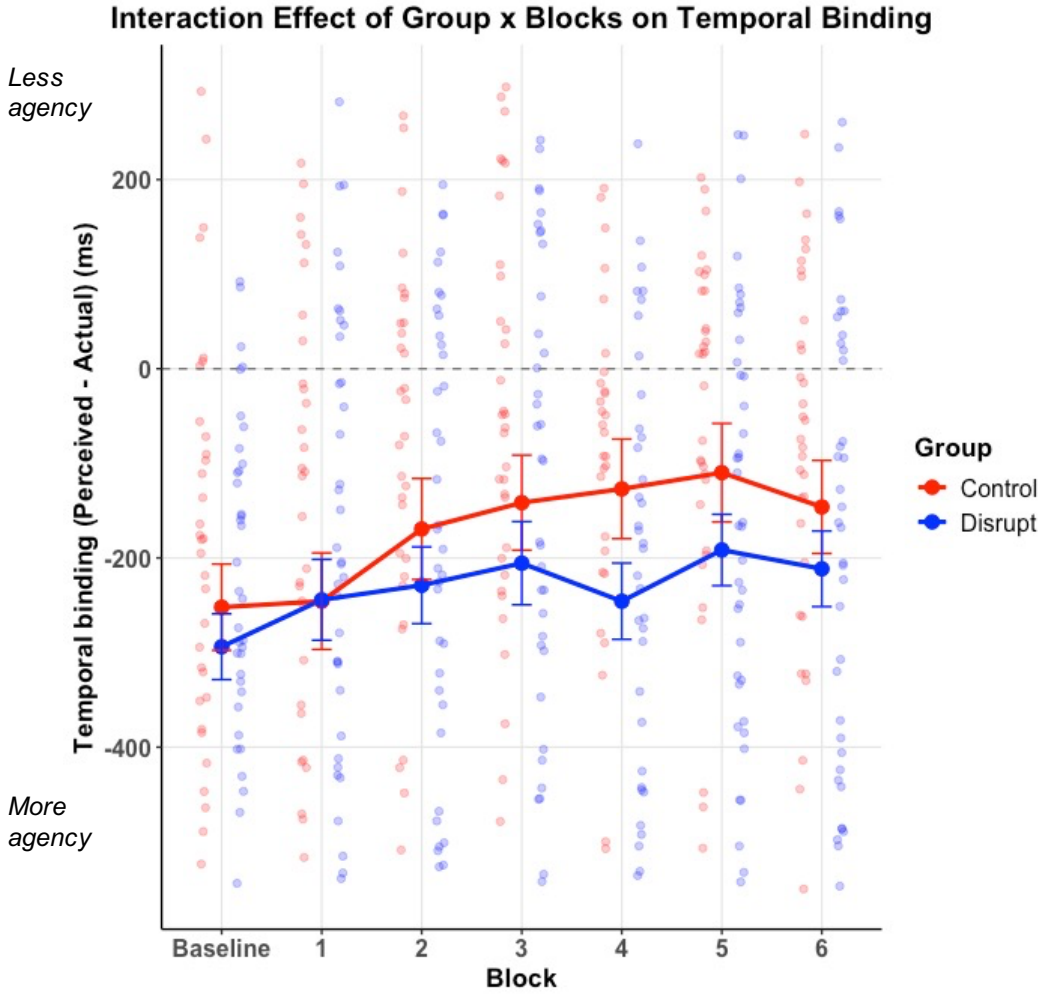


Figure 9. Line chart showing the progress of the mean temporal binding effect (ms) for baseline block (prior to motor control tasks) and across 6 later blocks (each following a motor control block) for Control group (in red) and the Disrupt group (in blue). Error bars represent standard errors. Dots represent individual data.

Other fixed-effects were not significant in the model. Frustration (β = -34.34, t(438.50) = -1.52, p = .129) and Group × Frustration (β = 39.03, t(436.65) =1.37, p = .171) were not significant. Explicit Control rating (β = 42.02, t(442.33) =1.45, p = .148) and Group × Explicit Control rating (β = -51.68, t(440.16) = -1.45, p = .149) were not significant. % Successful Trials (β = 137.32, t(371) =1.80 , p = .073) and Group × % Successful Trials were also not significant (β = -114.17, t(384.19) = -1.43 , p = .154). These results suggested that actual performance outcome and the subjective ratings did not predict temporal binding magnitude once block trajectory and group were accounted for.

Discussion

In this experiment, by utilising the multiple-blocks design, we were able to assess the effect of each motor control block on temporal binding and how such effect progressed over the course of the experiment with a total of six blocks. Importantly, we manipulated the difficulty of the motor task so that half of the participants performed solely full-control reaching task without any disturbance i.e., Control group. Meanwhile, the other half (i.e., the Disrupt group) was assigned to complete 90-270° linear angular bias motor control task, which possessed much higher level of disruption and required greater effort of control. The results showed that participants in the Control group had the magnitude of temporal binding effect significantly attenuate (or become less negative) over time as blocks progressed, thus a loss in implicit agency. In contrast, the group of participants i.e., the Disrupt group that were completing blocks of 90-270° motor control task showed sustained temporal binding effect after each block of motor control throughout the experiment.

Results further suggested that actual performance outcome and the subjective ratings of explicit control and frustration did not predict temporal binding magnitude. The type of motor control task each group of participants were engaged in and its interaction with time (block) was significantly predictive of the temporal binding effect. It is possible that repeating monotonous and non-challenging motor task might be associated with a reduction of the magnitude of temporal binding overtime. Meanwhile, despite the 90-270° linear angular bias reaching task being more challenging, this difficult task was learnable and might have been beneficial for the temporal binding effect over time. Exploratory findings of this experiment showed that the 90-270° motor control task was indeed learnable since participants of Disrupt group had increased successful trials as blocks progressed. Meanwhile, the Control group, who performed a simple and non-challenging motor task repetitively, reached a ceiling effect in the percentage of successful trials. One possible

interpretation is that regardless of actual success rate, the ongoing need of engagement via adjusting motor commands and resolving visuomotor discrepancies might have helped participants of the Disrupt group maintain the temporal binding effect over time. Altogether, these findings showed that performing an easy task repeatedly may introduce monotony and reduced engagement, and that this was associated with a diminished temporal binding effect relative to the group actively engaged in the more challenging but learnable 90-270° task.

General Discussion

This chapter shed light into the intricate relationship between the effect of motor disruption and sensorimotor adaptation as the antecedents of the subsequently measured temporal binding effect i.e., proxy for an implicit sense of agency. In summary, the two preliminary pilot experiments were intended to replicate the influence of learned helplessness on temporal binding as found in Soral et al. (2021) but with the utilisation of a highly perturbed reaching motor control task adapted from Wen and Haggard (2020). Thus, we investigated if helplessness induced by sensorimotor disturbances would cause similar attenuation in the sense of agency. However, we consistently showed that this relationship was far more complicated: individuals only lost temporal binding effect because of sensorimotor disruption if they had completed the easy version of the motor task first then completed the highly disruptive 90-270 angular bias task. Meanwhile, their counterpart who had performed the high perpetuation version first had their temporal binding effect sustained overtime. Experiment 1 was powered more to test this hypothesis of order effect (although acknowledged that it might still be underpowered according to Brysbaert, (2019) so findings were interpreted with caution). Additionally, by using the Libet Clock to measure temporal binding, we could quantify the impact of starting task order on action versus effect binding. Results from Experiment 1 appeared to support this notion of 'being thrown in the deep end' being beneficial for the implicit sense of agency: participants assigned to the starting with the highly disruptive 90-270° angular bias showed sustained magnitude of action binding over time. In contrast, individuals starting with the full-control and easy reaching motor task lost significantly their action binding after completing the 90-270° linear disturbance motor control block.

Finally, Experiment 2 utilised a multiple-block design whereby a block of motor control and a block of temporal binding measured using the interval reproduction task was alternated for a total of six times. This design enabled the assessment of how temporal binding effect timely unfolded over the course of the experiment, especially following each

brief exposure to the motor control task. Half of the participants performed a simple, full-control reaching task (Control group), while the other half completed a more challenging 90-270° linear angular bias task (Disrupt group). We found that the Control group who repeatedly performed the easy full-control motor task had the magnitude of the temporal binding effect significantly attenuated over time. In contrast, for those who completed blocks of the 90-270° linear disturbance motor task, the magnitude of the temporal binding maintained stably negative across the experiment, illustrating the preservation of implicit agency.

Performing easy motor control task then switching to the high-perturbance version impairs the temporal binding effect (specifically action binding)

The first primary finding of the study is that participants who started with the easy motor control task then transitioned to the difficult and high-perturbance version showed an attenuated temporal binding effect after completing the high-perturbance version, compared with their temporal binding following the easy motor task. Thus, the transition from an easily controllable task to a more challenging one might have a negative impact on the implicit sense of agency, particularly when later engaging in the highly disruptive motor task. On the contrary, for participants who faced the challenging task in the beginning and afterwards performed the easier version of the motor control task, the magnitude of the temporal binding effects following both motor control blocks were comparable. This pattern is broadly consistent with the hypothesis that early exposure to a more demanding motor task might be beneficial for preserving the implicit sense of agency though these findings should be treated as preliminary given the absence of a pre-manipulation baseline and the relatively modest sample size. Without a baseline temporal binding measure, it was not possible to confirm that the two groups were equivalent before motor task exposure, which limits the strength of causal conclusions that can be drawn.

One possible explanation for such pattern is that participants starting with 90-270° task first may have developed adapted internal models for correcting movement errors early in the experiment. When they subsequently encountered the easy full-control task, this may have been experienced as comparatively undemanding and thus allow action binding to be maintained. In contrast, beginning with the easy task may have established action-outcome contingency estimates that the extreme angular disturbance of the 90-270° condition then disrupted, making it difficult to learn an effective compensation strategy and thereby impairing action binding following this transition. Past research on motor control and motor learning showed similar patterns, as observed in Zhu et al. (2023), who employed a similar

motor reaching paradigm wherein there was an angular displacement between the mouse cursor feedback and the actual reach end-point. The authors showed the benefit of adapting to larger angular bias disruption: participants with motor adaptation were more effective at finding the least noisy direction than those who did not adapt. Similarly, Braun et al. (2009) showed that after exposing participants to horizontal/vertical visuomotor rotations in a three-dimensional reaching task, any new further perturbation then fell within their prior assumption about the distribution of possible perturbations. As a result, participants therefore only needed to adjust the key parameters that were appropriate for that structure (Braun et al., 2009). Sensorimotor learning allows participants to adapt more easily and promptly to other tasks with similar structure, showing the benefits of learning new structural parameters compared with no learning (Wolpert et al., 2011). However, this remains a post-hoc account and should be treated with appropriate caution because the order effect appeared perceptual rather than purely motoric in nature. It might be perceptual because a) actual task performance did not differ significantly between groups and b) because secondary explicit control ratings results showed that participants who started with the easy task reported significantly lower feelings of control during the 90-270° condition compared with those who had started with the difficult task. Further research should look into assessing further 'control felt', which is more closely related to sense of agency and our explicit measure here; as well as 'control used' i.e., how much effort participants felt like they have exerted. By studying both of these experiences of control, future research can examine if starting with the difficult task would involve participants feeling like they use less control/effort and feeling more in control whilst starting with the easy task would involve feeling like using more effort and feeling less in control (Potts & Carlson, 2019).

Action binding

We observed the statistically significant interaction effect between starting order of tasks and the different levels of motor perturbation on only action binding but not outcome binding, supporting the idea that action and outcome binding reflect partially distinct mechanisms (Wolpe et al., 2013). Tanaka et al. (2019) suggested that action binding may depend on the execution of actions which have been associated with any outcomes throughout prior learning whilst outcome shift may depend more on the results of comparing between predicted and actual outcome. Thus, action binding might be more related to predictive process such as action execution and tone binding might be more driven by postdictive process (assessing if actual outcome matches prediction). One possible interpretation of the present finding is that participants in the easy-first group might have

experienced a perceived loss of control when transitioning to the high-disturbance motor task, which could have produced a state somewhat analogous to the control deprivation observed in Soral et al. (2021) (even though it should be noted that control deprivation was directly manipulated and measured in that study, whereas here the motor task was still learnable, see further limitations). Such a state of perceived reduced control has been associated in prior research with heightened level of action disengagement, creating barriers to the execution of subsequent actions (Beckmann, 1998; Gollwitzer, 2012; Howard et al., 2016; Kuhl & Beckmann, 1985). It is possible that this state might have had a particular impact on action binding given that action binding is thought to reflect processes related to intentional action execution. This account is, however, speculative as we did not test directly perceived control loss and impaired action binding in the present study.

Repetition of motor task and temporal binding effect over time

Experiment 2 examined how temporal binding effect evolved following repeated exposure to each type of motor task that participants performed either a simple full-control task or a challenging 90-270° angular bias task, with each motor control block followed by a temporal binding block, across a total of six blocks. Temporal binding weakened over time in the Control group but was maintained in the Disrupt group. Thus, it is suggested that there might be possible benefit of engagement in difficult but learnable motor task on the implicit sense of agency measured via temporal binding effect for future, unrelated action-outcome sequence. Completing the novel and challenging 90-270° angular bias motor task might have created greater broad and general stimulation engagement with which the system is operating by which sense of agency, measured by temporal binding effect, is attributed and thus this keeps sense of agency sustained.

For those performing the challenging 90-270° linear disturbance task, by engaging in this high-perturbance motor control task, this provides the opportunity to learn and adapt, which is beneficial for the agency system. In contrast, doing the easy version of this motor task again and again might be associated with the possible cognitive strain of sheer repetition which could therefore be detrimental to the temporal binding effect because of the accompanying monotony, boredom and stimulus deprivation. Relevant here is the work of Vuorre and Metcalfe (2016) who demonstrated a dissociation between the sense of agency and the experience of flow i.e., the effortless pleasurable absorption in a task. One of their notable findings and relevant here, is that participants who reported stronger explicit sense of agency also reported lower levels of flows and that flow peaked at moderate difficulty levels where task demands and skills were in balance. This framework offers a possible way to

explain how the Control group's easy task might have led to breakdown in engaged absorption that sustains motivated action, possibly then led to reduced temporal binding effect (the balance hypothesis). It could also help possibly explain why even though the Disrupt group's motor control task was more challenging, the task might have created better flow as demonstrated by Vuorre and Metcalfe (2016) and it is possible that such flow helped sustain the temporal binding effect. This interpretation is however speculative in the present context because Vuorre and Metcalfe (2016) tested explicit agency, whilst we focussed on temporal binding effect here and results showed that explicit control rating did not predict temporal binding effect in Experiment 2. Furthermore, flow and engagement were not directly measured thus future work should include such measures would allow these claims to be tested.

As reviewed in Chapter 1, Pacherie (2008) distinguishes the feeling of being in control from the feeling of having to exert control as these can trade off against one another and this distinction helps explain why effortless action sometimes heightens the sense of agency and sometimes diminishes it. Along the same logic, Iso-Ahola (2022) proposed the theory of conscious-nonconscious processing which is how the two fundamental human tendencies a) following the path of least resistance and b) avoiding monotony and boredom shape how everyday behaviours are carried out. In their framework, consistent repetition of demanding behaviours gradually reduces cognitive strain by strengthening the cue-behaviour relationship and increasing automaticity and this therefore enhances task performance efficiency over time. However, sheer repetition alone is insufficient because consciously introducing variability into routine patterns ('repeated-with-variety') is important to perform well both demanding and non-demanding behaviours over time. The present findings are broadly consistent with this account such that the Control group's repeated performance of the same easy task without variability or challenge may have undermined the kind of engaged processing that sustains implicit sense of agency. Meanwhile, the Disrupt group's ongoing need to adapt to the challenging 90-270° motor task may have provided the variability and novelty that Iso-Ahola (2022)'s framework identifies as necessary for sustained nonconscious engagement. It should be noted however that this theory was developed in the context of long-term everyday behaviours rather than laboratory motor tasks and the mapping between their theoretical framework and the temporal binding system as measured here remains speculative.

Further limitations

Here we acknowledged further several short-comings in the current chapter that warrants caution in interpretation and also likely to be improved in future research. Temporal binding effect appeared to be attenuated over time across the experimental session which might have acted as a confounding variable, in the sense that some groups might have simply shown a greater drop-off than others as a function of random statistical variation rather than the experimental manipulation of the degree of disturbance in the motor task preceding it. Furthermore, 'learned helplessness' was not operationalised as seemed initially intended, given the choice of a type of disruption that might allow for (some) adaptation. The motor task utilised here was adapted from Wen and Haggard (2020) originally with the intention of causing high level of disruption in motor control because in their study the participants did show low mean of success rate (also only approximately 20%, similar to our study) and that we thought such manipulation would suffice to disrupt subsequentially measured temporal binding. However, as shown in the chapter, the touch the cross task was learnable. This therefore limits the degree to which it can be said to have meaningly and fully disrupted motor control. Had we used a type of noise that cannot be adapted as in Metcalfe et al. (2010)'s 'space pilot' studies, including the notably relevant evidence in such studies in differences in adaptations (from temporal 'Lag' between mouse and cursor movements to 'Turbulence' i.e., random noise in cursor movements) and their influence on (explicit) agency across different age groups, the 'learned helplessness' in motoric sense would be much better operationalised and thus better tested. Future research should therefore aim to utilise such unadaptable task to assess the research question.

Chapter Conclusion

This chapter investigated the states factor personal level of the implicit sense of agency as measured via the temporal binding by introducing sensorimotor control disruption and assessing the magnitude of the binding effect measured following the motor control task. The findings from Experiment 1 suggests that 'Being thrown in the deep end' of a difficult but doable sensorimotor task has beneficial impact on preserving the temporal binding effect. Completing the easy motor task then performing the high-perturbance version impaired the sense of agency. Experiment 2 further showed that the sheer repetition and repeating automation itself impairs the temporal binding effect, thus engaging in novelty is necessary for sense of agency protection.

CHAPTER 4

Personal Control: Exploration of Psychological Determinants, particularly Life Satisfaction and Positive Mood, as the Antecedents of Temporal Binding

As having reviewed in Chapter 1 and 2, theories have been proposed to help us understand how the sense of agency is formed and modulated; for instance, the cue integration theory denotes that sense of agency results from both internal low-level processes serving motor control but also from the external, higher-order situational factors and cues (J. W. Moore & Fletcher, 2012; J. W. Moore & Obhi, 2012). Experiences of agency could thus be modified and influenced based on the extent to which these cues are present and play out in the immediate contexts (J. W. Moore, 2016; J. W. Moore et al., 2009; Wegner et al., 2004). Indeed, a vast range of experiments have been conducted to assess the ability to control outcomes via discriminative action selection (Beck et al., 2017); causality (Buehner, 2015); sensory predictions (Bednark et al., 2015); availability of action choices (Barlas & Kopp, 2018); or actions which were freely executed versus coerced (Caspar et al., 2016) on temporal binding effect. Thus, the literature on temporal binding revolves around the nature of the temporal binding effect itself i.e., the underlying psychological and neural mechanisms and relationship between temporal binding and conscious experience of agency: experiments are often designed to measure binding of the manipulated actions and outcomes. Meanwhile, limited research has been done to investigate the antecedents that give rise to the sense of agency. That is, how the psychological antecedents of this implicit agency system, including experience, prior knowledge, mental states of mind could enhance or impair the later goal-directed actions and thus the translated magnitude of the binding effect as the distant consequences. As a result, the current chapter aimed at understanding what psychological determinants could moderate the individual differences in the magnitude of the temporal binding effect. Importantly, we would then examine empirically whether changes in such factors would also lead to changes in the binding effect of subsequently measured goal-directed actions.

As reviewed in Chapter 2 on the antecedents of temporal binding, some studies have given evidence that the cognitive and psychological backgrounds after being manipulated could then moderate the subsequently measured binding. These included inducing learned helplessness (Soral et al., 2021), power priming (Obhi et al., 2012); and testosterone (Van Der Westhuizen et al., 2017). That is, these studies manipulated the psychological backgrounds i.e., inducing the feeling of control deprivation with an impossible to solve task; priming participants with writing about episodes wherein they felt having power over others; or directly enhancing testosterone level amongst participants prior to measuring the temporal binding effect. The authors found that helplessness decreased temporal binding (Soral et al.,

2021); whilst others found that being primed with having high power and greater level of testerone increased the magitude of the binding effect (Obhi et al., 2012; Van Der Westhuizen et al., 2017). Studies looking directly at the individual differences in the magnitude of temporal binding are even more scarce. For example, Moore (2023) found that higher Borderline Personality Disorder trait (characterised by interpersonal, affective and identity instability) was associated with weaker temporal binding effect. Render and Jansen (2019) examined how people differed inter-individually in their perception pertaining to the intensity of their own intentions and actions. Their correlational results showed that high level of narcissism and substance use were the two significant personality predictors of weaker temporal binding effect. Another study looking at aging as the individual difference factor in temporal binding ((Mariano et al., 2024). The authors showed that older adults showed attenuated temporal binding effect compared with younger adults. Aarts and Van Den Bos (2011) also showed the association between strong belief in free will (i.e., the extent to which we are free agents) and stronger temporal binding effect. In contrast, participants with disbelief in freewill (i.e., everything we do is all determined by external factors that we cannot control, like god's will) showed weaker binding. These studies are valuable because they highlight the important roles of top-down, higher-order socio-psychological stable traits in moderating the general experience of agency and as the result, the temporal dimension associated with it. However, considering the vast and diverse of human cognition that many factors could influence the prior beliefs and consequently the cognitive system itself, this calls for more attempts to bridge agency and these constructs. Accordingly, the goal of the current chapter was to identify which variables best predicted individual differences in the temporal binding effect. We then would experimentally directly manipulate the level of these variables to establish causality: whether changes in these variables, being stable trait-like factors or temporary states, would influence the magnitude of temporal binding.

In this chapter, we explored a variety of trait and state factors which could be deemed relevant to sense of agency and the feeling of being in control. Firstly, we included level of satisfaction with life, which pertains to the cognitive component of human well-being, is one's overall assessment of quality of life and the establishment of self-images and interpersonal relationships (Deniz & Yıldırım Kurtuluş, 2025). It is worth noting that wellbeing is a broader construct to portray a state of wellness and health, encompassing both affective components (frequency of positive and absence of negative emotions) and cognitive-evaluative component, which is life satisfaction (Bautista et al., 2023). In this

chapter, we focussed specifically and only on life satisfaction. It could be expected that because having a high level of life satisfaction would indicate a better sense of control over one's life, higher life satisfaction would be associated with greater temporal binding effect. Next, we also explored self-efficacy i.e., the generative capability to deal with one's environment using cognitive, social and behavioural skills (Bandura, 1982). This construct is closely related to the sense of agency because perceived self-efficacy is linked with judgments of the degree to which one could execute courses of required actions in response to probable circumstances (Bandura & Cervone, 1983). Another self-rated construct that would also be markedly relevant is the subjective perception of control, partitioned into three primary spheres: personal control i.e., control over the immediate non-social personal environment; interpersonal control i.e., perceived control over others within dyads and/or groups; and socio-political control i.e., how much control one feels over social and political events and organisations (Paulhus, 1983). Furthermore, we would also examine prior beliefs, especially embedded ones such as belief in free will (like in Aarts and Van Den Bos (2011)). Altogether, we expected that these traits might play a role in moderating temporal binding magnitude across individuals, considering how they all heavily influence one's overall subjective sense of control.

Lastly, we included a state-like variable i.e., positive and negative mood at the time of the study was conducted. Mood refers to a diffuse, lower-intensity affective state that is not necessarily tied to a specific cause and persists over a longer period, typically hours to days (Goldsmith, 1994). We focussed here in this chapter specifically on mood rather than related construct such as emotions (which refers to brief, intense affective response that is typically directed at a specific object, event, or stimulus (Frijda, 1993)). Albeit being temporary, research has shown that mood can have pervasive effect on cognition and sensory processing (Canbeyli, 2010; Chepenik et al., 2007). Thus, it would be interesting to see whether being in a positive or negative mood would moderate the temporal binding effect.

In this chapter, the first two online experiments (Experiment 3 and 4) were set to explore which of the above variables were the strongest indicator of the implicit sense of agency. In other words, we focussed on whether individual differences in trait-like variables (life satisfaction, self-efficacy, perceived control and belief in free will) and states (positive v. negative mood) modulate the temporal binding effect as the index of the implicit sense of agency. The temporal binding effect was measured by how participants compressed the time interval between a voluntary action and the action outcome using the Interval Reproduction

task (Humphreys & Buehner, 2010; Poonian & Cunnington, 2013). Experiment 3 was merely exploratory, employing the backward regression method to find the model of best fit. Experiment 4 was conducted to replicate such findings. In both experiments, all participants performed a simple temporal binding task online and then completed self-reported questionnaires that measured individual difference variables. Considering whether temporal binding methods can be conducted online, there have been several studies that show temporal binding effect can be reliably observed in uncontrolled online environments with Galang et al. (2021); Cubillas et al., (2020); Soral et al., (2021) using Libet clock and more relevantly De Azevedo et al., (2025); Le et al., (2026) using interval reproduction paradigm. However, this remains an emerging area of research thus while these findings suggest interval reproduction could provide valid measurements in online settings; further robust and careful assessment of its validity and reliability, particularly for between-subjects comparisons, is still needed before conclusions could be confidently drawn.

Results from these correlational experiments informed us of what could potentially be important psychological mechanisms that moderate sense of agency or binding. Next, based on such results, in Experiment 5 and 6, we directly manipulated these variables to attempt establishing further causal evidence that they were the determinants of temporal binding, particularly satisfaction with life and positive mood. Thus, we experimentally manipulated changes in life satisfaction with life and level of positive mood and examined whether such changes would lead to moderation in the magnitude of binding effect.

Experiment 3

The first experiment of Chapter 4 was to explore which of the trait-level variables: life satisfaction, self-efficacy, perceived control and belief in free will and state-level factors: positive and negative mood were significantly associated with the temporal binding effect. As this experiment was fully exploratory, no strong directional hypotheses were specified for individual predictors. Participants were recruited online to complete the interval reproduction task that measured the general temporal binding effect and then respond to various questionnaires that measured the above constructs. Backward regression was utilised to identify the model of best fit with the most relevant predictors.

Method

Participants

For Experiment 3, a priori power analysis was conducted using G*Power (Faul et al., 2009) to determine the sample size given α=.05 needed for simple correlations to look at individual differences. Based on medium effect size assumptions (*r*= .30) as shown with the effect sizes reported in few studies reviewed in Chapter 2 (for instance, correlation between BPD trait and temporal binding effect r=.45 (J. W. Moore, 2023)) a sample size of 64 was needed to provide a power of .80. However we fully acknowledged that more complex regression would be likely underpowered given the difficulty of estimating power for specific predictors in regression model as suggested by Brysbaert (2019).

A total sample of 79 participants, with 65 participants were recruited from the University of East Anglia to complete the experiment in return for course credits; and 14 participants were recruited on Prolific in return for payment at the rate of £12/hour (aged 18-64, *M age* = 24.32, *SD* = 10.61, 17 Men, 2 Non-binary) (See Table 11 for detailed demographic info). Ethical approval for all experiments of this chapter was given by the University of East Anglia (UEA) Research Ethics Committee prior to any testing activities and data collection.

Table 11.

Participant demographic characteristics of Experiment 3 (N = 79)

Variable	Category	n	%
Household income	Less than £20,000	12	15.2
	£20,000-£39,999	25	31.6
	£40,000-£59,999	9	11.4
	£60,000-£79,999	7	8.9
	£80,000-£99,999	4	5.1
	£100,000+	8	10.1
	Prefer not to say	14	17.7
Ethnicity	White British	59	74.7
	Other White background	4	5.1
	African	1	1.3
	Caribbean	1	1.3
	Another Black/African/Caribbean	1	1.3
	Bangladeshi	1	1.3

Variable	Category	n	%
	Chinese	2	2.5
	Pakistani	1	1.3
	Another Asian background	4	5.1
	Mixed/Multiple	4	5.1
	Other	1	1.3
Religion	Christianity	16	20.3
	Islam	3	3.8
	Hinduism	0	0
	Buddhism	0	0
	Judaism	1	1.3
	Atheism	37	46.8
	Agnosticism	15	19
	Other	7	8.9

For the main temporal binding effect analysis, the sample used for this analysis excluded 4 participants, who failed to reproduce the intervals covarying monotonically with the actual action-tone intervals, thus N = 75 (See data exclusion of Results for details about the exclusion criteria).

For the regression analyses, one participant had missing data for the Scientific Determinism questionnaire and one participant had missing data for the Fatalistic Determinism questionnaire because they did not complete these measures. Therefore, the regression analyses were conducted on N = 73.

Materials

The experiment was conducted online using Gorilla.sc platform and was set up to be run in the participants' home computer browser. The program only worked for PC and laptops but not smartphones or tablets as a physical keyboard was required. As default, the browser window was set to full screen as soon as the link to the experiment was clicked on by the participants. Introduction and instruction videos with voiceover and animation that helped explain in detail what to expect from the task in both baseline and operant condition were given.

Each experimental trial was comprised of two events. The first event was either a key press 'y' on the keyboard (in the operant condition) or an auditory tone (in the baseline

condition). After a random interval between 500ms and 1500ms, the key press would trigger an auditory tone in the operant condition, while the first tone would be followed by a second tone in the baseline condition. In both operant and baseline conditions, all the tones used always lasted for 100ms with a volume of 1kHZ. During the operant trials, the inter-event interval was triggered as soon as the 'y' key was pressed.

Individual difference measures.

Several questionnaires that measured different constructs, such as life satisfaction, self-efficacy etc. were employed to understand the extent to which individual difference factors could help explain the variances in the temporal binding effect across the participants. Internal consistency of the measures was assessed using Cronbach's alpha coefficients (Table *12*).

Table 12

Reliability of the individual difference measures

Questionnaires/Subscales	Reliability Statistics (Cronbach's Alpha)
Satisfaction with Life	.88
Self-efficacy	.82
Free Will	.58
Scientific Determinism	.60
Fatalistic Determinism	.87
Unpredictability	.68
Personal Control	.63
Interpersonal Control	.79
Socio-Political Control	.24
Positive Mood	.83
Negative Mood	.87

The General Self-Efficacy Scale (Schwarzer & Jerusalem, 2012).

Participants were asked to rate how each of a total of 10 statements was characteristic or true of them e.g. *'I can always manage to solve difficult problems if I try hard enough'.* Responses were made on a four-point Likert scale (1=*Not at all true;* 4=*Exactly true*). All scores were computed into a mean for each participant, with the greater score indicating greater belief in personal competence.

Satisfaction with Life (Diener et al., 1985).

A five-item Likert scale was utilised to measure how satisfied participants felt with their life (e.g., '*In most ways my life is close to my ideal*') and participants rated how much they agreed or disagreed with the statements on the scale from 1 (*Strongly disagree*) to 7 (*Strongly agree*). All items' responses were computed into a single mean for each participant. The higher the score, the more satisfied with life one reported.

Free Will and Determinism (FAD) (D. L. Paulhus & Carey, 2011).

The 27-item measure of lay beliefs in freewill and three other closely related constructs, namely fatalistic determinism, scientific determinism, and unpredictability was employed. For each statement, participants were asked to rate on the scale from 1 (*Strong disagree*) to 5 (*Strongly agree*). Each participant was given four mean scores for the four subscales. The belief in Free Will was measured by items such as '*People can overcome obstacles if they truly want to*', with high score on this subscale indicating that the person believes in the internal locus of control and holding individuals accountable for their actions. On the other hand, Fatalistic Determinism (e.g., '*Fate already has a plan for each of us'*), Scientific Determinism (e.g., *'People's biological makeup influences their talents and personality*), and Unpredictability (e.g., '*Life is hard to predict because it is almost totally random*') subscales pertained significantly to the belief in the implication of inevitability. Thus, the higher an individual scores on these subscales, the more deterministic they perceived life events and the external world to span out of personal control.

Sphere of Control (D. L. Paulhus & Van Selst, 1990).

The Spheres of Control scale was consisted of 30 items, which could be divided into three subscales, including Personal Control (e.g., '*I can usually achieve what I want if I work hard for it.*'), Interpersonal Control (e.g., '*I have no trouble making and keeping friends.*') and Socio-Political Control (e.g., '*By taking an active part in political and social affairs, we the people can influence world events.*'). These statements were rated using 7-point scale (1=*Disagree*, 4=*Neutral,* 7=*Agree*). On the negatively keyed items, participants' responses were reverse-coded (i.e., 7=1, 6=2, 5=3, 4=4, 3=5, 2=6, 1=7). Reponses were then calculated into three mean scores by averaging the 10 items for each subscale, with higher scores on each sphere demonstrating greater a) *personal control* over the non-social environment as in personal achievement, (b) *interpersonal control* over other people in dyads and groups, and (c) *socio-political control* over social and political events and institutions.

Depressive symptoms

Here, we adapted the items from the European Social Survey (ESS) Round 3 Well-being module wherein Huppert and colleagues (2009) adopted the 8-item depression measure from the Centre for Epidemiological Studies-Depression (CES-D) to measure how often the participants experience symptoms which were associated with depression, including restless sleep, poor appetite, and loneliness (Radloff, 1977; Steffick, 2000 as cited in Huppert et al., 2009). In ESS Round 3 and in Huppert et al. (2009), they also included further items that specifically ask about participants' experiences during the past week. Thus, there was a total of 15 items (See Appendix M for full questionnaire items, directly retrieved from the ESS round 3, which are also identical to items included in Huppert et al., 2009), whereby participants were asked to rate on the scale from 1=*None or almost none of the time* to 4=*All or almost all of the time* how much of the time during the past week they felt depressed, or '*everything [I] did was an effort.*'. Positively worded items, such as '*[I] had a lot of energy'*, were computed into one mean score that measured positive mood. The rest of the statements that addressed depressive symptoms were computed into a mean score and the higher the negative-mood score, the more depressive symptoms a person experienced over the course of one week.

Design

In the within-subject design, all participants completed an interval reproduction task (Humphreys & Buehner, 2010) with a 40-trial experimental block each of both Operant and Baseline trials. Temporal binding effect was the outcome variable and was measured by calculating the difference between the error of estimation (i.e., perceived – actual) in the operant condition and the error of estimation in the baseline condition (in milliseconds). Negative reproduction errors indicated more underestimation of the intervals and the more negative the temporal binding measure for an individual was, the greater temporal binding effect they showed and thus had a stronger implicit sense of agency.

Procedure

Once signing up for the study, participants were given a private link to the experiment. They were first presented with the information sheet that described briefly the purpose of the experiment and how their data was handled. An embedded consent form whereby participants who agreed to take part would tick all of the boxes was then presented. Afterwards, an audio initialisation that tested the audio condition of the participants was implemented to ensure that their headphones or speakers were on.

Participants were subsequently shown an instruction video that explained the task requirements. During the interval reproduction task, participants were instructed that a tone would occur after a first event: either a self-initiated spacebar press with their index finger (Operant) or after a first auditory tone (Baseline). The stimulus interval was the delay between the first event and the outcome (randomised between 500ms-1500ms, with increment of 100ms). Participants were immediately asked to make response intervals by pressing and holding down the space bar with their index finger for the duration which they perceived the stimulus interval to have lasted. Afterwards, a fixation cross was presented for 1000ms, and the trial ended. A block of 40 trials for each condition was in place: with half of the participants undertaking the Baseline block then the Operant block; and the other half undergoing the Operant block followed by a block of the Baseline condition (counterbalancing). This interval reproduction task took approximately 10 minutes to complete.

At the end of the experimental blocks, participants were asked to fill out the questionnaires that measured self-efficacy, life satisfaction, spheres of control, free will and determinism, depressive symptoms; as well as demographic questions including age, gender, household income, ethnicity, and religion. Participants could skip any questions which they did not wish to answer. Having completed the questionnaires, they were debriefed, and course credits or rewards were granted. On average, the entire experiment lasted for 22 minutes.

Results

All analyses in this experiment were conducted in SPSS version 31.

Data exclusion

To control for outliers in the interval reproduction task, we calculated the z-scores for each trial of every participant and eliminated the trials a) whose z-scores exceeded ±3, and b) trials with reproductions of less than 100ms, resulting in 2.59% of total trials removed. We then proceeded to control for outliers across participants that individuals with z-scores of their mean baseline errors and operant errors greater than ±3 were excluded from the primary analysis. There was no participant whose z-scores of errors of estimation which had exceeded the thresholds. Further following exclusion criteria was applied, consistently with previous studies involving the same methodology (Pascolini et al., 2021, 2025) that participants not producing reproductions covarying monotonically with veridical intervals (i.e., $r < .4$) were

excluded. We identified 4 participants who failed to meet the criteria. Thus this leaves the dataset with N = 75 to calculate the magnitude of the temporal binding effect.

We acknowledge that without formal attention checks since this experiment (and Experiment 4) was conducted online, some inattentive responses may remain in the dataset. However, our exclusion criteria provide some protection by excluding trials with reproductions exceeding ±3 z-scores within participants and <100ms removes cases where participants may have merely pressed the space bar rather than properly holding it down to reproduce the interval.

Note that N = 73 was a smaller sample used only for the regression because of missing data not because of additional exclusions.

Primary analysis

Main temporal binding effect.

As predicted, participants underestimated the interval between a voluntary action and the produced tone (M = -323.11, SD = 297.95) significantly more than the interval between two observed events (M = -116.12, SD = 313.93) $t(74)$ =6.67, p < .001, d=.77 (Figure 10). Such findings were in line with past research using the interval reproduction paradigm to measure the implicit sense of agency (Humphreys & Buehner, 2010).

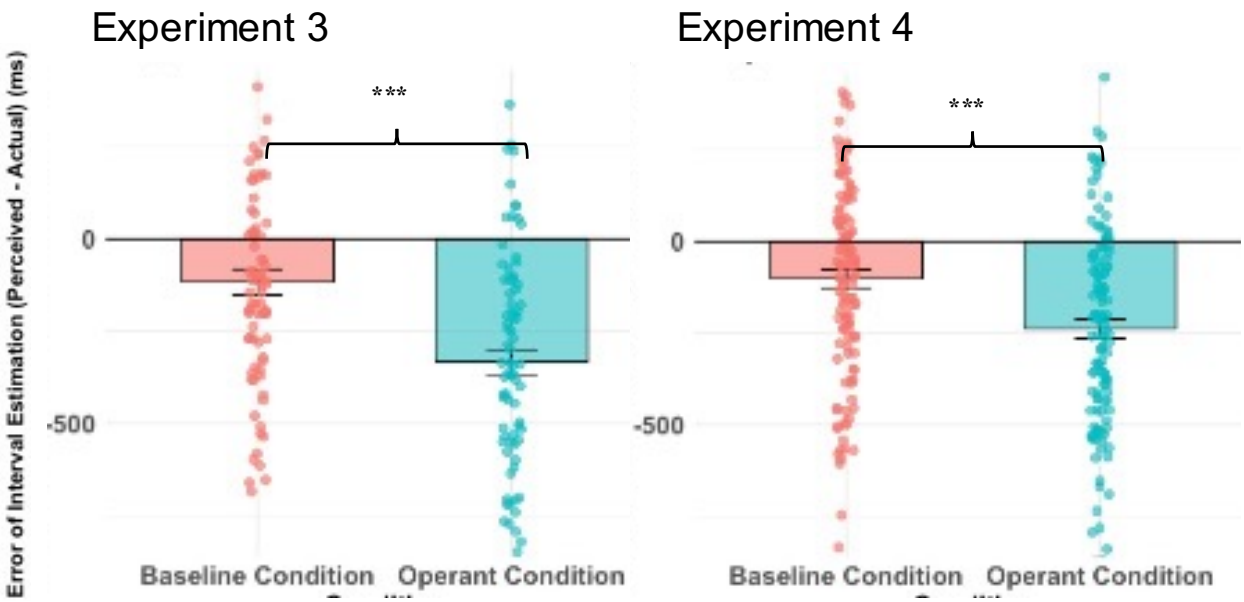


Figure 10. Bar graphs showing the mean error of interval estimation (perceived – actual) (*ms*) across conditions (Baseline v. Operant) in Experiment 3 (left) and Experiment 4 (right). Error bars represent standard errors. Dots represent individual data. Statistically significant

differences within an experiment and measure have been denoted with asterisks on the graphs with ***$p < .001$.

Individual differences.

A linear multiple regression was conducted to examine how the individual differences factors explained the outcome variable i.e., the magnitude of the temporal binding effect. Backward method was employed due to the exploratory nature of the study with open hypotheses. Gender, religion and ethnicity as categorical variables were dummy-coded before analysis. For Gender, male was coded 0 and female was coded 1. For Ethnicity, due to small cell sizes in several religion categories (see Table 11) religion was recoded into three broader categories prior to dummy coding which are Christianity, no religion (atheism and agnosticism) and other religion (Islan, Judaism and others). Christianity was coded as the reference category which yields two dummy variables (no religion and other religion). Likewise for ethnicity, it was coded into two main categories also due to small cell sizes (Table 11) which are White British and Other ethnicity (all other ethnicity responses apart from White British listed in Table 11 were grouped here). White British was coded as 0 and Other ethnicity was coded as 1. Note that household income was kept as a continuous variable, if participants selected 'prefer not to say' (see Table *11*) these were treated as missing data prior to analysis.

All of the predictors (See Table 13) were entered into the analysis. Variables which did not contribute significantly to explaining the variances in the temporal binding effect across individuals were excluded until the model of best fit was found, which was model 14.

Table 13.

Descriptive Statistics, beta coefficients, p values of the predictor variables entered into the regression using backward method & all models' statistics.

			Model													
	Mean	SD	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Temporal Binding (DV)	-210.85	271.15														
Satisfaction with Life	4.36	1.22	β = 0.38	0.38	0.38	0.38	0.39	0.39	0.39	0.37	0.36	0.37	0.37	0.36	0.37	0.35
			p =0.032	*0.029*	*0.027*	*0.024*	*0.016*	*0.013*	*0.012*	*0.012*	*0.013*	*0.01*	*0.01*	*0.012*	*0.008*	*0.012*
Self-Efficacy	2.75	0.39	0.22	0.22	0.22	0.22	0.21	0.21	0.21	0.20	0.19	0.20	0.22	0.23	0.24	0.24
			0.207	*0.201*	*0.17*	*0.165*	*0.153*	*0.15*	*0.145*	*0.157*	*0.125*	*0.096*	*0.068*	*0.06*	*0.058*	*0.062*
Belief in Free Will	3.46	0.63	0.06	0.06	0.06	0.06	0.06	0.05	0.06	0.05						
			0.686	*0.678*	*0.677*	*0.676*	*0.675*	*0.685*	*0.67*	*0.676*						
Scientific Determinism	2.98	0.57	-0.13	-0.13	-0.13	-0.13	-0.13	-0.13	-0.12	-0.12	-0.12	-0.12	-0.14	-0.15	-0.14	
			0.391	*0.375*	*0.371*	*0.352*	*0.34*	*0.337*	*0.339*	*0.326*	*0.344*	*0.324*	*0.26*	*0.206*	*0.222*	
Fatalistic Determinism	2.56	0.89	-0.13	-0.13	-0.13	-0.13	-0.13	-0.12	-0.12	-0.11	-0.12	-0.13	-0.14	-0.15	-0.15	-0.19
			0.402	*0.395*	*0.39*	*0.363*	*0.341*	*0.342*	*0.345*	*0.365*	*0.336*	*0.291*	*0.238*	*0.226*	*0.221*	*0.115*
Unpredictability	2.77	0.59	-0.01	-0.01	-0.01											
			0.954	*0.954*	*0.954*											
Negative Mood	3.46	0.54	0.02	0.02	0.02	0.02	0.03	0.03								
			0.905	*0.904*	*0.895*	*0.898*	*0.874*	*0.86*								
Positive Mood	2.23	0.67	-0.29	-0.29	-0.29	-0.29	-0.29	-0.29	-0.30	-0.31	-0.30	-0.30	-0.30	-0.31	-0.32	-0.31
			0.13	*0.117*	*0.114*	*0.1*	*0.096*	*0.093*	*0.052*	*0.04*	*0.043*	*0.039*	*0.04*	*0.032*	*0.028*	*0.033*
Personal Control	2.35	0.61	-0.01	-0.01												
			0.966	*0.964*												
Interpersonal Control	4.62	0.69	-0.05	-0.05	-0.05	-0.05	-0.05	-0.05	-0.06	-0.05						
			0.738	*0.736*	*0.726*	*0.727*	*0.726*	*0.708*	*0.682*	*0.738*						
Socio-Political Control	4.27	0.87	0.10	0.10	0.10	0.10	0.11	0.10	0.11	0.11	0.10	0.10				
			0.46	*0.453*	*0.446*	*0.427*	*0.409*	*0.408*	*0.395*	*0.389*	*0.398*	*0.409*				

Gender (Male v. Female)	-	-	0.01 *0.951*	0.01 *0.952*	0.01 *0.95*	0.01 *0.958*								
Income	3.59	2.15	0.12 *0.411*	0.12 *0.397*	0.12 *0.37*	0.12 *0.366*	0.12 *0.359*	0.13 *0.307*	0.13 *0.297*	0.12 *0.323*	0.12 *0.312*	0.12 *0.302*	0.11 *0.35*	0.09 *0.419*
Ethnic (White British v. Others)	-	-	-0.08 *0.569*	-0.08 *0.564*	-0.08 *0.554*	-0.08 *0.549*	-0.08 *0.544*	-0.08 *0.55*	-0.08 *0.552*	-0.08 *0.556*	-0.07 *0.601*			
Religion (Christianity v. No religion)	-	-	0.10 *0.618*	0.10 *0.547*	0.09 *0.545*	0.10 *0.539*	0.10 *0.533*	0.10 *0.469*	0.10 *0.461*	0.10 *0.457*	0.09 *0.498*	0.12 *0.355*	0.09 *0.457*	
Religion (Christianity v. Other religions)	-	-	0.00 *0.986*											
Age	24.44	10.73	-0.02 *0.902*	-0.02 *0.896*	-0.02 *0.895*	-0.02 *0.893*	-0.02 *0.896*							
Model statistics														
R^2 (%)			20.2	20.2	20.2	20.2	20.2	20.2	20.1	19.9	19.7	19.3	18.5	17.8	16.9	15.1
F			0.82	0.89	0.96	1.05	1.15	1.26	1.40	1.54	1.72	1.92	2.10	2.38	2.73	3.01
Df1, df2			17, 55	16, 56	15, 57	14, 58	13, 59	12, 60	11, 61	10,62	9, 63	8, 64	7, 65	6, 66	5, 67	4, 68
p			*0.665*	*0.587*	*0.505*	*0.421*	*0.339*	*0.264*	*0.197*	*0.146*	*0.104*	*0.073*	*0.056*	*0.039*	*0.026*	*0.024*

Note. For each predictor within each model, β values were shown with p beneath (p values were in italic). SD = Standard Deviation; df = degrees of freedom

The results showed that the best fit model (model 14) included life satisfaction, self-efficacy, fatalistic determinism, and positive mood as the predictors, having eliminated other variables that were initially entered.

The assumptions of regression for the final model were checked. Multicollinearity was not problematic, as the correlations between predictor variables were all less than .80 (see Table 14) and VIF was less than 10 (VIF ≤ 1.59). The histogram of residuals showed an approximately normal distribution, which suggests that the assumption was met (Appendix C). Homoscedasticity was likely met because the scatterplot showed a similar variance in residuals across predicted values (Appendix C). There were no multivariate outliers. The highest Cook's Distance value was .22.

Table 14.

Correlation coefficients between variables in the final regression model of Experiment 3 (N = 72)

	Temporal Binding	Satisfaction with Life	Self-efficacy	Positive Mood	Fatalistic Determinism
Temporal Binding	.	.200	.171	-.043	-.115
Satisfaction with Life		.	.263*	.515***	.257*
Self-efficacy			.	.443***	.026
Positive Mood				.	.09
Fatalistic Determinism					.

Note. *p <.05; *** p <.001

The final model was significant $F(4, 68) = 3.01$, $p = .024$, accounting for 15.1% of the variance in the magnitude of the temporal binding effect across individuals. Life satisfaction was the strongest predictor of the implicit sense of agency: $\beta = .35$, $t = 2.59$, $p = .012$ that higher life satisfaction scores were predictive of reduced temporal binding effect. On the other hand, positive mood was negatively predictive of the outcome variable: $\beta = -.31$, $t = -2.17$, $p = .033$ i.e., the more individuals reported experiencing positive mood within the past week, the stronger temporal binding effect they had. Self-efficacy $\beta = .24$, $t = 1.89$, $p = .062$ and Fatalistic determinism $\beta = -.19$, $t = -1.60$, $p = .115$ were not significant predictors but still retained in the final model.

Experiment 4

In this experiment, all experimental design remained the same as which of Experiment 3, but we aimed at testing whether we could replicate the best-fit model shown in the first findings, whose independent variables included life satisfaction, self-efficacy, positive mood, and fatalistic determinism on the magnitude of temporal binding as the outcome variable.

Method

Participants

For Experiment 4 a priori power analysis was conducted using G*Power (Faul et al., 2009) to determine the sample size required for a linear regression with four predictors. The model in Experiment 3 suggested that $R^2 = .151$ which equals to $f^2=.178$ so to achieve $f^2 = .178$ given $\alpha=.05$, power=.80 a sample size of 72 was required. A total of 128 psychology students were recruited from the University of East Anglia to complete the experiment in return for course credits (*M age* = 19.39, *SD* = 2.13, 24 Men, 4 Non-binary) (See Table 15 for further demographic information).

Table 15.

Participant demographic characteristics of Experiment 4 (N = 128)

Variable	Category	n	%
Household income	Less than £20,000	31	24.2
	£20,000-£39,999	13	10.2
	£40,000-£59,999	17	13.3
	£60,000-£79,999	15	11.7
	£80,000-£99,999	10	7.8
	£100,000+	9	7
	Prefer not to say	33	25.8
Ethnicity	White British	88	68.8
	Other White background	8	6.3
	African	2	1.6
	Caribbean	0	0
	Another Black/African/Caribbean	0	0
	Bangladeshi	1	0.8
	Chinese	8	6.3

Variable	Category	n	%
	Pakistani	1	0.8
	Another Asian background	12	9.4
	Mixed/Multiple	5	3.9
	Other	3	2.3
Religion	Christianity	28	21.9
	Islam	4	3.1
	Hinduism	1	0.8
	Buddhism	4	3.1
	Judaism	1	0.8
	Atheism	58	45.3
	Agnosticism	22	17.2
	Other	10	7.8

For all analyses in this experiment, the final sample excluded 3 participants, who failed to reproduce the intervals covarying monotonically with the actual action-tone intervals, thus final N = 125 (See data exclusion of Results for details about the exclusion criteria). This is the dataset to be used for all analyses of this Experiment 4.

Materials, design, and procedure

The materials, design and procedure were identical to which in Experiment 3.

Results

All analyses in this experiment were conducted in SPSS version 31.

Data exclusion

Similar to Experiment 3, to control for outliers in the interval reproduction task, we calculated the z-scores for each trial of every participant and eliminated the trials a) whose z-scores exceeded ±3, and b) trials with reproductions of less than 100ms (i.e., merely pressing down the space bar rather than holding it down to reproduce the interval), resulting in 2.94% of total trials removed. We then proceeded to control for outliers across participants that individuals with z-scores of their mean baseline errors and operant errors greater than ±3 were excluded from the primary analysis. There was no participant whose z-scores of errors of estimation which had exceeded the thresholds. Further following exclusion criteria was applied, consistently with previous studies involving the same methodology (Pascolini et al., 2021, 2025) that participants not producing reproductions covarying monotonically with

veridical intervals (i.e., $r < .4$) were excluded and we identified 3 participants who failed to meet the criteria. Thus this leaves the final dataset with N = 125.

Main temporal binding

As in Experiment 3, participants produced a greater degree of negative estimation error in the operant condition (M = -223.90, SD = 281.10) compared with the baseline condition (M = -90.19, SD = 271.98) $t(124) = 5.29$, $p < .001$, d=.47. Thus, we once again replicated the temporal binding effect (Figure 10).

Individual differences

We conducted a linear multiple regression using the enter method which forced all the four predictors, namely life satisfaction, self-efficacy, positive mood, and fatalistic determinism in the maximal model as observed in Experiment 3. The assumptions of regression for this model were checked. Multicollinearity was not problematic as the correlations between predictor variables were all less than .80 (see Table 16) and VIF was less than 10 (VIF ≤ 1.25). The histogram of residuals showed an approximately normal distribution, which suggests that the assumption was met (Appendix C). Homoscedasticity was likely met because the scatterplot showed a similar variance in residuals across predicted values (Appendix C). There were no multivariate outliers. The highest Cook's Distance value was .095.

Table 16.

Descriptive statistics and correlation coefficients between variables in the regression model of Experiment 4 (N = 125)

	Mean	SD	Temporal Binding	Satisfaction with Life	Self-efficacy	Positive Mood	Fatalistic Determinism
Temporal Binding	-133.71	282.71	.	.113	-.030	-.110	-.100
Satisfaction with Life	4.47	1.07		.	.271**	.409***	.054
Self-efficacy	2.83	.37			.	.292***	.075
Positive Mood	2.32	.50				.	.025
Fatalistic Determinism	2.52	.82					.

Note. SD = Standard Deviation. *p <.05; **p< .01; *** p <.001

The results suggested that our model was not significant $F(4, 120) = 1.70$, p = .154 while accounting for 5.4% of the variance observed in the temporal binding effect across

participants. However, even though the explanatory power of the model observed in the first study was not replicated, life satisfaction remained a significant predictor of the magnitude of temporal binding effect: β = .199, t = 2.01, p = .046. Positive mood was marginally non-significant: β = -.18, t = -1.84, p = .069. Neither self-efficacy β = -.022, t = -.24, p = .884 nor fatalistic determinism β = -.10, t = -1.17, p = .245 was a significant predictor.

Combined analysis of Experiment 3 and 4

The two experiments were identical regarding their designs and procedures, allowing the merging of the two datasets. Having combined the two experiments, we conducted a linear multiple regression using the same model with life satisfaction, positive mood, self-efficacy, and fatalistic determinism as the independent variables. The assumptions of regression for this model were also checked. Multicollinearity was not problematic as the correlations between predictor variables were all less than .80 (Table *17*.) and VIF was less than 10 (VIF ≤ 1.36). The histogram of residuals showed an approximately normal distribution, which suggests that the assumption was met (Appendix C). Homoscedasticity was likely met because the scatterplot showed a similar variance in residuals across predicted values (Appendix C). There were no multivariate outliers. The highest Cook's Distance value was .066.

Table 17.

Correlation coefficients between variables in the regression model of combined dataset Experiment 3 and 4 (Total N = 199)

	Mean	SD	Temporal Binding	Satisfaction with Life	Self-efficacy	Positive Mood	Fatalistic Determinism
Temporal Binding	-161.92	279.62	.	.151*	.060	-.085	-.108
Satisfaction with Life	4.43	1.13		.	.271***	.454***	.138
Self-efficacy	2.80	.38			.	.352***	.052
Positive Mood	2.33	.54				.	.054
Fatalistic Determinism	2.54	.84					.

Note. *p <.05; *** p <.001

The findings suggested that the model was significant F(4, 194) =4.01, R^2=.076, p = .004. Life satisfaction was significantly positively related to the temporal binding effect such that higher life satisfaction was predictive of weaker temporal binding effect β=.25, t = 3.18, p = .002; whereas higher positive mood was predictive of greater magnitude of temporal

binding β= -.22, t = -2.72, p = .007. Self-efficacy (β=.08, t = 1.08, p = .283) and fatalistic determinism (β= -.13, t = -1.93, p = .055) were not significantly predictive of the outcome in the model.

Interim Discussion for Experiment 3 and 4

Results from the regression models of Experiment 3 and 4 showed that the two predictors satisfaction with life and positive mood were significantly associated with the temporal binding effect, notably in opposite directions. That is, life satisfaction was positively correlated with temporal binding whilst positive mood was negatively correlated with binding magnitude. In other words, the more satisfied one was with their life, the weaker their temporal binding effect would be i.e., lower implicit agency. Meanwhile, more positive mood reported was associated with stronger temporal binding effect. These findings thus showed a possible nuanced dynamic of life satisfaction as trait, positive mood as state and the temporal binding effect.

It is rather a surprising finding that higher life satisfaction is predictive of lower temporal binding effect. This is because past studies often show that individual differences factors like aging, depressive trait, high hypnotisability are associated with weaker temporal binding (Lush et al., 2017; Mariano et al., 2024; N. J. Scott et al., 2022). Meanwhile, it has been found that research has shown that when individuals feel greater control over their lives i.e., having more autonomy, opportunity and self-expression, this enhanced explicit, general sense of control is associated with higher life satisfaction (Welzel & Inglehart, 2010). It was therefore reasonable to expect that individuals with higher life satisfaction would also demonstrate stronger temporal binding effect indexing pre-reflective, implicit sense of agency. However, findings from Welzel and Inglehart (2010) looked at the explicit, conscious evaluation of control over one's life as a whole whereas we are looking at the implicit, low-level sense of agency over motoric action-outcomes as indexed by temporal binding effect. Thus such sense of control over life and implicit sense of agency as two conceptually distinct variables may not relate to life satisfaction in the same way.

To explain how higher level of satisfaction with life would be linked with a weaker temporal binding magnitude, one possibility concerns the extent to which individuals actively monitor their goal pursuit. Kappes and Schattke (2022) discussed how people adjust their engagement with goals depending on perceived attainability and personal relevance and disengage effort when a goal is no longer felt to be worth pursing. Extending on this logic, it is possible that individuals with higher life satisfaction might perceive fewer unmet needs or

pressing goals that require active pursuit. These goals in question might pertain to Pacherie (2008)'s level of D-intention (reviewed in Chapter 1). Pacherie (2008) also describes that the D-, P- and M- intentions are interconnected so D-level processes could shape the plans monitored at the P-level. Thus, putatively it is therefore possible that reduced D-intention-level goal pursuit as driven by higher life satisfaction would feed downwards to reduce the degree of monitoring recruited at P-intention level, which putatively manifests as weaker temporal binding effect. It is worth noting however that this interpretation is speculative, but it does offer a possible theoretical account of the surprising direction of the life satisfaction-temporal binding relationship observed across Experiments 3 and 4.

Meanwhile, positive mood, as a temporary state, might provide a short-term boost to the temporal binding system underlying the sense of agency, potentially via modulating the speed of the internal clock i.e., composed of a pacemaker and accumulator enabling organism to anticipate and adjust to rhythmic phenomena (Buonomano & Laje, 2010). It has been demonstrated that temporal binding can be associated with the rate of the internal pacemaker: the internal clock slows down briefly after a voluntary action is made and then speeds up again (Wenke & Haggard, 2009). Aarts et al. (2012) showed that being primed with reward-related information via brief exposure to positive pictures increased temporal binding but notably, this effect was primarily pronounced for individuals with higher spontaneous blink rates, and such high blink rates indicate a slower internal clock. Thus their findings could help explain what we found here between the potential link between positive mood and temporal binding, although limitations are warranted because of our so-called 'positive mood', see limitation section below.

It is worth noting that satisfaction with life and positive mood are positively correlated (Table 14, 16 and 17), which may make it challenging to contextualise one without another. However, these two constructs are not entire overlapping. Life satisfaction can primarily originate from basic daily needs, including food, shelter and health being fulfilled while facing no adversities in these areas rather than mere positive mood (Maslow, 1943). It has been referred to as a cognitive evaluation of an individual's life and that life satisfaction can be related to yet partially independent of the positive or negative emotions (Diener et al., 1985; Lucas et al., 1996). Contextual influences, including small fluctuations in the participants' mood, on the other hand, have been shown to have relatively small effect on the validity of life satisfaction measure (Schimmack & Oishi, 2005; Schimmack et al., 2004; Eid & Diener, 2004). Thus, these suggest that it is possible to distinguish judgment of life satisfaction from positive mood and vice versa.

Limitation

Depression (i.e., persistent depressed mood, loss of interest or pleasure (W. Marx et al., 2023)) and depressive symptomatology were not constructs of theoretical interest in the present chapter. However, in Experiments 3 and 4, mood was assessed using the CES-D which is a measure originally designed to index depressive symptomatology in general population samples. This scale was chosen because it captures affective state over a one-week retrospective window, making it more sensitive to relatively stable mood than short-term state measures. However, we acknowledge that this represents a construct-measure mismatch such that the CES-D measures depressive symptoms specifically including low mood, anhedonia, sleep disturbance and hopelessness rather than mood more broadly defined. For 'positive mood', which we measured using the items that Huppert et al. (2009) included in the ESS Round 3 measure wherein they categorised them as 'positive affect' [although they did also refer to these questions as "positive and negative mood" on p. 303 (Huppert et al., 2009)]; and positive affect is more of a general term for feelings that encompass both moods and emotions (Diener et al., 2015). As such, findings from Experiments 3 and 4 relating to 'mood' are more accurately interpreted as reflecting subclinical depressive symptomatology and potentially conflating with related concepts of affect and emotions. This is therefore a substantial limitation of the present design and thus to address this Experiment 6 incorporated an additional validated mood measure to allow a cleaner assessment of mood independent of depressive symptomatology.

In summary, in Experiment 3 and 4, we found a dissociation between satisfaction with life and positive mood as predictors of temporal binding effect. While higher life satisfaction was associated with weaker temporal binding, greater positive mood was linked with higher binding effect. Positive mood may slow down the internal clock underlying temporal binding effect, whereas high life satisfaction may reduce the need for such vigilance, leading to the reversed effect. Building on these correlational findings, in Experiment 5 and 6, we aimed at utilising experimental manipulations to moderate individuals' level of life satisfaction and mood to establish causal relationship in addition to these correlations. We tested these hypotheses - whether inducing life satisfaction i.e., stable life trait, and positive mood i.e. emotional state could influence one's sense of agency as measured by the temporal binding effect in the similar dissociative directions which we observed in Experiment 3 and 4.

Experiment 5

Experiments 3 and 4 found that higher levels of life satisfaction were predictive of lower temporal binding. As this relationship was correlational, this made it challenging to determine whether life satisfaction could potentially causally influence temporal binding effect. In complement to the findings of Experiment 3 and 4, Experiment 5 therefore used a gratitude letter-writing manipulation, intended to increase life satisfaction, to test whether an experimentally induced increase in life satisfaction would correspondingly affect temporal binding.

There has been much research looking at the relationship between gratitude, which pertains to the subjective feeling of thankfulness and appreciation towards good things or other people in one's life (Emmons & Shelton, 2002). Many empirical studies show positive correlations between gratitude and life satisfaction even after controlling for personality, coping styles etc. (P. Sun & Kong, 2013; Wood et al., 2008, 2009). Meta-analysis showed that there was greater satisfaction amongst individuals who had undergone gratitude interventions (Diniz et al., 2023). Specifically, gratitude letter-writing has been shown to produce consistent increase in level of life satisfaction (Toepfer & Walker, 2009) and long-form gratitude writing activities have been found to produce greater level of life satisfaction than shorter list-based formats (Regan et al., 2023). The present study adopted the social gratitude letter paradigm from Regan et al. (2023), who demonstrated significant increases in life satisfaction among participants who wrote gratitude letters to benefactors over one week relative to an active control condition. Here, we assessed if participants with an increase in satisfaction with life by practising journalling would have lower temporal binding effect compared with those without life satisfaction increase. Two groups of participants were recruited (Gratitude exercise v. Control) and we measured their life satisfaction at time one (before intervention) and time two (after intervention) for both groups. After the intervention, all participants were invited in the lab-based session where they completed the Libet Clock task to measure temporal binding. In this experiment, we employed Libet Clock because this allowed us to disentangle action and effect binding since these could be driven by separate mechanisms and thus would be moderated by level of life satisfaction differently (Yoshie & Haggard, 2013). We could thus quantify the contributions of action shift and tone shift to the effects of life satisfaction intervention on the sense of agency.

Based on the correlational findings from Experiment 3 and 4, we expected that temporal binding effect would be lower for those with increase in level of life satisfaction

after the gratitude journal period. No specific predictions were made about whether type of journaling would affect action binding and tone binding differently. This was because Experiment 3 and 4 both used an interval reproduction method, which gives a single overall score for temporal binding rather than separate action and tone estimates. We acknowledge this meant that the correlation findings from Experiment 3 and 4 could not be used as a straightforward basis for predicting what would happen on the Libet clock measures thus we made no strong directional predictions in advance regarding differentiating action v. tone binding.

It is worth noting that we also included a measure of anxiety which is a future-oriented emotional state characterised by anticipatory cognitive, behavioural and affective changes reacting to uncertainty about a potential threat (Grupe & Nitschke, 2013). Since past research has also shown the effectiveness of gratitude intervention on reducing level of anxiety (Cregg & Cheavens, 2021; Heckendorf et al., 2019), we included anxiety measure as an extra exploratory variable here but note that it was not of a primary interest.

Method

Participants

A prior power analysis using G*Power (Faul et al., 2009) was conducted to determine the sample size needed and for a mixed MANOVA (Global effects) with 2 Response Variables (Action Binding and Tone Binding), 2 between-subject groups (Control v. Journal) with α=.05, power=.80 to detect a medium effect size V=.15; a sample of 68 participants was required. A total of 90 participants (*M age* = 20.60, *SD* = 5.49, 13 Men, 3 Non-binary) were recruited to account for dropouts since the experiment required a week-long commitment of journaling. Participants took part in return for course credits and were randomly assigned either to Control group or Journal group, with 44 participants (*M age* = 19.02, *SD* = .85, 8 Men, 2 Non-binary) in the Control group and 46 participants (*M age* = 22.11, *SD* = 7.36, 5 Men, 1 Non-binary) in the Journal group (See Table 18 for further demographic information).

Table 18.

Participant demographic characteristics of Experiment 5 (N = 90)

Variable	Category	n	%
Household income	Less than £20,000	14	16.1
	£20,000-£39,999	8	9.2
	£40,000-£59,999	18	20.7

Variable	Category	n	%
	£60,000-£79,999	12	13.8
	£80,000-£99,999	5	5.7
	£100,000+	9	10.3
	Prefer not to say	21	24.1
Ethnicity	White British	53	60.9
	Other White background	7	8.0
	African	3	3.4
	Caribbean	0	0
	Another Black/African/Caribbean	1	1.1
	Bangladeshi	0	0
	Chinese	2	2.3
	Pakistani	1	1.1
	Another Asian background	9	10.3
	Mixed/Multiple	10	11.5
	Other	4	4.44
Religion	Christianity	22	25.3
	Islam	3	3.4
	Hinduism	3	3.4
	Buddhism	0	0
	Judaism	1	1.1
	Atheism	38	43.7
	Agnosticism	11	12.60
	Other	9	10.1

For the primary analysis of temporal binding effect and the exploratory analysis, the final sample excluded 2 participants in the Control group and 1 participant in Journal group (see Results for data exclusion details), thus leaving the final dataset of N = 87 with N = 42 in the Control group and N = 45 in the Journal group. This is the dataset used for all analyses in this experiment.

Design

In this experiment, we focussed on using gratitude journaling as an intervention to improve life satisfaction, thus the between-subject independent variable was Group (Control v. Journal) whereby participants were randomly assigned to. We wanted to examine whether

an increase in satisfaction with life as a result of the gratitude journal may influence participants' implicit sense of agency, with the temporal binding effect as the proxy for agency. Using the Libet Clock, we could measure not only the temporal binding effect as a whole but also the action binding and outcome (tone) binding separately. That said, we also calculated a single dependent variable which would be the overall temporal binding score by combining action and tone binding together. In the Libet Clock task, there were four sub-blocks (Baseline Action, Baseline Tone, Operant Action, Operant Tone). These were presented in a pseudorandomised approach using a 4x4 Latin Square where each possible sequence was presented for across all participants.

Materials

Gratitude journaling.

We adopted the intervention paradigm using gratitude letters by (Regan et al., 2023). For the Journal group, the instructions to journal were to write a letter expressing gratitude to a benefactor ("*There are many people in our lives, both close and distant, that we might be grateful about. Please take a moment to think back over the past several years of your life and remember an instance when someone did a kind act (or acts) for you for which you are extremely grateful. Think of the people – parents, children, spouses/partners, relatives, friends, neighbours, teachers, employers, and so on – who have been especially generous and thoughtful towards you. For example, you may feel grateful for a friend who gave you a compliment or a parent who helped you. Now, write a letter to one of these individuals.")* (For more detailed instructions, Appendix D). For the control group, the instructions were focussed on keeping track of daily activities ("*Please take a moment to think about what you did yesterday. That is, create a mental outline of what you did during that time. Now please write these activities out in a list format.*") (Appendix D).

Questionnaires.

Similar to Experiment 3 and 4, we utilised the 5-item Satisfaction with Life scale to measure participants' level of life satisfaction (Diener et al., 1985) as well as the items measuring Positive Mood adapted from the depressive symptoms scale of the Centre for Epidemiological Studies-Depression (CES-D). In this current experiment, we further included an anxiety scale - the Beck Anxiety Inventory, a 21 item self-report scale (*Indicate how much you have been bothered by that symptom during the past month, including today: 'Heart pounding/racing'* etc.) using a 4-point Likert scale where 1 = *Not At All* to 4 = *Severely – it bothered me a lot.* For each participant, a grand sum was scored by adding all

the items. We also measure similar demographic questions including age, gender, household income, ethnicity, and religion as shown in the previous experiments.

Libet Clock.

To measure temporal binding effect, we employed Libet Clock, identical to which in Experiment 1 of Chapter 3. The task was comprised of four conditions, with two termed Operant (Operant Action and Operant Tone) and the other two Baseline conditions (Baseline Action and Baseline Tone). The Libet Clock task involved a clock face with a clock hand rotating in a clockwise direction, at the speed of 2560ms/full rotation (See Figure 4). The clock face circle had a diameter of 9.5° and the clock hand was 4.1° in length. The Tone used was a sine wave tone with 100ms duration at 1000Hz. This experimental task was conducted with a Dell 1909W monitor (the experiment was run full-screen, size of the monitor was 19 in. with 1440 x 900 resolution and refresh rate at 60 Hz), a keyboard, and a computer mouse.

Procedure

Upon signing up for the study, participants were asked to sign up for both the first time slot i.e., the initial date of the one-week long journaling period, and the second part of the study which was the in-person lab-based experiment taking place on the day after the 7-day period of journaling had ended. Having signed up, participants were randomly and automatically assigned to either the control and journal group; and they were sent the detailed respective instructions on how to journal, depending what group they were in. They were also instructed to complete immediately (before starting the journaling period) the questionnaires via a link attached in the email which we used to measure pre-intervention level of life satisfaction, positive mood and anxiety, as well as demographic questions including age, gender, household income, ethnicity, and religion. In the Gratitude group, participants wrote a letter to a person towards whom they were grateful for, and they completed this each day for a week, and they did so at home. The control group were writing about their daily activities. All participants were instructed to write their journaling on a notebook or papers and ensure to add time stamps i.e., the date of the journaling, meaning that there should be seven days' worth of journalling for each participant. After the intervention i.e. one week after the initial sign-up date, participants were invited to the lab in person to take part in the temporal binding task, as measured using the Libet Clock paradigm. Upon arrival, participants were asked to show proof of journalling which they should have completed every day for the last week by presenting the papers or notebooks on which they had used to journal to the experimenters. It is worth noting that while we asked participants to journal, we did not access or analyse the actual content of the journal.

All participants who attended the lab-based session one week after the journalling period fully completed their assigned everyday journaling task. They were then given the instructions on the Libet Clock task and were told that they would see a clock face on the screen with a rotating clock hand. Procedure for the Libet Clock paradigm was identical to which of Experiment 1. There were two practice trials for each of the four blocks, resulting in 8 practice trials, which were not analysed. For the main trials, participants completed 15 trials for each block, totalling 60 trials for entire the Libet Clock task.

At the end of the lab-based experiment, participants were asked to fill out questionnaires which measured the participants' post-intervention level of life satisfaction, positive mood and anxiety. All participants were then thanked and debriefed.

Results

Calculating binding from the Libet Clock

Temporal binding calculated here was identical to which in Experiment 1. To recap, for each trial, we calculated Judgement of Error (JE) i.e., the temporal difference which was the spatial discrepancy between the actual and estimated hand clock positions of the target event. For each participant, action binding was calculated by subtracting the mean JE in Baseline Action condition from the mean JE in Operant Action. Positive values of action binding indicated action binding effect that the key press was perceived by the participants to have occurred later in the Operant Action condition compared with the Baseline Action condition. Tone binding for each participant was calculated by subtracting the mean JE in Baseline Tone condition from the mean JE in Operant Tone. With the tone binding effect, this would mean that the tone binding values would be negative since participants would perceive the tone to take place earlier in the Operant Tone condition compared with the Baseline Tone condition. Having calculated Tone Binding and Action Binding, we could then calculate the Total Binding effect. The total binding scores represents the overall sense of binding across both action and outcome, showing how action and tone were bound together closer in time. The more negative the total binding score, the greater implicit sense of agency (Haggard et al., 2002).

Preliminary analysis and manipulation checks

Effect of Journaling Intervention on Life Satisfaction, Mood and Anxiety.

To check whether our manipulation of the week-long journaling type (Control i.e., listing out daily activities v. Journal i.e., writing letters expressing gratitude to a benefactor)

significantly increased level of life satisfaction, we conducted a two-way mixed ANOVA (within-subject factor: Before v. After week-long journaling × between-subject factor: Group Control v. Journal) on life satisfaction. Results showed that there was a significant main effect of time that life satisfaction significantly increased after one week (M = 4.75, SE = .13) compared with before the intervention (M = 4.45, SE = .12): $F(1, 88) = 17.34$, $p < .001$, $\eta p^2 = .17$. However, the main effect of Group i.e., journaling type was not significant $F(1, 88) = 2.12$, $p = .149$, $\eta p^2 = .02$, nor was the interaction effect $F(1, 88) = .41$, $p = .523$, $\eta p^2 = .01$. This suggests that the journaling intervention did not work as expected: the Gratitude Journal group did not have greater increase in life satisfaction after journaling compared to the Control group; and both group of participants had greater level of satisfaction with life post the week-long intervention.

ANOVAs with similar independent variables but on positive mood and anxiety level as the outcomes were conducted to examine whether the intervention type had any effect on participants' positive mood or anxiety after intervention compared with before intervention. We also found similar results that there were significant main effects of Time (before v. after intervention) on both positive mood $F(1, 88) = 17.34$, $p = .013$, $\eta p^2 = .07$ ($M_{\text{Mood before intervention}} = 2.35$, $SE = .05$; $M_{\text{Mood after intervention}} = 2.46$, SE = .06) and anxiety $F(1, 88) = 25.58$, $p < .001$, $\eta p^2 = .23$ ($M_{\text{Anxiety before intervention}} = 17.76$, $SE = 1.36$; $M_{\text{Anxiety after intervention}} = 14.59$, $SE = 1.34$); and we did not find any significant main effect of Group i.e., journaling type or interaction effect (ps≥.366). Altogether, this means that regardless of what Journaling group participants were assigned to, they reported significantly greater level of life satisfaction, positive mood and lower level of anxiety after completing the week-long journaling exercise.

Data exclusion.

For each participant, we excluded trials for each of the four Operant Action, Baseline Action, Operant Tone and Baseline Tone blocks where z-scores of JE > ± 3SDs from the mean JE of that participant, resulting in 96 trials removed in total i.e., 1.78% of trials across 90 participants. Participants with either z-scores > ± 3SDs from the mean action binding or tone binding of all participants would also be removed from the temporal binding analysis. 1 participant in the Control group had z -score for action binding exceeding 3SD. 1 participant in the Control group and 1 participant in the Journal group had z-scores for tone binding exceeding 3SDs. Thus this leaves the final dataset of N = 87 with N = 42 in the Control group and N = 45 in the Journal group.

Temporal Binding

We conducted a MANOVA with the two-level between-subject independent variable of Group (Control v. Journal) on two repeated-measures dependent variables (Action Binding and Tone Binding). Using Pillai's trace, there was a significant effect of Group on the magnitude of action and tone binding, V=.072, $F(2, 84) = 3.27$, $p = .043$, $\eta p^2 = .07$. Separate univariate tests on the outcome variables revealed that while Group did not have a significant effect on action binding $F(1, 85) = .62$, $p = .433$, $\eta p^2 = .01$; being assigned to Journal v. Control group did significantly influence tone binding $F(1, 85) = 6.00$, $p = .017$, $\eta p^2 = .07$.

Pairwise comparisons showed that for the Journal group (M = -108.03ms, SE = 14.60) had significantly weaker temporal binding effect compared to the Control group (M = -159.36ms, SE = 15.11) (p = .017). Thus, participants who practised journalling by writing a letter to express gratitude towards a benefactor as the intervention to enhance life satisfaction had weaker tone binding effect (which is the proxy for the implicit sense of agency) compared with their counterpart. A further analysis to check if total binding effect differed between the two groups and results showed that total binding of the Control group (M = -171.70, SE = 21.86) was indeed significantly more negative than total binding of the Journal group (M = -125.00, SE = 21.81) $t(85) = -2.14$, $p = .036$, $d = -.46$ (2-tailed).

Exploratory analysis

We conducted a linear multiple regression to assess if a) Group: Control v. Journal, b) life satisfaction level measured after intervention, c) change in life satisfaction (Δ Life Satisfaction) along with their interactions with Group could predict level of tone binding. This particular analysis was performed in R version 2023.03 with R packages using standard stats package with lm() function to fit the linear regression model; car package to check VIF:

Tone binding ~ Group (Control v. Journal)

+ *Life Satisfaction after intervention*

+ Δ *Life Satisfaction (after-intervention – post-intervention)*

+ *Group* × *Life Satisfaction after intervention*

+ *Group* × Δ *Life Satisfaction*

Here, Group was dummy-coded (Control = 0, Journal =1). The assumptions of regression for this model were checked. Multicollinearity was not problematic, as the correlations between the continuous main predictor variables were less than .80 (see Table

19) and VIF values (computed using the GVIF-adjusted method appropriate for models with interaction terms) were all below 3 (VIF ≤ 2.91). The histogram of residuals showed an approximately normal distribution, which suggests that the assumption was met (Appendix E). Homoscedasticity was likely met because the scatterplot showed a similar variance in residuals across predicted values (Appendix E). There were no multivariate outliers. The highest Cook's Distance value was .31.

Table 19.

Descriptive statistics and correlation coefficients between continuous variables in the exploratory regression model of Experiment 5

	Overall (N = 87) Mean (SD)	Control (N = 42) Mean (SD)	Journal (N = 45) Mean (SD)	Tone Binding	Life Satisfaction after intervention	Δ Life Satisfaction	Group
Tone Binding	-132.80 (100.73)	-159.36 (101.0)	-108.03 (95.4)	.	.172	.051	.256*
Life Satisfaction after intervention	4.75 (1.24)	4.58 (1.37)	4.90 (1.11)		.	.398***	.130
Δ Life Satisfaction	0.30 (0.69)	0.34 (.61)	0.27 (0.76)			.	-.049
Group							.

Note. * $p < .05$, *** $p < .001$. SD = Standard Deviation

Results showed that the overall model was significant, $F(5, 81) = 3.06$, $p = .014$, $R^2 = .16$. Similar to above Group was a significant predictor of tone binding that the Journal group had weaker tone binding compared with which of Control group ($\beta = 265.27$, $t = 3.04$, $p = .003$). Change in life satisfaction was not predictive of tone-binding ($\beta = .60$, $t = .02$, $p = .983$) and Group × Change in life satisfaction was also not significant ($\beta = .3.90$, $t = .11$, $p = .910$). However, there was a significant interaction effect of Group × Life Satisfaction after intervention $\beta = -45.81$, $t = -2.46$, $p = .016$. In particular, in the Control group, higher life satisfaction level after intervention was significantly predictive of weaker tone binding $\beta = 29.88$, $t = 2.43$, p = .017, consistent with the link between life satisfaction and temporal binding proposed Experiment 3 and 4. In the Journal group, this relationship was attenuated (with the slope in numerically reversing direction) and did not reach significance $\beta = -$

15.93,t = -1.41, p = .257. This could suggest that the Journal group did not show the relationship between life satisfaction and tone binding seen in Control which could suggest that gratitude writing might override such link.

Discussion

In this experiment, we aimed to test the hypothesis that higher life satisfaction might lead to lower temporal binding effect as derived from the correlational results in Experiment 3 and 4 by directly manipulating the level of life satisfaction amongst participants using gratitude journaling for one week. Firstly, regarding the outcome of the intervention, contrary to our expectations, we observed no difference in the level of satisfaction with life after seven days of journaling between the Control group, who wrote a list of activities they had done every day and the Journal group, who were instructed to write letters to a benefactor towards whom they were grateful for. In fact both groups showed significant increase in life satisfaction level after intervention compared with pre-intervention. However, we did find that participants who wrote the gratitude letters had significant weaker temporal binding effect, particularly tone binding, than the Control group.

We conducted further exploratory analysis examining whether life satisfaction level after the intervention (T2) and change in life satisfaction over the course of the intervention, alongside their interactions with Group, were predictive of tone binding. Findings showed that change in life satisfaction was not a significant predictor of tone binding in either group. Instead it is the level of life satisfaction concurrent with the temporal binding measure rather than change over the intervention period that relates meaningfully to tone binding. In particular, for those in the Control group, higher level of life satisfaction after intervention was predictive of weaker tone binding effect, consistent with the correlational pattern observed in Experiment 3 and 4 (such that higher life satisfaction might reduce the need for goal pursuit). In contrast, in the Journal group this relationship was attenuated and did not reach significance. Thus, this suggests that the type of Journal group might a) moderate general level of tone binding effect (Control group had greater tone binding than Journal) and b) moderate the relationship between life satisfaction and tone binding. These findings are however exploratory in nature and should be interpreted with caution rather than as confirmatory.

Limitations

We did not include pre-manipulation baseline temporal binding. At the time of the study, the researcher could only run the Libet Clock in person so it was necessary to invite

the participants in the lab to get temporal binding measure using Libet Clock. Inviting all 90 participants in lab twice and asking participants to perform journaling every day for seven days a week was too resource- and time-consuming. We fully acknowledge that pre-manipulation Libet clock to measure binding before journalling intervention would be needed because without a baseline (T1) measure of binding, we cannot reliably attribute any group effects to the intervention. Thus, while Experiment 5 aimed to move beyond the correlational design of Experiments 3 and 4 by experimentally manipulating life satisfaction, with no pre-manipulation temporal binding baseline this means the study cannot determine whether a change in life satisfaction produced a corresponding change in temporal binding magnitude which was the central causal question motivating the design. As such, the present findings remain correlational in nature (comparing temporal binding and life satisfaction level after the intervention, across groups) rather than constituting a true experimental test of whether increasing life satisfaction causes a change in temporal binding. Future studies should ensure to include baseline binding and researchers could use a remote/digital binding paradigm (now more developed see Galang et al. (2021); Cubillas et al., (2020); Soral et al., (2021)).

A further limitation concerns the design of the two journaling conditions. Following Regan et al. (2023), the gratitude letter condition differed from the control condition not only in gratitude content but also in timescales (many years v. yesterday); writing format (long-form letter vs. brief list) and social target (writing to a benefactor thus meaningful vs. merely recalling one's own daily activities). As Regan et al. (2023) acknowledge these features may each independently contribute to life satisfaction outcomes and cannot be fully disentangled in the current experiment. Future research could address this by including conditions that systematically isolate each feature listed above. Additionally, we only asked participants when attending in lab to show us the timestamps on their journaling notebooks as we did not wish to analyse the content. This is a limitation because such journaling tracking methods did not enable robust checking of intervention compliance, since timestamps etc. in a handwritten notebook could have been forged the day before. Future research should facilitate an online platform/electronic form, for instance, where participants could fill in their journaling every day and once they click submit the content would not be saved but the timestamps of submission will be.

Lastly, it should be noted that Experiment 5 employed the Libet-clock, differing from the binding task version used in Experiment 3 and 4. Differences in temporal binding task might affect the sensitivity or construct coverage of the temporal binding measure and findings from Experiment 5 should therefore be integrated with caution alongside the broader

thesis findings rather than treated as directly comparable in magnitude or mechanism to results obtained using the interval reproduction paradigm.

Experiment 6

In Experiment 6, we aimed to directly test the correlational results from Experiment 3 and 4 that positive mood was associated with greater temporal binding effect. Thus, we focussed on inducing positive mood through excerpt of film which was expected to facilitate positive affect, and we tested whether being in a positive mood would increase the temporal binding effect, in similar direction as which of Experiment 3 and 4. We employed the within-subject design so that all participants would view both Neutral and Positive videos (one-week apart to account for carry-over effect) and perform the Libet Clock task following the viewing of both videos to measure the temporal binding effect at both times. Like Experiment 5, using Libet Clock allowed us to quantify the action shift and outcome shift in time and how positive mood affected these two mechanisms as separate units. It was expected that unlike life satisfaction, positive mood was speculated to enhance the temporal binding effect.

Method

Participants

We conducted a prior power analysis using G*Power (Faul et al., 2009) to determine the sample size required to detect a medium effect size dz=.5 with α=.05, power=.80 for a repeated-measures t-test (two-tailed) to assess difference in temporal binding magnitude after watching neutral v. positive video; a sample size of 34 was needed. 34 participants (*M age* = 26.40, *SD* = 7.16, 8 Men, 3 Non-binary) were recruited and paid at the rate of £12/hour in exchange for their participation (See Table 20 for further demographic information). This is also the final dataset used for all analyses of Experiment 6.

Table 20.

Participant demographic characteristics of Experiment 6 (N = 34)

Variable	Category	n	%
Household income	Less than £20,000	7	20.6
	£20,000-£39,999	5	14.7
	£40,000-£59,999	5	14.7
	£60,000-£79,999	4	11.8
	£80,000-£99,999	2	5.9
	£100,000+	3	8.8

Variable	Category	n	%
	Prefer not to say	8	23.5
Ethnicity	White British	23	67.6
	Other White background	2	5.9
	African	1	2.9
	Caribbean	0	0
	Another Black/African/Caribbean	0	0
	Bangladeshi	0	0
	Chinese	2	5.9
	Pakistani	0	0
	Another Asian background	3	8.8
	Mixed/Multiple	2	5.9
	Other	1	2.9
Religion	Christianity	8	23.5
	Islam	1	2.9
	Hinduism	0	0
	Buddhism	1	2.9
	Judaism	0	0
	Atheism	15	44.1
	Agnosticism	6	17.6
	Other	3	8.8

Design

Experiment 6 focussed on the effect of positive mood on the magnitude of temporal binding using the within-subject design (Positive v. Neutral) whereby all participants viewed a Positive film excerpt and a Neutral video; and they completed the Libet Clock task following the video watching. We used counterbalancing so that half of the participants were randomly assigned to viewing the Positive video first in part one of the lab-based study then viewing the Neutral video in part two one week after; or vice versa for the other half of the participants. Similar to Experiment 5, the outcome variables were the magnitude of Action Binding and Tone Binding as measured using the Libet Clock paradigm; and we also calculated single dependent variable, which was the Total Binding effect by combining Action and Tone Binding. Like Experiment 5, in the Libet Clock task, there were four sub-blocks (Baseline Action, Baseline Tone, Operant Action, Operant Tone). These were

presented in a pseudorandomised approach using a 4x4 Latin Square where each possible sequence was presented for across all participants.

Materials

Videos.

To induce positive affect, we used the film clip suggested in Schaefer et al. (2010) which was the film excerpt from the movie called 'Something About Mary' wherein the main scene was Ben Stiller fighting with a dog, and such movie extract was three-minute long and it was rated to have induced a high level of amusement (Schaefer et al., 2010). For the neutral condition, we utilised a video with neutral content that there was a quiet street scene with minimal activity and steady ambient sound to prevent engagement, low arousal and familiar settings to help maintain the viewers' neutrality, based on a meta-analysis by Fernández-Aguilar et al. (2019). The neutral video also lasted for three minutes, matching with the duration of the Positive movie excerpt video.

Questionnaires.

Following Schaefer et al. (2010), we included a self-reported question to measure the intensity of subjective emotional arousal on a 7-point scale: '*While I was watching the film ...*' (1) '*I felt no emotions at all*' to (7) '*I felt very intense emotions*'. We also included the Differential Emotions Scale to examine the discrete emotional dimensions (DES, adapted version by (McHugo et al., 1982)) after the end of the videos asking participants to rate the extent to which they felt each state as they were watching each of the clips. There were 10 items and each was a group of emotional adjectives: (1) *interested, concentrated, alert*; (2) *joyful, happy, amused*; (3) *sad, downhearted, blue*; (4) *angry, irritated, mad*; (5) *fearful, scared, afraid*; (6) *anxious, tense, nervous*; (7) *disgusted, turned off, repulsed*; (8) *disdainful, scornful, contemptuous*; (9) *surprised, amazed, astonished*; (10) *warm hearted, gleeful, elated*. For each group of adjectives were rated on a 7-point scale (1='*not at all'*, 7='*very intense*'). Positively worded group of adjectives were (1), (2), (9) and (10); whilst negatively worded adjectives i.e., group (3), (4), (5), (6), (7) and (8). The Differential Emotions Scale (DES) was included as a secondary manipulation check to verify that the positive condition clip induced positive emotional responses relative to the neutral condition complementing the primary mood measures. As discrete emotion was not a primary theoretical focus of this experiment, DES items were collapsed into composite positive and negative emotion scores consistent with Schäfer et al. (2010) who adopted the same approach in their analyses of emotion responses to film clips. These composite scores served to confirm the effectiveness

of the mood induction in affective terms rather than to test hypotheses about specific discrete emotions.

In this experiment, we included further an additional measure of mood using the Brief Mood Introspection Scale (BMIS) since the depressive symptom's questionnaire adopted from (CES-D) in Experiment 3 and 4 did have some measure of mood but they are designed to measure depression (having acknowledged this as a limitation). Thus, the BMIS would allow us to measure more validly mood states with 16 items such *as 'Grouchy', 'Fed up', 'Lively', 'Happy'* etc. Participants rated on the scale from 1 ='*Definitely do not feel'* to 4 ='*Definitely feel'* on how well each adjective/phrase described their current mood. Eight positively worded items ('lively', 'happy' etc.) were computed into the mean Pleasant whilst the eight negatively worded items ('sad', 'tired' etc.) were computed into the Unpleasant subscale mean for each participant.

We also used similar questionnaires as Experiment 3 and 4, which were both negative/positively worded items from Depressive Symptoms (CES-D); Satisfaction with Life (Diener et al., 1985), age and gender as the demographic measures.

Libet Clock.

The materials for the Libet Clock task were identical to which of Experiment 5.

Procedure

All participants were asked to sign up for both part one and part two of the study, which had to be seven days apart. Both parts of the study were in person. In the first part, participants were invited in the lab, briefly explained about what to expect from the experiment and asked to sign the consent form. They were then asked to fill out the pre-video mood BMIS 16-item questionnaire to measure baseline mood prior to any video watching. Afterwards, they were informed that a video was going to be shown but it was only going to be played once, so that they should watch the video with full attention, and participants were instructed to press the space bar to play the video when they were ready. As previously mentioned, half of the participants would watch the Positive video in this first part of the study (Something About Mary movie excerpt, three-minute long) whilst the other half viewed the neutral ambient street-scene with minimal activities (also three minutes in duration) in this part one; and they were randomly assigned. At the end of the video, participants were asked to rate the emotion intensity level of the video they had just watched i.e., if they were feeling intense or no emotion at all whilst viewing the video. They were also asked to rate

during watching that video, how much of positive emotions (like 'Joyful, happy, amused') and negative emotions ('Sad, down-hearted, blue') they felt, using the DES scale.

We then measured post-video mood 16-item BMIS again to assess participants' mood after watching the video as this would allow us to understand how the video participants just viewed changed participants' mood. Having completed the post-video BMIS, participants performed the Libet Clock task in similar procedure as which of Experiment 5, where the first practiced with 8 trials and completed 60 of main trials in total for the four blocks of the Libet Clock. Following this, participants were asked to fill out the final set of questionnaires which was comprised of the mood measure adapted from Depressive Symptoms (CES-D); Satisfaction with Life (Diener et al., 1985), age and gender questions.

One week afterwards, participants were invited to the lab again to complete the part two of the study. The procedure of the second part was identical to which of the first part, except for the video presented that participants would view whichever video (Neutral or Positive) that they had not watched in the first part. We also measured pre- and post-video mood using the BMIS; the perceived intensity of the video shown in this part two; the temporal binding via Libet Clock; the CES-D mood measure and Satisfaction with Life again in this second part. At the end of the part-two session, participants were thanked, debriefed and paid.

Results

Preliminary analysis and manipulation check

Level of Emotional Intensity.

First, we checked the difference in the intensity of subjective emotion between the two video types reported by participants. Results suggested that participants did rate greater emotion intensity level of the positive video they had just watched (i.e., if they were feeling intense or no emotion at all whilst viewing the video) (M = 4.68, SE = .25) compared to the neutral video (M = 2.79, SE = .21) $t(33)$ =8.96, $p < .001$, d=1.54(two-tailed). Participants also reported significantly greater level of positive emotions (measured using the DES scale) whilst watching the Positive video (M = 4.32, SE = .21) than when watching the Neutral video (M = 2.74, SE = .18) $t(33)$ =7.57, $p < .001$, d=1.30(two-tailed). No significant difference in reported negative emotions between during watching the Positive (M = 2.08, SE = .21) and the Neutral video (M = 1.79, SE = .15) $t(33)$ =1.59, p = .121, d=.27(two-tailed).

Effect of Video Types on Mood.

As having stated, we included two mood measures in this experiment, positively worded items from Depressive Symptoms (CES-D) and Brief Mood Introspection Scale (BMIS). Firstly, a paired t-test was conducted to examine whether positive mood from CES-D after watching the positive video (M = 2.31, SE = .08) would differ from positive mood after watching the neutral video (M = 2.31, SE = .09), but we did not find a significant difference $t(33)$ =.46, p = .922, d=.02 (two-tailed). Regarding the BMIS, participants reported significantly more pleasant mood after watching the positive video (M = 4.36, SE = .17) compared with post neutral video watching (M = 4.00, SE = .18) $t(33)$ =2.98, p = .005, d=.51(two-tailed). Unpleasant mood was also significantly lower after watching the positive video (M = 2.28, SE = .20) compared with post neutral-video (M = 2.75, SE = .21) $t(33)$ = -4.35, p < .001, d= -.75(two-tailed). Level of satisfaction with life did not differ between the one-week period i.e., first (M = 4.41, SE = .25) v. second video watching session (M = 4.41, SE = .25) $t(33)$ =.10, p = .955, d=.01(two-tailed).

We further conducted analyses to examine whether the order in which participants viewed the clips i.e., watching the positive video first versus the neutral video first would influence the temporal binding effect at each time point. Specifically, we ran separate 2×2 ANOVAs for both action binding and tone binding, with factors being video order (positive-first vs. neutral-first) and binding type (binding after watching the positive video vs. binding after watching the neutral video). Results revealed no significant interactions between the type of video viewed first and the magnitude of the action binding effect following each video $F(1, 32)$ =.65, p = .426, ηp^2 =.00. Similarly, no significant interactions were found for tone binding $F(1, 32)$ =.00, p = .991, ηp^2=.00. These findings confirmed the counterbalancing that watching which video first did not influence the binding effects.

Data exclusion.

Similar to Experiment 5, for each participant, we excluded trials where z-scores of JE>± 3SDs from the mean JE of that participant for each of the four Operant Action, Baseline Action, Operant Tone and Baseline Tone in both Libet Clock tasks that followed the viewing of the two videos, resulting in 1.12% of trials across 34 participants. At participant level, individuals with z-scores > ± 3SDs in any of the four variables: mean action-binding/tone-binding post-neutral/positive video were excluded from the temporal binding analysis. No participants in this experiment had any z-scores of the four mean binding values to exceed 3SDs, thus leaving the final dataset of N = 34.

Temporal Binding

For this experiment, paired t-tests were conducted to examine whether after watching the positive video would influence binding effect (action, tone and total) compared with after watching the neutral video. Results showed no significant difference in any of the temporal binding measures between the two repeated-measures video conditions (See Table *21*). thus, our manipulation of video watching to induce positive mood did not influence participants' temporal binding effect.

Table 21.

Results of paired t-tests compared binding effects after watching positive v. after neutral video.

	Mean (ms)	*SE*	Paired t-test results (two-tailed)
Action binding after Positive video	21.79	7.67	$t(33) = .82$, $p = .419$,
Action binding after Neutral video	16.38	7.13	$d = .14$
Tone binding after Positive video	-148.34	14.18	$t(33) = .98$, $p = .333$,
Tone binding after Neutral video	-157.52	12.44	$d = .17$
Total binding after Positive video	-170.13	15.77	$t(33) = .32$, $p = .748$,
Total binding after Neutral video	-173.91	13.08	$d = .06$

Discussion

This experiment aimed to test the correlational findings from Experiment 3 and 4 that positive mood was associated with greater temporal binding effect; by establishing a causal relationship through mood induction. Participants watched both a neutral and positive video clip, which was spaced one week apart to minimise the carry-over effect. After watching each video, they completed the Libet Clock task that we used to measure temporal binding. As a manipulation check, mood was measured using two scales, the BMIS and the positive mood items adapted from the Depressive Symptom questionnaire used in Experiment 3 and 4. Participants reported significantly higher positive mood on the BMIS scale after watching the positive video compared with the neutral clip which indicated that the mood induction was effective according to this measure. Nevertheless, mood ratings based on the Depressive Symptom questionnaire, used in Experiment 3 and 4, did not differ between the two video

conditions. Most importantly, there was no significant difference in the temporal binding effect between the positive and neutral conditions. That is, the magnitude of action binding, tone binding and thus overall temporal binding remained comparable across both video conditions.

We wish to also clarify the conceptual distinction between mood and emotion here in Experiment 6. The primary construct of interest in this experiment was positive mood to be induced by the film clip manipulation rather than discrete emotion. Mood and emotion are related but distinct constructs that while emotions are brief, intense and directed toward a specific eliciting stimulus; moods are lower in intensity and persist over a longer period (Davidson, 1994; Frijda, 1993). The film clip was selected specifically as a mood induction procedure intended to create a positive affective background state as film clips have been shown to be greatly potent inducers of both positive and negative mood states; although watching the film clip would indeed also lead to experiencing specific emotions such as joy or amusement (Schaefer et al., 2010; Westermann et al., 1996).

General discussion

The current research investigated the individual differences in the temporal binding effect as an implicit measure of agency. In both online Experiment 3 and 4, we used the interval reproduction paradigm to measure the general temporal binding system and included a variety of individual differences factors, including satisfaction with life, self-efficacy, free will, fatalistic determinism, spheres of control and mood. We conducted exploratory analyses in Experiment 3 with backward regression on the temporal binding effect as the outcome variable and found that the model of best fit comprised of four predictors: satisfaction with life, positive mood, self-efficacy and fatalistic determinism. Experiment 4 was aimed at replicating the results from Experiment 3 and we found that life satisfaction was a significant predictor of the magnitude of temporal binding. Combined analyses from both experiments (as they were identical in methods) showed that life satisfaction and positive mood were significant predictors but in opposite directions. That is, the greater level of satisfaction with life was associated with weaker temporal binding, whereas more positive mood was predictive of stronger binding effect. Thus, we further conducted experiments that directly manipulated the level of satisfaction with life and positive mood, and we assessed how changes in the trait (life satisfaction) and state (positive mood) could influence temporal binding. It is worth noting that we employed the Libet Clock task in later experiments to

measure implicit agency because we could then quantify the effects of changes in life satisfaction and mood on action and tone binding separately.

In Experiment 5, we aimed to test whether higher life satisfaction would lead to weaker temporal binding by directly manipulating life satisfaction through gratitude journaling over one week. Contrary to our expectations, the intervention did not produce differential changes in life satisfaction between the Control and Journal groups and that both groups showed significant increases in life satisfaction following the intervention. However, participants in the Journal group did show significantly weaker tone binding compared to the Control group. Further exploratory analysis showed that change in life satisfaction was not predictive of tone binding.

In contrast, in Experiment 6, we did not find the expected causal relationship between increase in positive mood and enhanced temporal binding effect as observed in the first two experiments. Participants did not report stronger binding after watching the positive movie excerpt compared to after watching a neutral clip with minimal level of stimulation.

Self-focused v. Other-focused journaling

Experiment 5's key findings showed that participants who only listed out activities every day for one week were also the individuals with greater level of tone binding effect than those who wrote gratitude letters. Thus, one possible way to explain this is to conceptualise the type of journaling: Control as self-focused v. Gratitude journaling as others-focused. Thus such distinctive type of journaling might potentially moderate temporal binding effect. Past research has suggested that internal locus of control stems from how one perceives that outcomes are determined by their own actions, and by extension, life experiences (Dong et al., 2022; Golding, Gregory, Ellis, et al., 2017). It is possible that by listing out activities which had been accomplished, this perhaps allowed participants of the Control group to retrospectively evaluate the outcomes of their actions, which as a result strengthened the perceptual action-effect link (K. A. Schwarz et al., 2018). On the other hand, external locus of control focuses on other sources, particularly other people (Golding, Gregory, Iles-Caven, et al., 2017). Writing to other to express gratitude means that the things having been achieved would be a result of others, not the self. One could argue that the gratitude journaling intervention adapted here (writing letters to benefactors) held more emphasis on the external source of gratitude, leading to a shift in locus of control from internal (Lefcourt, 1991). Being grateful to an external benefactor might have taken the spotlight on the 'self' away, while the 'self' element happens to be most central to the agency system, the development and maintenance of sense of agency (De Vignemont & Fourneret,

2004; Pacherie, 2008; Ryon & Gleason, 2014). Such external shift in locus of control could possibly explain for the weaker temporal binding effect for those in the Journal group. It is important to note that locus of control was not directly measured in the current experiment so such reference to internal/external orientation is only speculative in nature.

Tone binding

Here we found that action binding did not differ between journaling groups whereas tone binding of the Control (self-focused journaling) group was greater than which of the Journal (others-focused group). Past research has shown that reductions in action binding are typically associated with psychological distancing from outcomes linked to negative valence (Christensen et al., 2019; Render & Jansen, 2021). Here, no negative valence was present, in fact, life satisfaction and positive mood increased, while negative affect such as anxiety decreased, which might explain why action binding remained intact. Tone binding, on the other hand, is dependent on the degree to which participants can predict and monitor outcomes (Tanaka et al., 2019). With this in mind, it is possible that journaling gratitude towards others shifted the emphasis of one's outcomes towards external sources, which in turn reduced the demand for predicting and monitoring self-generated outcomes (Fredrickson, 2004; Kappes & Schattke, 2022). In contrast, those who were in the Control group retrospectively listed out what they had achieved every day, thus possibly enhancing the self-outcome monitoring and hence greater tone binding effect (Ryon & Gleason, 2014).

Furthermore, our intervention did not yield the expected results that the Control and Journal groups did not differ in level of change in life satisfaction, as suggested in study by Regan et al. (2023) whose paradigm we adapted. We proposed that future research could examine the effect of change in life satisfaction on temporal binding effect by utilising the Best Possible Self (BPS) intervention which lays emphasis on working hard to accomplish personal life goals, future-oriented striving, and it has been shown to increase life satisfaction more than gratitude journaling (Carrillo et al., 2019; Nicolson et al., 2020). Utilising BPS might therefore provide a stronger test of the relationship between self-focused/internal locus of control, life satisfaction and temporal binding. Unlike gratitude letter-writing which shifted attentional focus towards external sources of outcomes, the BPS intervention is inherently self-focused and future-oriented. One possible outcome if BPS is used is that such striving orientation of BPS may maintain or even enhance self-outcome monitoring and thus by extension enhance temporal binding even as it increases life satisfaction (so opposite of the 'life satisfaction is linked to no need for vigilance, and lower binding' account we suggested in Experiment 3 and 4). This is possible because the mechanism driving satisfaction is active

engagement with goals rather than reduced need for monitoring. Under this account, it is not life satisfaction per se that determines the direction of the binding effect but the motivational and attentional processes through which that satisfaction is generated. This remains speculative but it shows an important theoretical question for future research to address directly for instance by comparing the effects of contentment-focused versus striving-focused wellbeing interventions on temporal binding within the same design.

Null effect of positive mood on binding

Increase in positive mood did not increase temporal binding effect as we expected. Here, we provided a few lines of explanations for this. Firstly, it is worth noting that we did find a significant within-subject increase in positive mood after watching the positive film excerpt compared with after watching the neutral video. However, this positive mood induction was only significant when being measured using the BMIS scale but not on the positive mood items adapted from the Depressive Symptoms (DS), which we have already acknowledged that they were not appropriately capturing 'positive mood' but they were measuring depression. As a result, it is not surprising that Experiment 6's manipulation of the types of videos being shown did not moderate temporal binding, given that its rationale was based on the problematic/putative link between 'positive mood' measured using DS items and temporal binding in Experiment 3 and 4.

Second, it is worth noting that past research by Render et al. (2024) and Render and Jansen (2021) who utilised similar methods (i.e., participants were watching videos/images that produced high or low level of arousal) did show that being in a high arousal, especially a high sexual arousal state, reduced temporal binding effect. Arousal i.e., a subjective state of feeling activated or deactivated (Barrett, 1998) was not a primary construct of interest in the present experiment. We did not find any effects of video watching on temporal binding like they did, possibly because we only aimed to increase positive mood/valence rather than focussing on states of arousal. Even though the positive video in our study was indeed perceived to have greater intensity of positive emotion compared to the neutral video, this might not have produced level of arousal to the same extent as which observed in Render et al. (2024) and Render and Jansen (2021). Future research should consider measuring and controlling for these constructs: mood, emotion and arousal independently to disentangle their potential contribution to any observed effects on temporal binding.

Chapter Conclusion

This chapter investigated individual differences factors in the temporal binding effect. Gratitude journaling towards others was associated with weaker temporal binding effect, possibly because locus of control was shifted outwards, compared with self-focused journaling i.e., writing about one own's daily activities. Manipulating positive mood did not change the magnitude of the temporal binding effect. Overall, these experiments contributed to the development of the increasingly sophisticated and nuanced theories on the mechanisms that underlie sense of agency and the relationships between agency and other constructs, pointing the way towards promising new directions for future sense of agency research.

CHAPTER 5

Interpersonal Control: The Effect of Identifying with the Experimenter on Coercion, Prosocial Disobedience and Sense of Agency

Experiment 7

For decades, the social identity theory has helped us understand how individuals form the concept of self in relation to others (Tajfel & Turner, 1979). Under many social circumstances, people view themselves and others as group members rather than unique individuals. As a result, such perspective can have a significant influence on how individuals then think and behave in social situations (Ellemers & Haslam, 2012). One of the prominent examples of how the perceived self-other dynamics influence behaviours is when one complies with given instructions or orders from someone else. Complying with authorities, particularly given orders, is referred to as obedience. This line of research often utilises the Milgram's seminal experiments to test the willingness of participants to obey an authority figure who gives instructions to perform immoral acts (Milgram, 1963, 1965, 1983). It is worth noting that researchers have constructively addressed the ethical concerns about Milgram's original paradigm, whose nature of deception and extremity could have led to potential lasting psychological harm (Götz et al., 2023; Grzyb, Dolinski, Sudoł-Malisz, et al., 2025). Ever since, one emerging and prominent finding from such research on obedience is that individuals are more likely to follow the guidance of leaders perceived as prototypical members of their ingroup. Meanwhile, individuals are less likely to comply with outgroup authorities and thus disobey the immoral orders (Reicher & Haslam, 2019). However, there are numerous other complex and nuanced individual and socio-cultural factors that could support disobedience (Caspar, 2021). Thus, it is important to empirically investigate the extent to which identifying with the authority figure supports disobedience; or whether other individual differences factors would better explain the mechanisms underlying disobedience. Accordingly, this chapter aims to understand the extent to which identifying with the authority figure could influence one's decision to obey or disobey with the immoral orders and the associated mechanisms.

By focussing on this 'agent' - 'authority figure' relationship, researchers have re-analysed the results from Milgram's seminal studies in a critical light, showed that people are not always passive agents (Griggs, 2017; Haslam, Reicher, Millard, et al., 2015). Rather, participants often obey not out of blind obedience but through a process of 'engaged followership', whereby identification with the authority figure and the goals they represent becomes a crucial factor in determining behavioural outcomes (Griggs, 2017; Reicher et al., 2012). Haslam and Reicher (2012) argued that that "compliance is bound up with a sense of commitment to the experiment and the experimenter over and above commitment to the learner" (p.3). Furthermore, they also said that "when the study is conducted in commercial

premises as opposed to prestigious Yale University labs one might expect the identification to diminish and (as our argument implies) compliance to decrease. It does." (p.3). Importantly, the authors suggested that participants identify with both victim and the experimenter to some extent, which results in the tension often observed in these experiments. That said, stronger identification with the experimenter has been shown to be significantly associated with greater degree of shocks being administered by the participants across the various versions of the Milgram's paradigm (Haslam, Reicher, & Millard, 2015; Reicher et al., 2012). Other research has also shown that shared social identity might serve as a basis for those who follow orders from their leaders, or that obedience increases if participants identify with the experimenter as a legitimate scientific authority (Birney et al., 2023; Haslam & Reicher, 2012; Sewell et al., 2022; Steffens et al., 2014, 2021).

However, recent research has adopted a novel approach to study prosocial disobedience in a moral context with real shocks instead of the Milgram-like paradigm and no relationship was found between strong identification with the experimenter and the rate of obedience, nor with the rate of prosocial disobedience (i.e., refusing to obey the orders to send a shock) . The proposed link between identification with the authority figure and the tendency to obey was also not replicated in later studies conducted on more diverse populations in various locations (Caspar et al., 2022; Seyll et al., 2026; Tricoche et al., 2024). The nuances here could be explained by various factors. First, the methodological differences could yield diverging results i.e., the impact of environments (university vs. non-university settings) or the framing of the study. The paradigm adapted by Reicher et al. (2012), for instance, differs substantially from those developed by Caspar (2021), Caspar et al. (2022). The former reinterprets Milgram's obedience paradigm as a psychological process of identification with the experimenter as the authority figure within a controlled behavioural framework using deception. In contrast, Caspar et al. (2022) and Tricoche et al. (2024) used a paradigm involving real shocks and no deception. They employed a more comprehensive assessment of the multi-phase mechanisms of obedience using both neural and behavioural measures. In this paradigm, pairs of participants, who must not know each other and must be naïve to the purpose of the study, took turn to play the role as 'agent' or 'victim'. The agent received direct orders from an experimenter to either send or not send a mildly painful electric shock to the victim, whose hand was attached to a stimulator that sent real shocks. The agent had to decide whether to obey or disobey these orders whilst wearing Electroencephalography (EEG) cap or in the functional magnetic resonance imaging (fMRI) scanner, allowing researchers to examine the neural correlates of moral decision-making.

Additionally, the sample was relatively homogenous, composed mainly of academic social psychologists and first-year psychology students in Reicher et al. (2012) or just professional actors in Haslam, Reicher, & Millard (2015). In contrast, the studies conducted by Caspar and colleagues involved university students as well as real former genocide perpetrators, rescuers, and descendants of a war-torn population in Rwanda (Caspar et al., 2022, 2023, 2024; Pech et al., 2024). Homogeneity could potentially reduce variability in identification with the experimenter responses, which might inflate correlations between identification and obedience.

Furthermore, the issue lies at how there might be other factors besides identifying with the experimenter that modulate the outcome of participants obeying or disobeying orders. A detailed analysis of post-experimental interviews with Milgram's participants supports this view, indicating that the idea they obeyed solely out of identification with the experimenter is an overly simplistic interpretation (Hollander & Turowetz, 2018). Rather, participants provided a wide variety of explanations for their behaviour in the laboratory, a result also observed in other studies with more diverse populations (Seyll et al., 2025; Tricoche et al., 2025). Also, Grzyb and Dolinski (2023) found the importance of participants' empathy towards the victim (or learner) after being led to believe that they share highly similar values with the victim. The authors deployed similar methods as which in Reicher et al. (2012) but showed that situational empathy based on perceived similarity between the self and the victim reduces obedience to the experimenter. Grzyb and colleagues (2025) also recently examined the demographic characteristic of the experimenter, specifically the experimenter's gender, and found that gender of the authority figure has no significant impact on participants' reported willingness to obey. Likewise, in Caspar et al. (2022)'s supplementary analysis, for the identification with the experimenter, although they observed with the frequentist approach that the personal bond subscale (e.g. "*On a personal level, I can relate to this experimenter*") was the best predictor of prosocial disobedience whereby the more participant felt a personal bond with the experiment, the less they disobeyed; with the Bayesian approach, there was only an anecdotal evidence.

Thus, the present chapter has two primary objectives, which are 1) to verify the assumptions that a greater identification with the experimenter influences disobedience and 2) to examine how such identification with the experimenter could influence the multifaceted neurocognitive mechanisms associated with disobedience. We aimed to recruit a heterogenous sample, as diversity in socio-cultural backgrounds and nationalities relative to that of the experimenter might introduce more nuanced perceptions of authority. This could

lead to greater modulation in identification with the experimenter and more ecologically valid insights into obedience dynamics. Additionally, we aimed to include a variety of measures to assess participants' perception of the experimenter in addition to the identification with the experimenter scale (which all Caspar et al. (2022), Reicher et al. (2012) etc. similarly employed). The additional measures employed in this experiment included attitudes towards the experimenter i.e., participants rated the experimenter on the scale of favourability, warmth and competence and how participants perceived the ethnic identity of the experimenter (inclusion/exclusion of the experimenter's group). Furthermore, recent literature has shown that various neuro-cognitive and affective mechanisms are involved in the decision-making process leading to (dis)obedience (Caspar et al., 2022; Tricoche et al., 2024, 2025). It is a dynamic process which have three distinct but interconnected phases: pre-decision phase; decision and action phase and post-decision phase (Tricoche et al., 2024). Each of these phases involve specific neurocognitive mechanisms which could be differently associated with the identification with the experimenter, a key objective of the present chapter. By examining the chronological phases of these mechanisms, we could best understand when, how and whether identifying with the experimenter would matter most when it comes to the decisions to obey or disobey.

Pre-decisional processes of (dis)obedience

The pre-decision-making phase involves individuals gathering and interpreting information from their environment. In the (dis)obedience literature, this phase is particularly relevant for understanding how participants process auditory instructions from the experimenter giving orders the participants to harm or not the victim (Caspar et al., 2022; Seyll et al., 2026). Studies have shown that auditory attention can be reflected by the midfrontal theta activity (FMθ), with an a reduced FMθ in the 4-8 Hz frequency range was related to an attenuated level of conscious attention engagement in the processing of the auditory stimuli as well as reduced vigilance to such auditory processing (Paus et al., 1997; Villena-González et al., 2018). Interestingly, a previous study found that a greater identification with the experimenter was associated with a greater auditory FMθ, a finding not observed with the other phases of decision-making (Caspar et al., 2022). Based on these findings, identification with the experimenter might modulate FMθ after hearing the experimenter's orders, which will be investigated again in this chapter.

Decision-making phase

Just before the actual execution of an action, individuals might experience cognitive conflict when there exist incongruent and competing information, which thus requires extra cognitive resources to resolve the discrepancies. Past research has shown that cognitive conflict occurs commonly in tasks like the Stroop task or Flanker task that require making ambiguous and contradictory decisions (Goldfarb & Henik, 2007; Ye & Damian, 2023). To measure cognitive conflict using EEG, researchers have shown that enhanced midfrontal theta activity (4-8Hz) is associated with greater cognitive conflict (Brunetti et al., 2019; M. X. Cohen, 2014), which reflects the engaged cognitive control processes in the medial frontal areas and the anterior cingulate cortex. Difficult decisions are often linked with higher FMθ compared to easier decisions (Nigbur et al., 2011). In the context of obedience and immoral orders/behaviours similar to this experiment, participants experience cognitive conflict when having to choose to a) follow orders and harm the victim or b) disobey the experimenter's orders. It has been found that FMθ was significantly attenuated when participants obeyed to give shocks to the victim, in comparison to when they acted freely, indicating that obedience to inflict harm reduces cognitive conflict (Caspar & Pech, 2024). In addition, having a high FMθ before obeying orders to harm others has been shown to be predictive of more frequent prosocial disobedience (Caspar et al., 2022). It is unclear, though, how identifying with the experimenter could influence cognitive conflict before action. It is possible that identification with the experimenter would make the choice of following orders easier to choose, hence lower cognitive conflict. Thus, we also wished to investigate whether participants might undergo cognitive conflict when having to decide to obey the experimenter or send painful shock to the victim; and whether identification with the experimenter would moderate this neural process.

As a reminder, the sense of agency is known as the feeling of being in control over one's action and its action outcome (Haggard, 2017; Haggard et al., 2002) and is also linked to the decision-making phase. Prior work has shown that coercion reduces both explicit feelings of agency and the temporal binding effect as the implicit sense of agency (Akyüz et al., 2024; Caspar et al., 2018, 2020, 2025) compared to being free to act. Such an effect is also observed (although to a lower extent) in tasks contrasting free and instructed choices in a neutral context (Barlas et al., 2017, 2018; Barlas & Kopp, 2018). Accordingly, these findings might indicate that when being coerced to harm, individuals might not perceive themselves as the initiators of such immoral actions and thus distance themselves from the harmful consequences, resulting in agentic shifts (Akyüz et al., 2024; Caspar et al., 2016). Such

agentic shift takes place, according to Milgram's theory of agentic state, when the person perceives themselves as passive agent and gives up their autonomy and moral responsibility to the authority figure (Milgram, 1974). This allows the individual to participate in wars or atrocities by 'just following orders' because the individual may believe that they were not the source or origin of immoral decisions (Caspar, 2024; Gibson et al., 2018). Past studies have however not yet directly investigated the relationship between identifying with the experimenter and the sense of agency in coercive context. Reflecting back on the engaged followership model, a shared identity with authority figures in this context would increase compliance and obedience due to perceived identification with the experimenter as the authority figure. Thus, it could be expected that greater identification with the experimenter would decrease sense of agency, since identification with the coercer might make it easier to shift the agency attribution from oneself to the coercer.

Post-decisional processes of (dis)obedience

A crucial post-decisional process is how individuals rate the feeling of responsibility for the actions. Such attribution of personal responsibility is characteristic of authorship over the action and to what extent do they feel responsible for the consequences of their actions (Lepron et al., 2015; Moretto et al., 2011). Previous research has found that responsibility ratings decrease as the result of obeying the orders to harm others as opposed to when being able to choose which actions to execute (Caspar et al., 2016, 2021). Past studies have however not yet directly investigated the relationship between identifying with the experimenter and the feeling of being responsible for immoral actions in coercive context. Like sense of agency, it is expected that identifying with the experimenter as an authority figure could make individuals attribute responsibility to the coercer as the source of action, hence attenuating the feeling of being responsible for the immoral decisions. Although it is worth noting that opposite pattern can occur like in Malter et al. (2021) where participants following orders actually have increased self-reported guilt and regret over harmful outcome compared with when deciding autonomously. This reflects the possibility that there might be counterfactual thinking involved like feeling that 'I should have resisted' which then heightens rather than reducing guilt. However it is also worth noting that Malter et al. (2021)'s paradigm involved hypothetical scenarios asking participants to imagine themselves in a role rather than real actions with real consequences (harmful shocks) as we intended to test here (thus this could explain any divergent findings).

Chapter overview

This chapter examined whether identifying with an authority figure (Low vs. High identification levels) influences participants' responses to immoral orders and related neural processes. We used the paradigm established by Caspar et al. (2022) because it allows us to investigate the neural and cognitive processes with electroencephalography surrounding the decision to obey or disobey the experimenter's orders. To address identification with the experimenter, we recruited a culturally diverse sample as differences in ethnic backgrounds and nationality groups could influence perceptions of identification with the experimenter and with authority. This could produce greater modulation in identification with the experimenter and more ecologically valid outcomes. We recruited participants from various cultural backgrounds, close (South-East Asian/East Asian) or less close to which of the experimenter (European/African) (see Table 22). Past research has indeed shown that people tend to identify more with others from the same ethnic backgrounds (Dovidio et al., 2001; Salas-Schweikart et al., 2025). That being said, the perceived ethnicity and group identification are definitely more nuanced. For instance, Japanese individuals have been found to view themselves as different from Koreans, despite the two ethnic cultures being geographically proximate (Foster & White, 1982; Park et al., 2017). For those reasons, we aimed to recruit as diverse population as possible, and we utilised participants' own self-reported identification with the experimenter for group categorisation.

For the behavioural results, if identification with the authority figure influenced the disobedience rate, we expected that participants reporting higher identification with the experimenter to show reduced disobedience, whereas participants with lower identification might show increased disobedience. A non-significant behavioural difference between the Low and High identification groups would be considered as a lack of effect of identification on (dis)obedience. Likewise, it was expected that participants in the High identification with the experimenter group would show attenuated temporal binding as an index of implicit agency and reduced responsibility rating, possibly due to easier shift of agency and responsibility attribution from the self to the experimenter.

Regarding the neuro-cognitive mechanisms occurring at different phases of the decision process, based on Caspar et al. (2022), we expected to observe that participants with greater identification to the experimenter would show greater FMθ when hearing the experimenter's orders during the pre-decisional phase. In other words, identifying with the experimenter might result in participants being more engaged and paying more attention to the orders, as indicated by enhanced FMθ after auditory instructions. Regarding cognitive

conflict experienced during the decisional phase, as having mentioned, cognitive conflict occurs when individuals experience tension between competing choices. In this context, participants faced a conflict between obeying the experimenter's orders and avoiding inflicting harm on another person. Thus, we predicted that for participants who identified with the experimenter, their motivation to follow through with the experimenter's orders could outweigh the motivation to disobey. Therefore, the decision to obey the orders would generate less cognitive conflict, which would be linked with lower FMθ before key press.

Lastly, to best understand which factors drove the rate of prosocial disobedience and to what extent identifying with the experimenter contributed to this outcome; a comprehensive linear mixed-effects model was conducted. This model included several variables that measure socio-cognitive, individual differences and neural measures and assessed their effects on the rate of prosocial disobedience. Note that in this model, the different nationality groups from the diverse sample were included as a random intercept to account for unexplained variability within each nationality group (Table 26). Such statistical approach could help determine whether identifying with the experimenter, particularly when participant samples displayed greater variability in relation to the experimenter's identity, would be the predominant predictor of obedience or prosocial disobedience, or whether other factors played more significant roles.

Method

Participants

Participants were either students at Ghent University or residents in Ghent (non-university students) and must be above 18 years old. We recruited participants in same-gender dyads i.e., two participants from the same gender took part in the study simultaneously. Participants had to be fluent in English; had normal or corrected to normal vision; and no history of neurological, psychiatric or developmental disorders.

We assigned participants to Low and High identification with the experimenter groups based on participants' self-reported scores. Participants were recruited initially without regard to their level of identification with the experimenter. After all data was collected, participants were divided into Low v. High identification groups using median-split method based on their responses to the identification with the experimenter questionnaire (see Results).

The sample size determined here was based on Caspar et al. (2022), because a) they employed similar paradigm to investigate coercion to harm others and prosocial disobedience

(all trials were forced-choice to shock or not shock the victim) and b) the study also assessed group-level differences i.e. between-subject factor (Belgian v. Rwandan groups), similar to which we also utilised here. In the study, the authors found main effect of Group on the rate of prosocial disobedience that Rwandan participants tested in Belgium prosocially disobeyed more than Rwandan participants tested in Rwanda, with np^2 =.250 which corresponds to Cohen's d = 1.16 (large effect size). Thus, in this experiment, to achieve a power of .95, given α = .05 for this effect size d= 1.16, the estimated sample size was 21 for each group (power analysis was conducted using GPower). However, we increased the sample size up to 35 for each group to account for data loss due to bad EEG signal-to-noise ratios and failure to understand the interval estimation task. Seventy participants (*M*age = 24.04, *SD* = 6.25, 23 Men) were recruited via multiple channels, including SONA (a platform to recruit participants at Ghent University) and social media platforms (Facebook groups for students/job advertisements in Ghent). Participants were paid at the rate of 12.50EUR/hour. This study was performed in line with the principles of the Declaration of Helsinki. Approval was granted by the Ethics Committee of Faculty of Psychology and Educational Sciences, Ghent University (reference number: 2023-146). Informed consent to participate and for publication was obtained from all individual participants included in the study.

Because we focused on the effect of the experimenter A.H.L (Female, South-East Asian, international postgraduate researcher in Belgium), we recruited a diverse sample which could be categorised into four primary nationality groups of participants relative to the experimenter's A.H.L social identity to introduce variability to identification with the experimenter: Belgian (the largest group considering that the study was conducted in Belgium), Southeast/East Asian (SE/EA), South Asian & Middle Eastern (SA/ME), Other European, and African (see Table 22 and also Table 23 for further demographic information).

Table 22.

Demographic information of each nationality group

Nationality groups	Sample N	Mean Age (SD)	Gender
Belgian	36	*M*age = 22.9, *SD* = 7.7	14 Men
Southeast/East Asian (SE/EA)	14 (Vietnamese: 1; Chinese: 2; Filipino: 2; Indonesian: 1; Singaporean: 2; South Korean: 2; Taiwanese: 4)	*M*age = 25.6, *SD* = 4.6	4 Men

Nationality groups	Sample N	Mean Age (SD)	Gender
South Asian & Middle Eastern (SA/ME)	7 (Indian: 3; Bangladeshi: 1; Iranian 2; Pakistani: 1)	*M*age = 26.6, *SD* = 3.8	2 Men
Other European countries	11 (Bosnian: 1; Dutch: 2; Estonian: 1; German: 1; Russian: 1; Slovenian: 1; Spanish: 1; UK: 1; Ukrainian: 2)	*M*age = 23.7, *SD* = 3.6	2 Men
African	2 (Zimbabwean: 1; Cameroonian: 1)	*M*age = 26.0, *SD* = 1.4	0 Men

Table 23.

Further participant demographic characteristics of Experiment 7 (N = 70)

Variable	Category	n	%
Household income	Less than €20,000	14	20
	€20,000- €39,999	13	18.6
	€40,000- €59,999	12	17.1
	€60,000- €79,999	8	11.4
	€80,000- €99,999	1	1.4
	€100,000+	5	7.1
	Prefer not to say	17	24.2
Religion	Christianity	16	22.9
	Islam	3	4.3
	Hinduism	0	0
	Buddhism	1	1.4
	Judaism	1	1.4
	Atheism	25	35.7
	Agnosticism	11	15.7
	Other	13	18.5

We included a data exclusion criterion whereby participants who failed to produce the temporal intervals covarying monotonically with the actual action-tone intervals would be excluded, following Caspar et al. (2022). Thus, linear trend analyses with contrast

coefficients -1, 0, -1 for the three delays of 200ms, 500ms and 800ms of the action-tone intervals were conducted to identify participants whose action-tone intervals did not gradually increase with the actual action-tone intervals. Eight participants were excluded from the subsequent temporal binding analyses due to having non-significant linear trend, leaving N = 31 for each Low/High identification with the experimenter group. We also checked outliers for the time frequency midfrontal theta analyses.

For mid-frontal theta activity (FMθ) after auditory instructions, no participants' FMθ power exceeded z±3SDs, thus all retained for the analysis. For mid-frontal theta activity (FMθ) before key press, 1 participant in this analysis were excluded for excessive movements during pressing the key which confounded their FMθ results (their z-scores for FMθ power were well above the ±3SDs range). To calculate FMθ power before participants disobeyed to shock, we also excluded 20 participants who did not disobey for more than 5 trials (similar to past research (Caspar & Pech, 2024), leaving final N = 24 in Low identification and N = 25 in High identification group.

Design

While the study focusses on many behavioural and neuro-imaging outcomes, the primary between-subject Group factor was the identification with the Experimenter: Low v. High. With regards to the repeated-measures factors, all participants were given the same orders from the experimenter i.e., two forced-choice 'Give a Shock' v. 'Don't give a Shock' experimental conditions. Thus, the mixed design allows us to investigate how Group level (Low v. High Identification with the experimenter) × Forced Orders (Give a Shock v. Don't give a Shock) influenced the behavioural and neuroimaging outcomes of the participants.

The first behavioural outcome was the prosocial disobedience rate (i.e., the percentage of trials the agent refused to deliver the real shock to the victim despite the experimenter ordering the agent to do). The second primary behavioural outcome was the sense of agency measured via the temporal binding effect based on the perception of the temporal delay between the action i.e., when the agent pressed the shock/no shock button and the outcome of that action i.e., a beep tone. The beep tone occurred after a random delay of 200, 500 or 800ms. We calculated the temporal binding using the error of estimation method (expressed in ms): Temporal Binding = Perceived interval – Actual interval between key press and outcome. The more negative these values, the stronger temporal binding effect which was the proxy for a stronger implicit sense of agency.

Regarding the neuro-imaging outcome, this experiment focussed on the midfrontal theta activity FMθ power (dB) a) after hearing the auditory instructions from the experimenter ('Give a shock' v. 'Don't give a shock') b) before key press (deciding to shock/not to shock the victim). For the FMθ after hearing the auditory instructions as the outcome i.e., the higher the FMθ, the greater attention was paid to the auditory orders, we looked at a mixed-design 2 ('Give a shock' v. 'Don't give a shock') × 2 (Low v. High identification with the experimenter) and investigated how these factors influenced the power of FMθ. Regarding the FMθ before key press i.e., higher FMθ before key press indicated a greater experience of cognitive conflict; our design was a mixed 3 (Repeated-measures: Gave A Shock – Obeyed v. Did Not Shock – Obeyed v. Prosocial Disobeyed) x 2 (Between-subject: Low v. High experimenter identification) on the power of FMθ before key press.

Material and Procedure

The current experiment replicated the paradigm established by Caspar et al. (2022), wherein participants were recruited in the same gender dyads. Furthermore, for each pair of participants, we also tried to match one participant with another participant with the closest nationality group based on Table 22. (Belgian – Belgian; Vietnamese – Chinese etc.) in order to control, to a certain extent, for the ingroup/outgroup differences between the pair of participants themselves (i.e., between the agent and the victim) and avoid having this factor as a confound. In addition, we also included an explicit measure of empathy, which past research has shown to be most related to identifying with the victim, to understand if feeling empathic towards the victim would moderate decision to disobey (Grzyb & Dolinski, 2023). Before the experiment, participants who were attending the session in pair were asked whether they knew each other and if they knew the experimenter personally. We ensured that only pairs of participants who claimed to not be acquainted with each other or the experimenter would proceed to take part in the study.

On the day of the experiment, each pair of participants was first given a detailed explanation of the task in English by the experimenter and asked to complete the consent form. They were reminded that they could stop at any time during the experiment. The participants were told that they would both play the role of 'agent', who received the orders from the experimenter to either give or withhold a shock to the 'victim'. Whether participants started as the 'agent' or 'victim' first was randomly assigned at the beginning of the experiment by using a random wheel generator, but they were offered the opportunity of change the order if they wanted to. Some participants preferred to choose, as this opportunity

reassured them that we were not using deception or a rigged wheel generator, as in Milgram's original study and its variants (Milgram, 1963, 1965). Each pair of participants recruited for an experimental session swapped roles halfway through the experiment, so that the victim became the agent and vice versa. They were told that the experimenter would give instructions to either administer or withhold a painful shock to the victim (i.e., the fellow participant). After receiving the verbal order, a picture displaying two rectangles, a red one labelled 'Give a shock' and a green one labelled 'Don't give a shock', appeared at the bottom left and right of the screen (Figure 11). Participants were instructed to decide to press the corresponding button to decide whether to deliver the shock or not after hearing the instructions. It is important to note that participants were not incentivised to either obey or disobey the orders.

Shocks were delivered using a constant current stimulator (Digitimer DS7A) connected to two electrodes placed on the abductor pollicis muscle of the victims' non-dominant hand, visible to the agent through the camera display. Individual pain thresholds were determined for both participants before starting the experiment. This allows participants to understand what it would feel like to receive shocks. Each participant's appropriate shock threshold was determined by increasing stimulation in steps of 1 mA (Caspar et al., 2016). One participant at a time was asked a series of questions during the calibration process about how uncomfortable and painful each shock was given: 'Is it uncomfortable' – 'Is it painful' – 'Could you cope with a possible maximum of 96 times of these shocks?' – 'Is it possible for me to increase this threshold?'. Once the first participant was complete with the pain threshold process, the other participant took part in the same procedure immediately after. The threshold for each person was made unknown to the other fellow participant. The mean stimulation selected based on this procedure was 677.81 mA (SD = 182.17 mA, V = 272.46, Pulse duration = 285.51 μs). Lastly, both participants were informed that this threshold would not change throughout the experiment.

The participant who started as the agent first was then brought into another room that was isolated from where the other participant i.e., the victim was located (Figure 11). As having mentioned, the victim would have the electrodes placed on their non-dominant hand that should produce a clear visible muscle twitch every time a shock was given. The victim was asked to place their hand on the table in front of the camera and they were asked to refrain from moving the hand during the experiment. Throughout the time that they played their role as the victim, they were asked to fill out the questionnaires that would take

approximately 25 minutes to complete, which aligned with the duration of the agent's decision task to administer shocks or not.

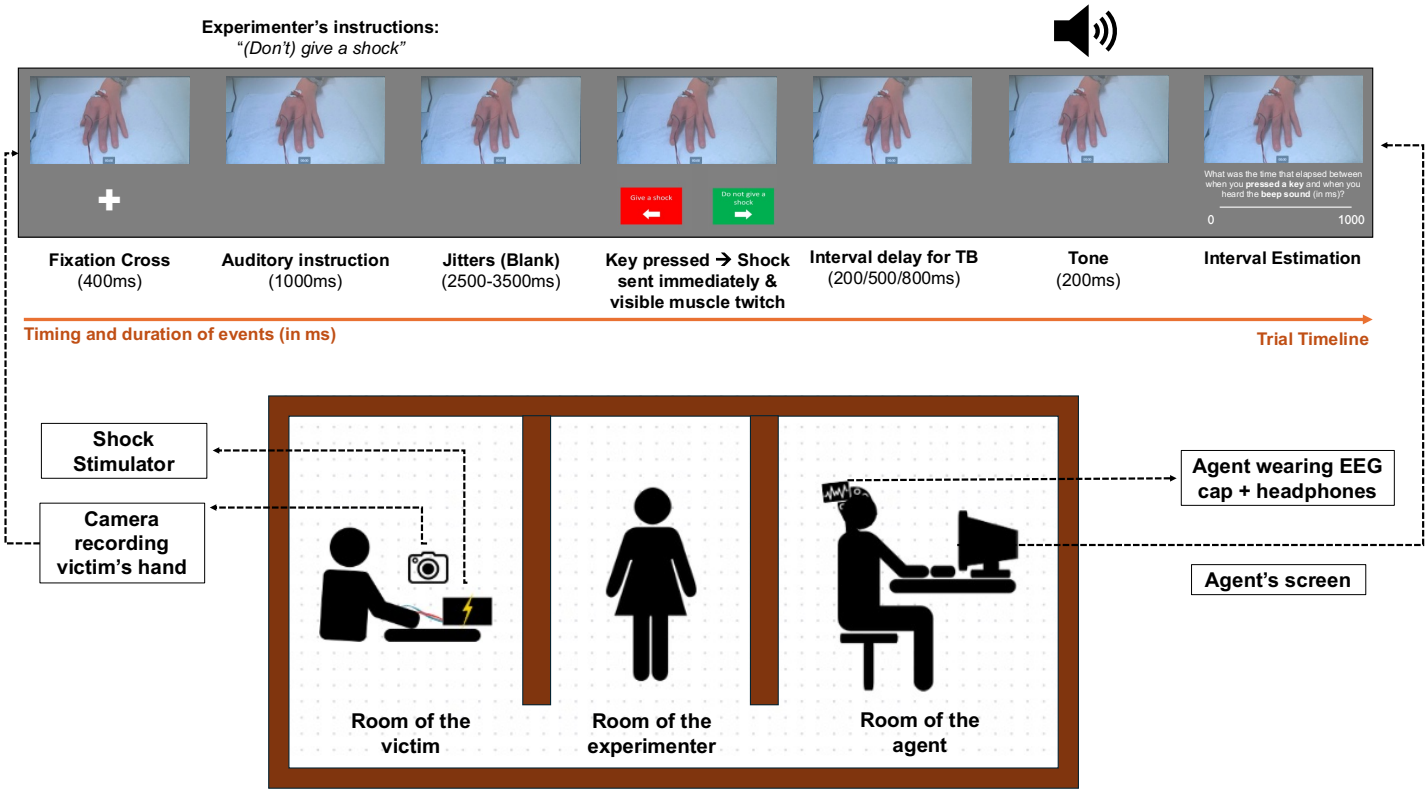


Figure 11. Illustration of the experimental set up. Timeline of an example trial is shown in the upper panel, illustrating the agent's screen during a trial where the agent gave a shock to the victim. Lower panel shows the room layout with the victim, agent and experimenter each in their separate rooms.

With regards to the agent, they were taken to a room separate from the victim, where their EEG recordings were taken while the decided whether or not to deliver a shock to the victim after hearing the experimenter's instructions. On the computer screen, the agent continuously saw a real-time video display of the victim's hand, which was fixed in the upper half of the screen (Figure 11). This allowed the agent to clearly observe the consequences of their actions each time they administered a shock to the victim and to remain fully aware that the shocks were real.

The agent was given a pair of headphones and instructed that they would hear the experimenter's orders through a microphone connected to the headphones from another room. In reality, the orders were pre-recorded, as using audio recordings ensured precise timing of the instruction onset, an essential component for EEG recordings. The instructions consisted of either *'Give a shock'* or '*Don't give a shock'.* Each sentence was recorded six

different times, with slight variations in tone of voice and with background noise simulating interphone communication to increase authenticity. These recordings were played to the agent in random order on each trial.

For each trial, a fixation cross was shown for 400ms. Immediately afterwards, an auditory instruction (i.e., the verbal order from the experimenter (‘*Give a shock’* or ‘*Don’t give a shock’*) was presented for approximately 1,000ms (see Figure 11 for the trial timeline). Following the auditory instruction, a jittered interval of 2,500ms to 3,500ms was presented during which nothing occurred. This period ensured a clear separation of EEG activity associated with different cognitive processes (explained in detail below). After the jitter period, an image showing two rectangles with a red one labelled ‘Give a shock’ and a green one labelled ‘Don’t give a shock’ were presented in the lower half of the screen, on the left and right sides, respectively. Although the key-outcome mapping varied on a trial-by-trial basis, the outcome was always fully congruent with the mapping shown on the agent’s screen. The agent could then press one of the two buttons: pressing the ‘Give a shock’ key immediately delivered an electric shock to the victim, whereas pressing the ‘Don’t give a shock’ did not. After a variable delay of 200, 500 or 800ms following the keypress, a short beep (duration = 200ms, 131Hz) was played. This set-up allowed us to measure the implicit sense of agency using the action-outcome interval estimates paradigm, in which the agent estimated the time elapsed between their action (pressing the button) and the outcome (the beep). An analogue scale from 0 (left) to 1,000ms (right) was displayed on the screen, and the agent indicated their perceived interval on this scale. Underestimation of the interval reflects the temporal binding effect, meaning that the greater the underestimation, the stronger the agent’s implicit sense of agency. After the agent provided their estimate, the response was saved and and a new trial began following a fixed 1,500ms inter-trial interval.

Additionally, the agent was asked to rate on an analogue scale how painful the shock in the very last trial appeared to be for the victim, using a scale from 0 (“Not at all”) to 100 (“ Very”). This rating was collected randomly after 15 of the trials, following the interval estimate task, to ensure that the agent was indeed looking at the real-time display of the victim’s hand on the screen.

There were eight practice trials during which no real shocks were administered to the victim, even if the agent pressed ‘Give a shock’. These training trials familiarised the agent with the time-interval estimation procedure. The agent was instructed to remain as still as possible, avoiding excessive movements and blinking, especially during the jitter period (2,500ms and 3,500ms) and immediately before pressing a button, to ensure a clean EEG

signal during these critical time windows. During experimental phase, there were 96 trials. In 64 trials (67%), the experimenter ordered the agent to deliver a shock to the victim, and in 32 trials (33%), the instruction was not to give a shock, following the procedure described in Caspar et al (2022). The order of the trials was fully randomised across participants. At the end of all 96 trials, the agent was asked to rate how responsible they felt as a measure of perceived responsibility, using an analogue scale from 0 ("Not at all") to 100 ("Very"). After completing the first half of the experiment, the victim and agent switched roles, such that the previous agent became the victim and vice versa.

Questionnaires

Based on our main research questions, the most important questionnaire in this experiment was the identification with the experimenter, measured with 14 items, such as '*I feel strong ties with this experimenter'* on a 7-point Likert-scale (1 = Strongly disagree; 7 = Strongly agree) (Steffens et al., 2014). The scores from this questionnaire were utilised to split participants into two groups Low v. High identification with the experimenter based on the median split method (see Results).

It is the primary interest of chapter to understand the effect of the experimenter on the behavioural and neural processes underlying disobedience. Thus, we further measured a variety of factors related to how participants perceived the experimenter. Firstly, we measured the participants' attitudes towards the experimenter that the experimenter was rated on favourability, warmth and competence on the scale from 0 (= not at all) to 100 (= very). Next, the inclusion/exclusion of the experimenter was assessed with four items adapted from the 7-point Moral Inclusion/Exclusion of Other Groups (MIEG) scale. For example, participants were asked to indicate whether the experimenter belonged to a group whose "members deserve no respect" (scored as 1) or whose "members deserve our utmost respect" (scored as 7) (Passini & Morselli, 2017).

Additional questionnaires that participants completed included a wide range of constructs at both individual and social levels. We measured:

a) Patriotism i.e., the extent to which participants identify with their nationality, for instance '*It is not that important for me to serve my country'* on the scale from 1 = Strongly disagree to 5 = Strongly agree) adapted from Patriotic and Nationalistic Attitudes scale (Kosterman & Feshbach, 1989).

b) Moral Foundation Questionnaire was included to assess individual differences in the endorsement of the five moral foundations: *Care, Fairness, Loyalty, Authority* and *Purity,* rated from 0 (= not at all relevant (This consideration has nothing to do with my judgments of

right and wrong/Strongly Disagree) to 5 (= extremely relevant (This is one of the most important factors when I judge right and wrong)/Strongly Agree) (Graham et al., 2011).

c) Satisfaction with Life scale was included to measure participants' overall life satisfaction e.g., '*In most ways my life is close to my ideal*'), rated from 1 (= Strongly Disagree) to 7 (= Strongly Agree) (Diener et al., 1985).

d) The General Self-Efficacy Scale was adopted to assess the extent to which each of 10 statements was characteristic or true of participants themselves (e.g. '*I can always manage to solve difficult problems if I try hard enough'*), rated from 1 (Not at all true) to 4 (Exactly true) (Schwarzer & Jerusalem, 2012).

e) Right-Wing Authoritarianism (RWA) is a measure of a strong, unitary social-attitude dimension reflecting conventionalism, authoritarian aggression and authoritarian submission, such as '*The "old-fashioned ways" and "old-fashioned values" still show the best way to live*', rated from 1 = very negative to 7 = very positive). We adopted the measure based on the short 15-item version of Right-Wing Authoritarianism (RWA) (Zakrisson, 2005).

f) Social Dominance Orientation (SDO) was also included. SDO scale is a 16-item measure of the extent to which participants endorse group-based hierarchy; e.g. '*Some groups of people are simply inferior to other groups*', rated from 1 = very negative to 7 = very positive (Pratto et al., 1994).

g) Depressive symptoms adopted the Centre for Epidemiological Studies-Depression (CES-D) with 15 items were included to measure symptoms of depression, including restless sleep, poor appetite, and loneliness. Participants were asked to rate on the scale from 1 = None or almost none of the time to 4 = All or almost all of the time how much of the time during the past week they felt depressed, or 'everything I did was an effort.'. Positively worded items, such as 'I enjoyed life', were computed into one mean score that measured positive mood. The rest of the statements that addressed depressive symptoms were computed into a mean score and the higher the negative-mood score, the more depressive symptoms a person experienced over the course of one week (Carleton et al., 2013).

Participants also responded to demographic questions including age, gender, household income, ethnicity, and religion. It is worth noting that for participants who began the experiment as victims, during the first half of the session, they only completed the questionnaires unrelated to the experimenter (e.g., Moral Foundation Questionnaire, Satisfaction with Life, etc.). Victim-first participants filled out the questionnaires concerning their perceptions of and attitudes toward the experimenter only at the very end of the entire

session, that is, after they had also taken on the role of agent. Conversely, participants who started as agents completed all questionnaires during the second half of the session, after switching roles and becoming the victim. This procedure ensured that the self-reported ratings concerning the experimenter were based on the participants' experience after having played both roles as victims and agents, thereby avoiding any potential confounds related to order effects (i.e., starting as the victim or the agent first).

EEG Recordings

We conducted EEG recordings using the 64-channel electrode cap with the Biosemi equipment. The data was acquired at a sampling rate of 2,048 Hz and the channels were placed according to the international 10-20 system. Two additional electrodes were used to capture activities from left and right mastoids. We used MNE-Python to pre-process and analyse all EEG data. Data were referenced to the average signal of the two mastoids and filtered low-pass at 40 Hz and high-pass at 0.01 Hz. Eye movement artefacts were removed using an Independent Component Analysis (ICA), wherein Fp1 and Fp2 were set to find bad ICA components that significantly correlated with horizontal and vertical eye movements (threshold correlation = 0.5) alongside with visual inspection to detect bad ICA components. ICA solution was finally reapplied on the filtered data.

To conduct Time Frequency Analysis (TFR), the frontal midline theta frequency (FMθ) was computed by averaging the Fz, Cz, FC1 and FC2 electrodes. For each trial, the time-frequency power was extracted with the tfr_morlet() function following the calculation of a Fast Fourier transform (FFT) on the data. An FFT of a complex Morlet wave was computed based on these parameters: frequency range from 2 Hz to 30 Hz with 80 logarithmically spaced bins, logarithmically spaced cycles from 4 to 14. Epoching was performed by taking -3 to 3s around the stimulus or response window.

All power values in the time-frequency representation were normalised to the average baseline power in each frequency band using a decibel (dB) transform (Db power = 10 x log10(power/baseline), using MNE-python's 'logratio' parameter. To find the significantly different value, values that were above the mean +1.96* standard deviation from all values were selected. Such formula produces a range of values within which the true population mean could be expected to fall with a 95% level of confidence (Field, 2012). We then used this value to create a contiguous mask, extracting FMθ power within the specific time and frequency windows and for each participant, their mean value within this mask defined the time/frequency window. This mean + 1.96*SD approach was similarly adopted by Caspar & Pech (2024) and Seyll et al. (2026).

For TFR after audio instructions i.e., stimulus-locked, the mean and standard deviation of baseline-normalised power were computed across shorter epochs that were cropped from -500ms to 1,000ms to determine the time and frequency windows of interest and select the value which was significantly different from the others, grand-averaged across all participants and conditions. Values exceeding the mean + 1.96*SD threshold were identified which was used to define the FMθ time-frequency mask (see Figure 14A). The baseline power was computed as the average power across all experimental conditions (instructions to 'Give a Shock' / 'Don't give a shock') from -500ms to -100ms before the onset of the audio instructions for stimulus-locked TFR (see Figure 14A). The baseline window for stimulus-locked FMθ differed slightly from that used in Caspar et al. (2022), who employed a -200 to 0ms window. Here, we instead used -500 to -100 ms. This adjustment is due to difference in experimental software: Caspar et al. (2022) presented stimuli using MATLAB/Psychtoolbox, which typically times stimulus onset using absolute values calculated from the system clock, whereas the present study used Python/PsychoPy, which relies on a frame-based presentation approach (e.g., stimuli presented for an exact number of frames). Although rare and generally minimal, frame-based timing can be susceptible to occasional frame-rate lags, particularly when playing auditory files. Thus the baseline window was shifted to end 100ms before auditory onset (rather than at 0ms) to help buffer against potential frame lags. As having mentioned, a variable jittered interval (2,500ms - 3,500ms) was included between the auditory instruction and the response screen to ensure a clear temporal separation between the instruction-related processing and the later decision-making/motor preparation of key press. Such separation allowed cleaner EEG epochs when we computed stimulus-locked and response locked TFRs to make sure that FMθ after auditory instructions did not get contaminated or overlapped with FMθ before key press.

For TFR before keypress i.e., response-locked, shorter epochs from -2,500ms to 500ms were analysed to determine the time and frequency windows of interest and select the value which was significantly different from the others, grand-averaged across all participants and conditions. Values exceeding the mean + 1.96*SD threshold were identified, which was used to define the FMθ time-frequency mask (see Figure 15A). The baseline power was computed as the average power across all experimental conditions (Gave A Shock - Obeyed, Did Not Shock - Obeyed, Prosocial Disobeyed), from -1,500ms to -1,000ms before the key press for response-locked TFR (Figure 15A). In Caspar et al. (2022) the baseline for FMθ after key press was 300-500ms after the key press. Such baseline was not suitable here since a visible physical shock to the victim's hand was presented immediately upon keypress

on shock trials. Using a post-keypress baseline would therefore risk contaminating the baseline estimate with visual processing of the shock outcome. Such contamination would not be equivalent across conditions because on trials where a shock was delivered, participants might still be processing the visual shock feedback within the 300-500 ms window, whereas no comparable visual event occurred on trials where no shock was given. This asymmetry could introduce a systematic confound between conditions at the level of the baseline itself. To avoid this a pre-keypress baseline (-1,500 to -1,000ms relative to keypress) was used instead. This window fell within the 2,500-3,500ms jittered interval between the end of the auditory instruction and the appearance of the response options during which no stimuli were presented, allowing a clean baseline estimate.

To clarify, the mean + 1.96*SD approach to establish threshold (for both stimulus-locked and response-locked TFR) was used as a data-driven procedure to identify a time-frequency region of pronounced theta activity for subsequent participant-level extraction, rather than as a formal statistical test or correction for multiple comparisons across the time-frequency space (e.g., cluster-based permutation testing). The window was defined independently of condition contrasts (for FMθ after hearing the auditory instructions: 'Give a shock' v. 'Don't give a shock'; for FMθ before key press: Gave A Shock – Obeyed v. Did Not Shock – Obeyed v. Did Not Shock – Prosocially Disobeyed trials), reducing analytic circularity in the subsequent hypothesis tests.

Results

Low v. High identification with the experimenter

Participants were divided into Low (N = 34, $M = 4.46$, $SD = .45$) and High identification groups (N = 36, $M = 5.78$, $SD = .50$) based solely on their reported scores on the identification with the experimenter scale, using median split method (Iacobucci et al., 2015) (median = 5.1, $M = 5.14$, $SD = .81$, Min = 3.10, Max = 7.00). This categorisation was independent of the participants' nationality groups. As expected, an independent sample test confirmed that the two groups Low v. High reported identification significantly differed in how much they identified with the experimenter $t(68) = -11.67$, $p < .001$, $d = -2.79$ (two-tailed). The two Low v. High identification with the experimenter groups (independent of nationality) also differed significantly regarding the positive perception of the experimenter's group that the High identification ($M = 6.41$, $SD = .50$) group reported higher score than the Low identification group ($M = 6.01$, $SD = .99$) $t(68) = 2.12$, $p = .037$, $d = .51$ (see also

Appendix F for full comparisons between the two groups on other questionnaires surrounding participants' perception of the experimenter).

To confirm that nationality did not systematically affect the identification grouping, we examined the nationality distribution across the two groups. Participants who shared the experimenter's nationality i.e., Southeast/East Asian were evenly represented in both groups (Low = 6, High = 8), and the overall distribution of nationalities was similar across groups (see Table 24). Thus, this justified our splitting of participants into Low and High identification with the Experimenter groups based on their self-reported identification score rather than their nationality groups.

Table 24.

Distribution of Participants' Nationalities Across Low and High Identification Groups

Nationality groups	Low Identification with the Experimenter (N = 34)	High Identification with the Experimenter (N = 36)
Belgian	16 (47.1% of the Low Identification group)	20 (55.6% of the High Identification group
Southeast/East Asian (SE/EA)	6 (17.6%)	8 (22.2%)
South Asian & Middle Eastern (SA/ME)	4 (11.8%)	3 (8.3%)
Other European countries	7 (20.6%)	4 (11.1%)
African	1 (2.9%)	1 (2.8%)

Behavioural explicit measures

Prosocial disobedience rate was calculated by dividing the number of trials the agent refused to shock the victim by the total number of trials, whilst the obedience rate was calculated by diving the number of trials participants obeyed the experimenter's orders (both to give/not give shocks) by total trials. Participants' obedience rate was on average 76.1% (*SD* = 23.32), ranging from 29.8% to 100%. The distribution was moderately negatively skewed (-.61), suggesting that more participants had higher obedience scores (Figure 12). For prosocial outcome, the mean prosocial disobedience rate across all participants was 22.4% (*SD* = 23.22, range = 0 - 69.2). The distribution was moderately positively skewed (.67), with a small number of participants disobeying substantially more than the average (Figure 12).

Empathy towards the victim, which was measured randomly in 15 trials asking participants to rate how painful the shock of the very last trial was for the victim (from 0 to 100), was on average of 49.43 (*SD* = 20.39) across all participants with range from 0 to 96.90. The distribution of empathy scores was with a slight negative skew (-.52) which suggests that participants tended to score somewhat higher on empathy (Figure 12). Participants were also asked at the end of the experiment to indicate how responsible did they feel on the scale from 0 to 100, and on average, participants' responsibility rating was 70.86 (*SD* = 24.12) (range = 0 - 100). Likewise, skewness of responsibility ratings was also negative (-1.02) meaning that most participants rated high on feeling responsible for their actions (Figure 12).

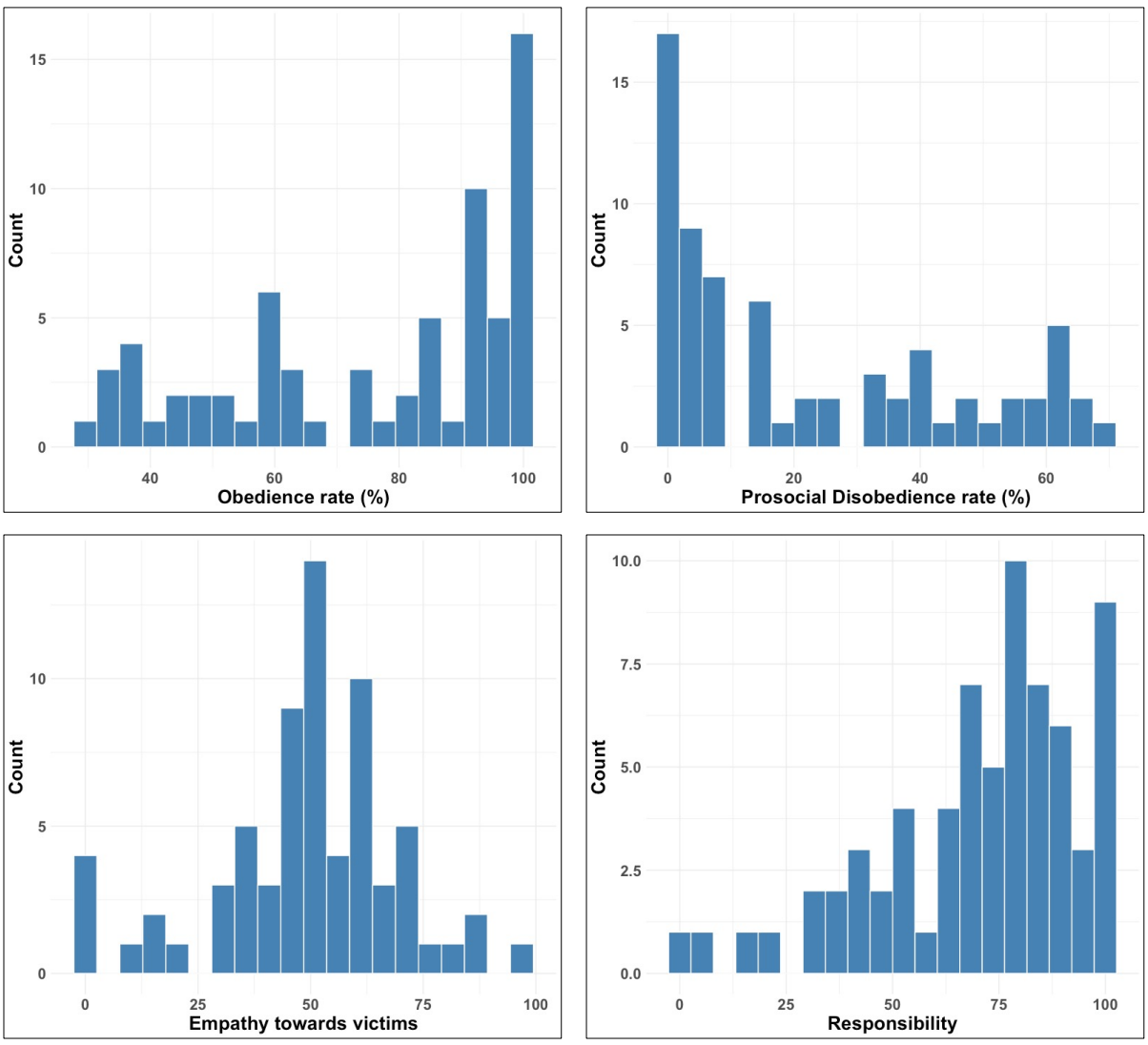


Figure 12. Histograms showing distributions of obedience rate, prosocial disobedience rate, empathy towards victims and responsibility.

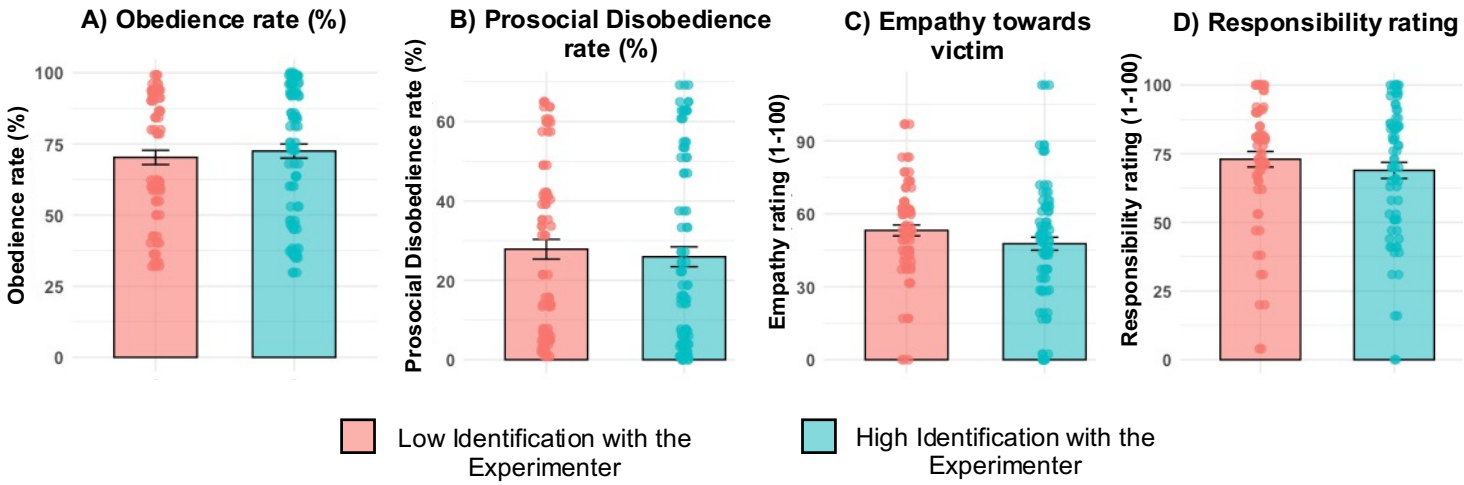


Figure 13. Graphical representations of the comparison of various behavioural outcomes between the low and high identification with the Experimenter, including the mean values of obedience rate (A), prosocial disobedience rate (B), feeling of empathy towards the victim (C) and responsibility rating (D). Error bars represent standard errors. All tests were two tailed. Dots represent individual data.

The two Low and High identification with the experimenter groups did not differ in the prosocial disobedience rate $t(67) = .28$, $p = .780$, $d = .07$; in the obedience rate $t(67) = -.35$, $p = .725$, $d = -.09$; in the level of empathy reported towards the victim $t(67) = .74$, $p = .465$, $d = -.18$; or the rating of feeling responsible for the outcome $t(67) = .69$, $p = .490$, $d = .17$ (all two-tailed) (Figure 13). These results suggested that identification with the experimenter did not have significant effect on the behavioural outcomes of the participants in this coercive context.

Mid-frontal theta activity (FMθ) after hearing the auditory instructions

A 2 ('Give a shock' v. 'Don't give a shock') x 2 (Low v. High Identification) ANOVA was conducted to examine the effects of auditory instruction and level of identification with the experimenter on the FMθ power after hearing the auditory instructions. Results revealed a significant main effect of Coercion to harm $F(1, 66) = 23.35$, $p < .001$, $np^2 = .26$, indicating that FMθ power (dB) was greater when participants heard the instruction to 'give a shock' ($M = 2.86$, $SE = .20$) compared to 'don't give a shock' ($M = 2.17$, $SE = .17$) (Figure 14B). We found neither a significant main effect of Group (Low v. High Identification) $F(1, 66) = .00$, $p = .948$, $np^2 = .00$ nor significant interaction effect $F(1, 66) = .68$, $p = .414$, $np^2 = .01$. Thus, identification with the experimenter did not meaningfully moderate the participants' FMθ after hearing the experimenter's auditory instructions.

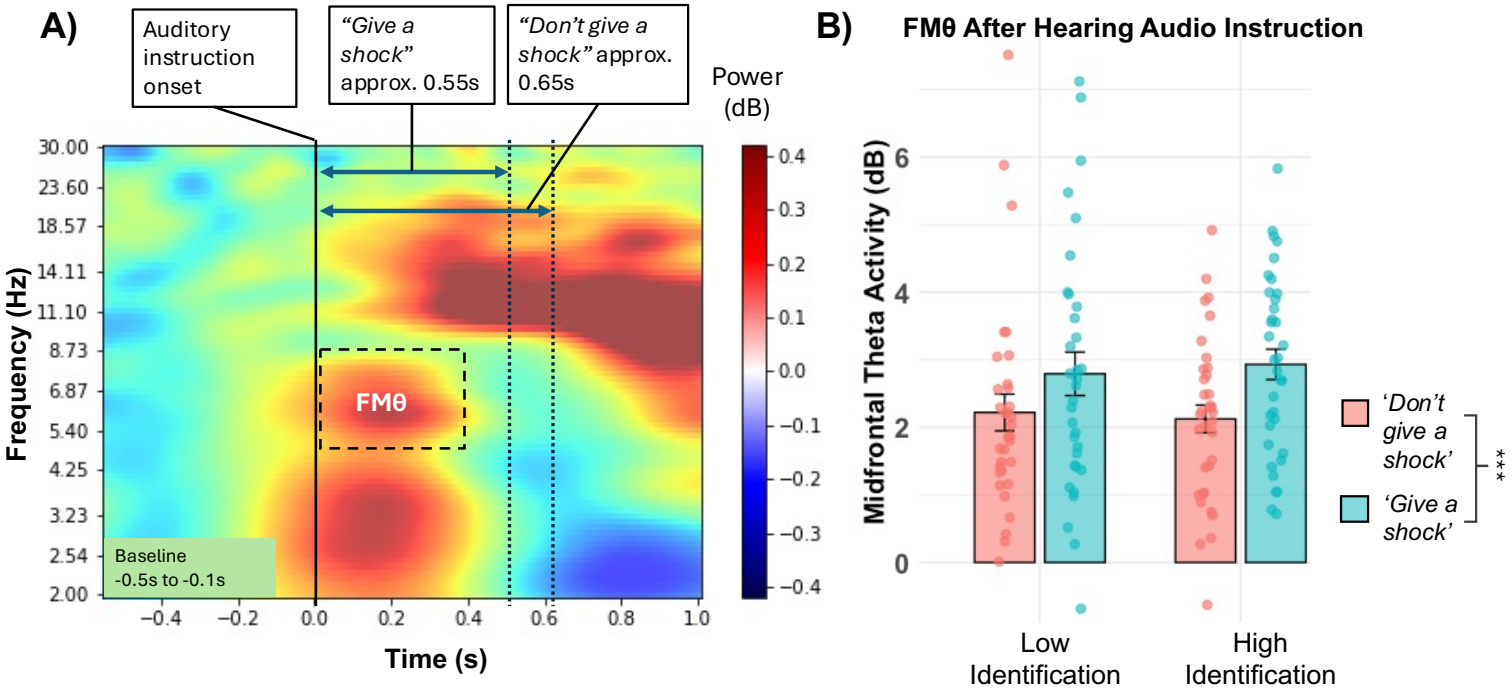


Figure 14. A) Time frequency power plot for the 4 electrodes (Fz, Cz, FC1, FC2) after hearing the instructions with baseline -500ms to -100ms before the onset of the auditory instruction. B) Bar graphs showing the FMθ power after hearing the auditory instructions to give a shock/not to give a shock amongst Low v. High identification with the experimenter groups (right). Error bars represent standard errors. Statistically significant differences have been denoted with asterisks on the graphs with ***$p < .001$. All tests were two tailed. Dots represent individual data.

Given the noisy measure and imbalance in design for neural measures (Bagiella et al., 2000), a Linear Mixed Model (LMM) was also run here with FMθ after auditory instruction as the dependent variable; with a) Coercion to harm: 'Give a shock' v. 'Don't give a shock'; b) Identification with the Experimenter group (2 levels: Low vs. High), and c) their interactions as fixed effects, with participant and nationality group as random effects: *FMθ after auditory instruction ~ Coercion to harm + Identification with the Experimenter Group + Coercion to harm × Identification with the Experimenter Group + (1 | Participant) + (1 | Nationality group)*

Identification with the Experimenter group was treatment-coded with Low identification group as the reference category. Coercion to harm was also treatment-coded with 'Don't give a shock' as the reference category. Significance was assessed via Satterthwaite's method. VIF values were within acceptable ranges (VIFs ≤ 2.24). The random effects showed considerable between-participant variability in the outcome (random intercept

variance = 1.51, *SD* = 1.23). Nationality group showed no meaningful variability (random intercept variance = .01, *SD* = .01). Regarding fixed effects, a significant main effect of Coercion to harm was found that FMθ power (dB) was greater when participants heard the instruction to 'give a shock' compared to 'don't give a shock' similar to ANOVA results above $F(1, 68) = 24.05$, $p < .001$. We found no significant main effect of Group $F(1, 68) = .00$, $p = .947$. The interaction effect was also not significant $F(1, 68) = .70$, $p = .407$.

Mid-frontal theta activity (FMθ) before key press

We conducted a mixed 3 (Conditions: Gave A Shock – Obeyed v. Did Not Shock – Obeyed v. Did Not Shock – Prosocially Disobeyed) x 2 (Group: Low/High Identification with the experimenter) ANOVA on the magnitude of FMθ key press. We found no significant main effects of Condition $F(2, 46) = .23$, $p = .796$, $np^2 = .01$; or Group $F(2, 47) = 1.01$, $p = .320$, $np^2 = .02$. The interaction effect was also not significant $F(1, 46) = 1.14$, $p = .324$, $np^2 = .02$ (Figure 15B).

Linear Mixed Model (LMM) was also run here with FMθ before key press as the dependent variable; with a) Conditions: Gave A Shock – Obeyed v. Did Not Shock – Obeyed v. Did Not Shock – Prosocially Disobeyed); b) Identification with the Experimenter group (2 levels: Low vs. High), and c) their interactions as fixed effects, with participant and nationality group as random effects:

FMθ before key press ~ Condition + Identification with the Experimenter Group + Condition × Identification with the Experimenter Group + (1 | Participant) + (1 | Nationality group)

Identification with the Experimenter group was treatment-coded with Low identification group as the reference category. Condition was also treatment-coded with Did Not Shock – Obeyed as the reference category. Significance was assessed via Satterthwaite's method. VIF values were within acceptable ranges (VIFs ≤ 1.52). The random effects showed considerable between-participant variability in the outcome (random intercept variance = 2.53, *SD* = 1.59). Nationality group showed no meaningful variability (random intercept variance = .02, *SD* = .03). Regarding fixed effects, we found no significant main effects of Condition $F(2, 112.42) = .37$, $p = .690$; or Group $F(1, 57.10) = 1.10$, $p = .299$. The interaction effect was also not significant $F(2, 112.42) = 1.76$, $p = .177$ similar to ANOVA results.

Exploratory analyses showed that favourability towards the experimenter was significantly associated with lower FMθ power before obeying not to shock the victims $r(66) = -.381$, $p = .017$ (FDR corrected), suggesting that positive attitudes towards the experimenter was naturally indicative of the response to the uncomplicated order to not harm others

(Figure 15C). It is worth noting that in Figure 15C there was one notable outlier, so we ran a separate Pearson correlational analysis without this data point and results still showed a significant correlation between favourability towards the experimenter and FMθ power before obeying not to shock the victims $r(65) = -.450$, $p < .001$. Because favourability towards the experimenter showing a highly skewed distribution (see also Figure 15C), we then ran Spearman's rho nonparametric correlation and results still suggested a significant negative Spearman's rho correlation between favourability towards the experimenter and FMθ power before obeying not to shock the victims $r(65) = -.269$, $p = .028$.

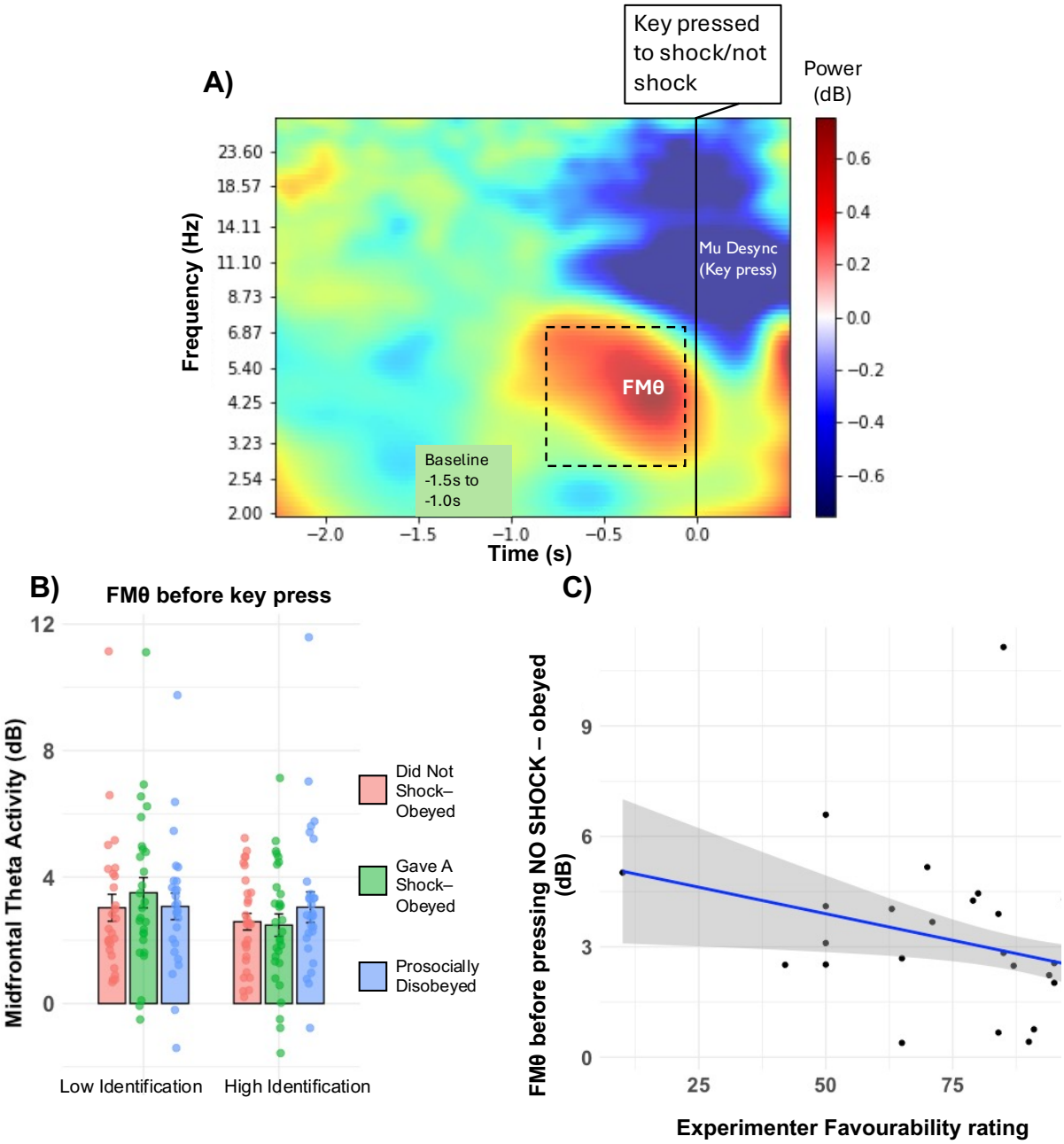


Figure 15. A) Time frequency power plots for the 4 electrodes (Fz, Cz, FC1, FC2) before pressing the key to obey the orders of the experimenter. Mu Desync represents the

desynchronisation in the Mu band (8-12 Hz), which is typical of movement execution (i.e., keypress). B) Bar graph illustrating the FMθ power before executing the key press to give a shock (obeyed), not give a shock (obeyed) and not give a shock (disobeyed). Error bars represent standard errors. Dots represent individual data. C) Graphical correlation between FMθ power before key press to obey and not shock to the victim and Experimenter's Favourability Rating.

Temporal Binding (Sense of agency)

To compute the temporal binding effect, in this experiment we used the Error of Estimation (expressed in ms), where the temporal binding effect was defined as the difference between the perceived interval and the actual interval between the key press and the outcome. To recap, the more negative the values, the stronger the temporal binding effect, which serves as a proxy for a stronger implicit sense of agency.

Given that several participants lacked data for trials where they did not disobey and that individuals differed in temporal perception which thus yielded variability, we utilised Linear Mixed Models (LMMs) instead of repeated-measures ANOVAs for temporal binding analysis. LMMs handle missing data without listwise exclusion and allow random intercepts for each participant.

This LMM was modelled with trial-level data where the dependent variable was the error of estimation for each trial; with a) Outcome (3 levels: Prosocially Disobeyed – Did not shock v. Obeyed – Gave shock v. Obeyed – Did not shock), b) Identification with the Experimenter group (2 levels: Low vs. High), and c) their interactions as fixed effects. Random intercepts were included for Participant and Nationality group, with an additional random slope for Outcome by Participant:

Temporal Binding ~ Outcome + Identification with the Experimenter Group + Outcome × Identification with the Experimenter Group + (1 + Outcome | Participant) + (1 | Nationality group)

Note that for Outcome as the predictor, there was one extra level of Disobeyed – Gave shock i.e., antisocial disobedience. However, we did not include this level in the model because the data was too extreme: half of the participants did not have any trials where they antisocially disobeyed orders and gave shocks to the victim (32/62 participants). For those who did have data (30/62), 25 participants had maximum of 5 trials where they disobeyed orders and gave shock. With such extremity, this would make the dataset to be critically unbalanced and it has been shown that even though LMMs are flexible, massively

unbalanced data still causes issues of false negatives in LMMs output (Hesselmann, 2018). Thus, the predictor Outcome was comprised of three levels with decent data: Prosocially Disobeyed – Did not shock v. Obeyed – Gave shock v. Obeyed – Did not shock.

Group was treatment-coded with Low identification group as the reference category. Significance was assessed via Satterthwaite's method. Outcome was also treatment-coded with Obeyed – Did not shock as the reference category. VIF values were within acceptable ranges (VIFs ≤ 1.45). The random effects showed considerable between-participant variability in temporal binding effect (random intercept variance = 24093, SD = 155.22). Nationality group also showed meaningful variability (random intercept variance = 47878, SD = 218.81), unlike in previous models likely because trial-level data gives better power to estimate it. Participants also varied somewhat in their sensitivity to Outcome as random slope variances of 2211 (SD = 47.02) for Obeyed – Gave Shock and 2121 (*SD* = 46.05) for Prosocially Disobeyed – Did not shock (relative to the reference level Obeyed – Did not shock).

Regarding the fixed effects, there was a significant main effect of Outcome, $F(2, 46.16) = 3.86$, $p = .028$ (Figure 16). Estimated marginal means (EMM) contrasts from the model showed that temporal binding was significantly greater for Prosocially Disobeyed – Did not shock outcome (EMM = -101.6, *SE* = 103) compared to Obeyed – Gave shock outcome (EMM = -75.4, *SE* = 103) $t(48.4) = -2.30$, $p = .026$. EMM temporal binding calculated using error of estimation method for Obeyed – Did not shock outcome (EMM = -96.6, *SE* = 103) was also greater than Obeyed – Gave shock $t(59.8) = -2.13$, $p = .037$. There was no significant difference in temporal binding effect between Obeyed – Did not shock and Prosocially Disobeyed – Did not shock $t(49.4) = .42$, $p = .678$. The main effect of identification group ($F(1, 52.98) = .62$, $p = .436$) and the Outcome × Identification group interaction ($F(2, 46.15) = .089$, $p = .915$) were not significant

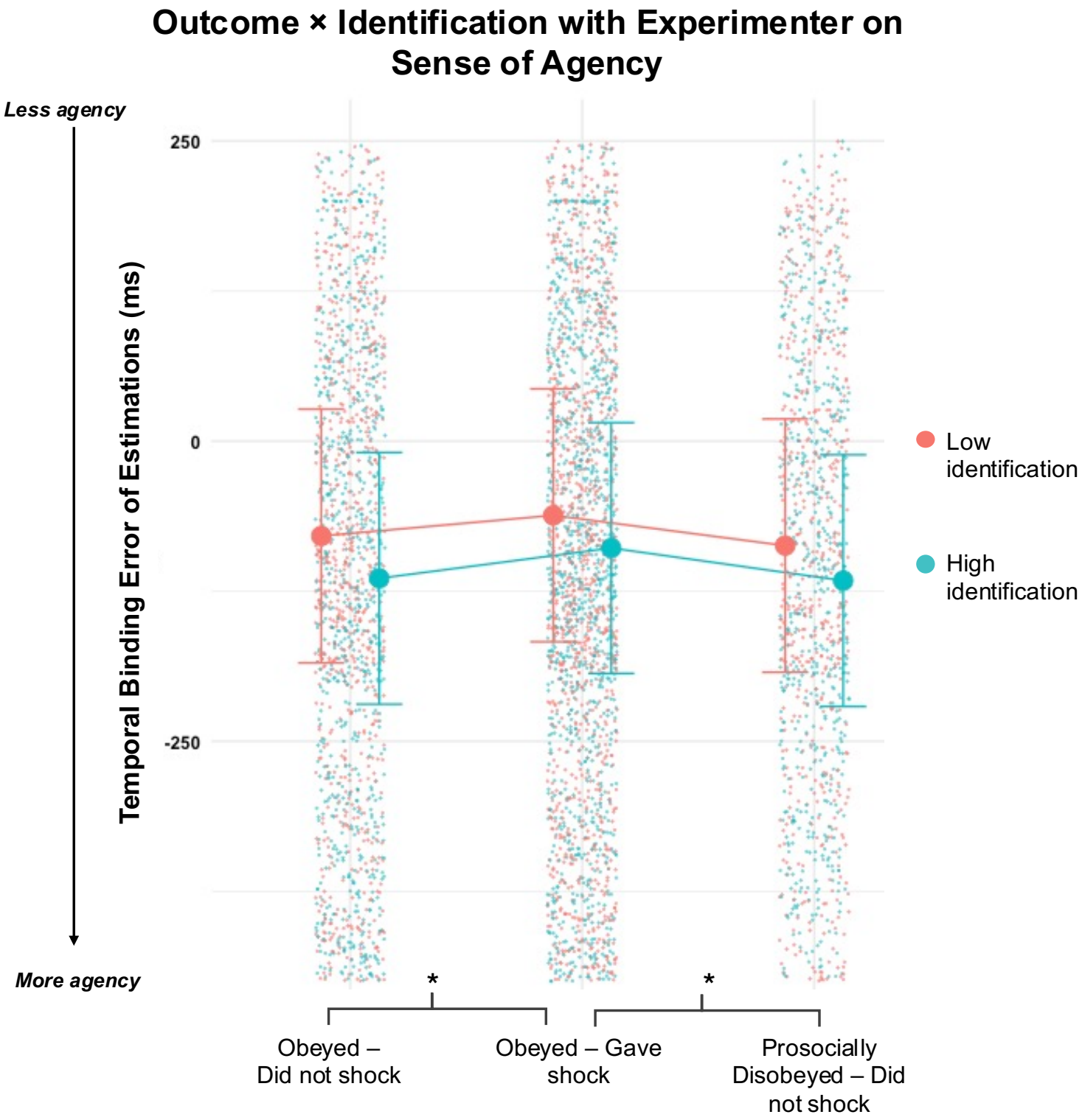


Figure 16. Results from LMMs of Outcome (Prosocially Disobeyed – Did not shock v. Obeyed – Gave shock v. Obeyed – Did not shock) × Identification with Experimenter group (Low v. High Identification) on Temporal binding (implicit sense of agency). Plotted values with smaller and coloured dots show raw individual trial-level participant data, whilst bigger

coloured dots are estimated marginal means from linear mixed models. Error bars represent standard errors. Note that statistically significant differences at the Outcome level for all participants were denoted by asterisks with *$p < .05$.

Prosocial Disobedience – further individual differences factors

To provide further insights into which factors drove the rate of prosocial disobedience, i.e., what contributed to the likelihood of participants disobeying the immoral orders to harm others; exploratory correlational analyses were conducted (using FDR procedure for corrections) between the rate of prosocial disobedience and the socio-cognitive, individual differences and neural measures. Results suggested that there was a significant negative correlation between scores on Right-wing Authoritarianism and the rate of prosocial disobedience, suggesting that individuals scoring higher on Right-wing Authoritarianism were less likely to disobey $r(69) = -.42$, $p = .007$ (FDR corrected). Similar pattern was observed the higher one scored on Authority/Respect, the lower the rate of prosocial disobedience $r(69) = -.41$, $p = .007$ (FDR corrected). Rating of feeling responsible was positively correlated with prosocial disobedience rate that the more responsibility the participants felt over their actions and outcome, the more they disobeyed $r(69) = .28$, $p = .022$ (Before FDR correction) however FDR corrected p was not significant: $pFDR = .155$.

Next, we conducted an exploratory linear mixed-effects model with several socio-cognitive, individual differences and neural measures of interest to provide further insights into which factors drove the rate of prosocial disobedience. Here the dataset used was the one used for calculating temporal binding with N = 62 since the model included temporal binding Prosocially disobeyed as one of the fixed effects. It is also worth noting that even though we measured a lot of variables and questionnaires, we did not include all variables in the model as this risks overfitting. Thus, we selected the most prominent variables based on Caspar et al. (2022) where relevant and added sense of agency, Authority/Respect subscale, RWA and identification with the experimenter as extra predictors (see Table 26). Note that in this model, nationality group (Belgian, SE/EA, SA/ME, Other European and African) was included as a random intercept to account for unexplained variability within each nationality group and thus to obtain more accurate fixed-effect estimates. Nationality group (1=Belgian, 2=SE/EA, 3=SA/ME; 4=Other European; 5=African) i.e., categorical variable was dummy-coded. All continuous predictor variables were standardised (z-scored) prior to analysis to facilitate interpretation and model convergence.

Table 25.

Correlation coefficients between variables in the linear mixed-effects model on Prosocial disobedience of Experiment 7

	Prosocial Disobedience	FMθ after 'Give a shock'	FMθ before key press to obeyed and gave a shock	Sense of Agency Prosocially Disobeyed	Empathy	Responsibility	Identification Experimenter	RWA	Authority/ Respect	Harm/ Care
Prosocial Disobedience	.	.06	-.099	.09	-.203	.275*	-.035	-.416***	-.411***	.087
FMθ after 'Give a shock'		.	.479***	.011	.024	.218	-.147	-.288*	-.235	-.107
FMθ before key press to obeyed and gave a shock			.	-.058	-.036	.099	-.188	-.182	-.086	-.069
Sense of Agency Prosocially Disobeyed				.	.028	-.244	-.102	-.184	-.221	-.104
Empathy					.	.15	-.17	.138	.282*	-.061
Responsibility						.	-.063	-.178	.058	.091
Identification Experimenter							.	.137	.174	.198
RWA								.	.542***	-.091
Authority/Respect									.	.106
Harm/Care										.

*Note. * p <.05; *** p < .001*

Table 26.

Summary of the linear mixed-effects model on prosocial disobedience as the outcome variable

Predictors	*β*	*SE β*	*t*	*p*	95% CI
Intercept	0.23	0.11	1.99	0.052	-0.21, 0.46
1. FMθ after hearing 'Give a shock'	-0.05	0.14	-0.37	0.710	-0.34, 0.22
2. FMθ before key press to obeyed and gave a shock	-0.11	0.12	-0.93	0.359	-0.35, 0.13
3. Sense of Agency Prosocially Disobeyed – Did not shock	0.30	0.27	1.11	0.271	-0.24, 0.84
4. Empathy towards victim's pain	-0.15	0.11	-1.42	0.163	-0.37, 0.06
5. Responsibility	0.24	0.12	2.02	0.048*	0, 0.48
6. Identification with the Experimenter	-0.03	0.11	-0.28	0.785	-0.25, 0.19
7. Right-wing Authoritarianism (RWA)	-0.20	0.14	-1.47	0.148	-0.48, 0.07
8. Authority/Respect	-0.29	0.14	-2.09	0.042*	-0.57, -0.01
9. Harm/Care	0.07	0.11	0.65	0.521	-0.15, 0.3

Note. The table presents the fixed effects from the linear mixed-effects model, including the standardised regression coefficient (*β*), the standard error of the coefficient (*SE β*), t-statistic (*t*), p-value (*p*), and the 95% confidence interval (95% CI) for each predictor. Statistically significant differences have been denoted with asterisks *$p < .05$.

Our model fit indices indicated that the model was an adequate fit: AIC = 147.6, BIC = 171.2, log-likelihood = -61.80. Scaled residuals ranged between -1.59 and 2.42, indicating no extreme outliers. Variance inflation factors (VIF) for all predictors were below 5 and as the correlations between the continuous fixed effects were less than .80 hence no problematic multicollinearity (Table 25). Confidence intervals for key significant predictors (95%) did not include zero (Table 26). The nationality groups as the random intercept had variance of .00, SD = .00 and this suggests that there was no detectable between-nation variation in prosocial disobedience rate. Fixed effects thus explained effectively the outcome variable.

Results showed that similar to the correlational analysis, individuals scoring higher on Authority/Respect subscale were less likely to disobey $\beta = -.29$, $t = -2.09$, $p = .042$. Although the correlation between RWA and prosocial disobedience rate was significant, RWA did not survive the model as a fixed-effect predictor. Lastly, responsibility rating was a

significant predictor: the more responsibility the participants felt over their actions and outcome, the more they disobeyed: $\beta = .24$, $t = 2.02$, $p = .048$.

Discussion

This chapter focussed on understanding how the identification with the experimenter influences the neurocognitive mechanisms involved in deciding whether to obey or disobey the experimenter's harmful orders. The paradigm utilised here allowed us to not only look at the sole outcome of obedience or disobedience but also the neurocognitive processes involved across the decision-making phases (pre-decisional, decisional and post-decisional) (Caspar et al., 2021, 2022). Crucially, by examining the chronological phases of these mechanisms, our study provides further insights into when, how and whether identifying with the experimenter would matter most when it comes to the decisions to obey or disobey immoral orders.

Primary effect of identification with the experimenter on prosocial disobedience

We did not find the effect of identifying with the experimenter on participants' tendency to obey with coercive and harmful commands, nor on their rate of prosocial disobedience. The Low v. High identification with the experimenter groups did not differ in the extent to which they followed the orders to shock the victim or resisted administering painful shocks. Participants in the Low identification group also did not show any differences in feeling of empathy towards the victim or feeling responsible for the outcome of their own actions, suggesting that the effect of identifying with the experimenter on post-decisional behavioural outcomes in the coercive context here was not meaningful.

Thus, in contrary to the body of research supporting the engaged followership model (Haslam & Reicher, 2012; Reicher et al., 2012; Steffens et al., 2014, 2021), we did not find significant effect of the experimenter on obedience or prosocial disobedience in coercive to harm context. This aligns with results obtained in other studies using diverse population samples, where the relationship between identification with the experimenter and disobedience supported the null hypothesis (Caspar et al., 2021, 2022; Seyll et al., 2026). Such diverging results could be due to the differences in the paradigms employed as in the experimental procedures. For instance, in Reicher et al. (2012), participants were made specifically aware that the study they were taking part in was relevant to the Milgram's original obedience study. Furthermore, when participants were asked to rate how much they would identify with the experimenter, who was particularly framed to be a scientist representing the scientific community, participants were told that in Milgram's original

research, the identification with the experimenter had been high whilst with the learner (i.e., victim) had only been moderate, which could have primed participants regarding the experimenter's expectancies. In contrast, our paradigm did not emphasise any components of obedience or identification with the experimenter to the participants before and during the experiment. The participants in the present study, as well as in the previous studies where the null hypothesis was supported (Caspar et al., 2021, 2022; Seyll et al., 2026), were only told that they were taking part in a social decision-making task without any further information about obedience, Milgram's seminal studies, or the experimenter as the representative of the scientific community.

In later studies, Birney et al. (2023) and Haslam et al. (2014) utilised a word-image association task where participants were instructed to assign negative adjectives ('lazy'; 'cruel' etc.) to social groups (family walking in a park etc.) by the experimenter. Compliance was found to be positively predicted by prods referencing scientific goals, supporting engaged followership model wherein obedience depends on identification with a valued scientific cause rather than blind submission. However, such paradigm lacks the ecological validity that the task is far less dramatic and morally charged than administering shocks to a person. Meanwhile, the paradigm we utilised in this chapter allows us to examine the physical and immediate harm that one would cause under coercion, as well as the neuro-cognitive processes associated with it. Here, we found that identification with the experimenter showed no modulation on whether one obeyed or disobeyed. Indeed, a recent study using a paradigm inspired by Milgram also undermines the engaged followership model (Grzyb, Dolinski, Sudoł-Malisz, et al., 2025). The authors demonstrated that when the study was framed as a purely commercial marketing test, participants obeyed to send shocks just as frequently as when they believed that the study served the scientific causes (Grzyb, Dolinski, Sudoł-Malisz, et al., 2025). As a result, compliance with immoral orders is most likely to involve multiple processes that obedience cannot be not sufficiently explained by mere identification with the experimenter and the scientific cause, but rather a mixture of situational and personal factors as we aimed to unpack here in this chapter.

Sense of agency

Overall, we found that temporal binding magnitude was significantly more negative, indicating a stronger sense of implicit agency, when participants disobeyed the experimenter's order to harm the victim, compared to when they obeyed and administered shocks. Such result is somewhat in line with past research. Previous studies have often shown that when the coercive orders or outcome are prosocial i.e., not harming the victim, the

temporal binding effect is generally greater compared to when the given orders or outcome are antisocial/immoral i.e., harming the victim. In particular, temporal binding effect is stronger when agents actually deliver financial harm compared to when they do not (Caspar et al., 2016) ; when agents offer help compared to when they do not help others (Pech & Caspar, 2024); or when participants conform with prosocial orders to not send shocks compared to when participants conform with orders to send shocks (Piperno et al., 2025).

However, focussing on specifically whether prosocial disobedience would increase the sense of agency, this has not been shown in past findings. Here, we found that prosocial disobedience led to greater sense of agency compared to obedience to harm others. Like past research suggest, obeying orders might modify the subjective experience of being the author of such action: the perceived temporal association between actions and the consequences attenuate (Caspar et al., 2021, 2025; Pech & Caspar, 2023). In contrast, here we showed evidence that when one deliberately chooses to defy immoral orders, this results in an increased implicit sense of agency. Thus, the act of prosocial disobedience may reinstate the subjective experience of authorship and control over one's actions. Finally, we did not find an effect of experimenter identification on temporal binding magnitude measured using the interval estimation method. This illustrates that the powerful influence of deliberate prosocial disobedience to avoid harming others on boosting the temporal binding effect (and thus one's sense of agency) is independent of whether or not one identifies with the coercer.

FMθ after auditory instructions and FMθ before key press

Our results demonstrated that FMθ after hearing coercive auditory instructions to administer pain shocks to the victim was significantly greater than after participants heard being instructed not to give shocks. Notably, such effects of enhanced FMθ after hearing immoral orders and weaker FMθ after hearing moral instructions were not modulated by the extent to which participants identified with the experimenter.

We argued that this elevated FMθ indicated a heightened sense of cognitive conflict and increased attentional engagement when participants were processing the coercive immoral orders (Brady et al., 2020). Previous studies showed mixed results regarding if listening to immoral orders are associated with higher FMθ or whether processing morally sound auditory orders heightens FMθ. On the one hand, some studies found the opposite pattern as compared to ours. One study utilised fMRI testing with civilians and found that the non-harmful orders "do not send a shock" resulted in greater activation in the superior temporal gyrus compared to the harm orders i.e., "send a shock" (Tricoche et al., 2024). Likewise, an EEG study whose paradigm we closely followed here also showed that more

FMθ after hearing the instruction to not send a shock than after hearing instruction to send a shock, although the Bayesian test for this was inconclusive (Caspar et al., 2022). On the other hand, a recent EEG study who involved genocide perpetrators, bystanders and rescuers did find similar results observed in our current study with greater FMθ after hearing harmful orders compared to non-harmful orders (Seyll et al., 2026). Such findings could reflect greater attention allocated to harmful and immoral commands amongst individuals who had lived the consequences of obedience firsthand. Taken together, contrasting findings shown here highlight the need to strengthen external validity in social neuroscience research on obedience before making broad theoretical claims.

While FMθ was notably responsive to auditory instructions, we did not find any difference in FMθ before the key press between conditions or identification with the experimenter groups. Past studies also show that the FMθ before sending shocks does not differ from FMθ before not sending shocks (Caspar et al., 2022; Caspar & Pech, 2024). This suggests that, by the time of action execution, cognitive conflict might have already been resolved, regardless of whether the participants decided to give shocks or not to give shocks to the victim. Another explanation could be that because our experiment involved only forced-choice rather than free-choice trials; thus here, participants may merely follow the experimenter's orders which consequentially frees up cognitive load involved in making a decision (Caspar & Pech, 2024; Giersiepen et al., 2023). This interpretation is supported by how participants explicitly report that simply following orders was perceived as easier (Caspar, 2024). Whilst FMθ before key press was not modulated by the level of identifying with the experimenter, we did find that higher favourability towards the experimenter was associated with lower FMθ when obeying instructions not to shock. Thus, the positive attitude towards the experimenter was naturally indicative of the low level of cognitive conflict when processing the morally sound commands. Overall, these findings from the EEG time frequency analyses demonstrated how conflict processing unfolds during the pre-decisional stages of one's action.

Prosocial disobedience – further individual differences factors

Scholars supporting the engaged followership model have advocated the identification with the experimenter is central to one's decision to follow through with orders (Griggs, 2017; Haslam et al., 2014; Haslam & Reicher, 2012). However, from results above, identifying with the experimenter did not modulate the rate of prosocial disobedience and its associated neuro-cognitive mechanisms in this ecological valid paradigm. Thus, one should look beyond the identification with the experimenter as this alone does not explain for

individuals' decision to obey or disobey. Here, we found that prosocial disobedience was indeed modulated by the feeling of responsibility and individual differences factors including Right-wing Authoritarianism and authority/respect subscale of the Moral Foundation questionnaire. With regards to the attribution of personal responsibility, participants who reported to feel highly responsible for their action and the action consequences of harming others; they were more likely to exert prosocial disobedience. Such findings were in line with past studies that the feeling of responsibility positively facilitated prosocial behavioural outcomes (Caspar et al., 2022; Lepron et al., 2015; Piperno et al., 2025; Tricoche et al., 2024).

Related, experimental philosophy work on the distinction between judgements of freedom v. responsibility for spontaneous v. deliberate actions show that deliberate (as opposed to spontaneous) action increased perceived responsibility but decreased perceived freedom (Vierkant et al., 2019). Thus, future work could further look into the element of a) spontaneity and deliberation and b) free choice and force choice too as it would be insightful to contrast whether under free choice condition, if participants have taken longer to decide (i.e., deliberation more than spontaneity) they would rate feeling more responsible but less feeling of freedom (despite the condition is free-choice). For forced choices, Seyll et al., (2026) show that longer decision times were related to higher rates of prosocial disobedience, which suggests that an enhanced reflection time helps resist immoral commands.

However, caution is warranted regarding the sense of responsibility measured here in our study because it was based on a single measure at the end of the experiment. It is also possible that participants who administered more shocks might have underreported their feeling of responsibility at the end as a means of self-serving bias and preserving a positive self-image (Carlson et al., 2020; Caspar et al., 2022). That said, a recent study which utilised an object-destruction paradigm (ordering participants to kill live bugs) did show that obedient participants still reported high responsibility and perceived themselves as responsible factors (Götz et al., 2023). Thus, self-serving bias responses may not always perpetuate and feeling responsible may indeed facilitate prosocial behaviour. Further research should also consider diffusion of responsibility scenarios and the relevance of objectively sharing of responsibility such that the decision to shock or not could be a joint decision with others rather than just oneself. Past studies have shown that social decision-making that involves sharing responsibility allows people to take advantage of the flexibility of the collective context to benefit themselves i.e., the self-serving bias whereby individuals take credit for positive outcomes whilst avoid attribution of responsibility for bad outcomes (El Zein et al.,

2022). Alternatively, future research could look into whether if there is mere presence of other agent (not the experimenter per se) such as a by-stander, this would also moderate feeling of responsibility and decision to act (give a shock v. not to shock) (Beyer et al., 2017).

Importantly, our results also showed the importance of individual differences in Socio-political belief that individuals scoring higher on Right-wing Authoritarianism and Authority/Respect as one's principle of moral foundation were less likely to disobey. These socio-political views have been demonstrated to be strongly influencing individuals' behavioural outcome, especially in the context of coercion, obedience and prosocial dispositions (Blass, 1995; Haidt & Graham, 2007; Krispenz & Bertrams, 2024). Holding such belief might hinder individuals from opting for the right thing i.e., resisting the immoral orders given by authorities. Altogether, these findings show the importance of assessing socio-cultural factors as well since they can shape individuals' propensity to obey or resist immoral orders from authority. Considering the socio-political orientations and moral foundations alongside situational factors could thus offer a comprehensive understanding of prosocial disobedience (Kivikangas et al., 2021).

Limitations

Firstly while we aimed to follow as much as we could with past studies like Caspar et al. (2022), it is acknowledged that instruction processing and conflict processing might have overlapped temporally rather than occur sequentially. Because the first word of the instruction ('Give' vs. 'Don't') already gave away the order, participants might have already begun decision-making and conflict resolution while still hearing the instruction. Therefore, stimulus-locked and action-locked theta effects should not necessarily be interpreted as reflecting distinct cognitive processes and instead they might have reflected a broader engagement of attentional and cognitive control mechanisms across task performance (Cavanagh & Frank, 2014). Secondly, although introducing a longer delay between auditory stimulus presentation and action execution was intended to minimise motor-related confounds, it did not establish when participants began or completed the decision-making process.

Furthermore, due to a coding error identified after participant recruitment had already commenced, the experimental design differed from that of Caspar et al. (2022) such that visual feedback of the victim's hand (i.e., the presence or absence of a visible twitch) was presented immediately following the participant's key press. This unintended modification could introduce a potential visual confound to action-locked neural activity since the observed effects might have reflected both motor-related and visual processing. In addition,

separating the visual and auditory action outcomes could have influenced the temporal binding effect itself, as previous research has shown that the presence of multiple action outcomes can attenuate or alter magnitude of temporal binding (Ruess et al., 2018). However, their results showed that temporal binding does occur for more than one single action-effect and the magnitude of temporal binding appears to be determined by the effect's absolute temporal distance from the action rather than the effect's position in a sequence (i.e., first or second effects after the action) (Ruess et al., 2018). Therefore, the temporal interval between the action and tone might still capture the perceived causal relationship between initiating the action and the resulting outcome sequence (i.e., action and physical consequence then tone), although we acknowledge that the contribution of each individual outcome could not be fully disentangled.

A further limitation pertains to the interpretation of the FMθ effect derived from the stimulus-locked TFR as it was rather immediate relative to the auditory onset than in Caspar et al. (2022) (although close-to-onset stimulus-locked FMθ could also be observed in Seyll et al. (2026) and Piperno et al. (2025)). One possibility is that this might reflect how the first instruction word (Don't v. Give) already revealed the required response as having discussed above. It is also possible that the theta activity identified via the mean +1.96*SD method might partly reflect stimulus-evoked, phase-locked activity (such as the auditory N1/P2 complex) rather than purely non-phase-locked, induced theta oscillations. David et al. (2006) showed that time-frequency approaches do not explicitly decompose total power into its evoked and induced components thus high-amplitude evoked deflections could have produced broadband spectral smearing that overlapped with the theta range, particularly within the 100-250ms latency window typical of the N1/P2 response. Future work should incorporate inter-trial phase coherence analyses or evoked/induced power decomposition to better characterise the oscillatory nature of the observed FMθ response (David et al., 2006; Xia et al., 2023).

Another caveat is that, in the current study, we used a median split primarily for analytic clarity since it allowed us to examine the effect of identification with the experimenter on many neuro-cognitive processes at many stages of decision-making, comparing between relatively lower and higher identifying participants (Iacobucci et al., 2015). However, the measure of identification was continuous and the data could support a more graded analysis. Future research could model identification as a continuous or time-varying variable to capture both individual differences and rapid shifts during the

experimental interaction, in line with recent evidence (Heitmann & Reichardt, 2024; Krishna & Götz, 2024)

Chapter Conclusion

The current chapter aims to assess whether the identification with the experimenter influenced individuals' decision to obey or disobey the immoral commands to inflict painful electric shocks on others. We utilised an ecologically valid paradigm that allows us not only to examine the (dis)obedience as the sole outcome but the mechanisms associated with it. The results showed that the identification with the experimenter did not affect participants' rate of (dis)obedience, temporal binding, or neuro-cognitive conflict processing components as shown via midfrontal theta activity after hearing the experimenter's auditory commands or before pressing the shock key. We did show that temporal binding effect was greater when participants disobeyed orders to harm than when they obeyed to harm, suggesting that such act of disobedience boosted the sense of authorship and feeling of being in control, without the need for conscious or active reflection. Since the identification with the experimenter did not sufficiently explain whether participants disobeyed, the mixed-effects model showed that prosocial disobedience could indeed be explained by several psychological and socio-cultural factors, namely feeling of responsibility; Right-wing Authoritarianism; and Authority/Respect subscale (Moral Foundation questionnaire). Altogether, these results thus illustrate the nuances in individuals' decision to prosocially disobey immoral orders that it must be investigated within various decision-making processes to unfold the individual differences in such coercive and morally charged context.

CHAPTER 6

Socio-political Control: Political Agency and Voting in the 2024 US Election

Experiment 8

In the 2024 US Presidential Election, Donald Trump defeated Kamala Harris, winning 312 electoral votes to Harris's 226 and securing a broader, more diverse coalition than in past campaigns. It goes without saying that such time-sensitive and major life event could impact the US citizens' psychological state to a various extent, depending critically on who was their preferred candidates to win the election and the actual outcome. Indeed, past research on the psychological correlates of election outcomes suggests that it is chiefly the losing side for whom measurable change occurs: Dwyer et al. (2019) reported that supporters of the losing candidate in the 2016 US election showed a marked increase in negative affect and momentary unhappiness in the days following the result. Lench et al. (2019) reported a broadly comparable short-term dip in current happiness among those on the losing side. Critically, both studies pertain primarily to state measures of wellbeing; that is, to momentary mood or current happiness rather than to trait indices such as life satisfaction and the evidence that election 'winners' experience a corresponding boost in wellbeing is considerably weaker and less consistent than the evidence for a loss-related dip. Furthermore, it is unclear whether such major political event as the Presidential Election which is deemed emotionally charged could influence one's sense of agency. From previous chapters, it has been established that by using the temporal binding effect, one could measure the sense of agency implicitly. Evidence from the results of previous chapters at the personal and interpersonal level of control suggests that temporal binding effect can be malleable: it could be shifted by (some but not all) psychological and cognitive processes such as sensorimotor disruption, journaling and coercion. Reviewed past research also showed that learned helplessness, stress or aging etc. could also moderate one's magnitude of the temporal binding effect (Edwards et al., 2025; Mariano et al., 2024; Soral et al., 2021). In this chapter, we extended to the socio-political level: how the aftermath of a major societal event like national election, particularly if one's expectation about political outcomes is confirmed or violated, could alter the temporal binding phenomenon. Experiment 8 thus capitalised on the time-sensitive 2024 US Presidential Election to understand the influence of voting outcome on one's implicit sense of agency.

However, besides just the implicit sense of agency, it is worth situating this question within the much larger and longer-standing literature on political efficacy in political science and political psychology and being explicit about how different this construct is from the implicit sense of agency indexed by the temporal binding effect and from a third, related

construct we also measure: explicit political agency (Figure 17). Firstly, sense of political efficacy is originally defined as "the feeling that individual political action does have, or can have, an impact upon the political process i.e., that it is worth while to perform one's civic duties. It is the feeling that the individual citizen can play a part in bringing about this change" (Campbell et al., 1954, p. 187). Thus, the sense of political efficacy is explicit, deliberative and it concerns a specific domain which is one's relationship to the political system as a citizen (Wolak, 2018). Explicit political agency, by contrast, here we defined as a retrospective, outcome-specific judgement measured after the result was known of how much participants believe they personally influenced the actual electoral outcome. Finally, in contrast to these first constructs, the temporal binding effect indexes an implicit, low-level sense of agency over the immediate sensorimotor consequences of one's own actions. These three constructs reflect different levels of processing (implicit/perceptual versus explicit/deliberative) and at different temporal points relative to the outcome (prospective v. retrospective) and should not be assumed to track one another.



Figure 17. *Three main constructs measured in Experiment 8*

Furthermore, as mentioned about the US political system, the notion of itself is not singular in this context that the US presidential election decided by the electoral college rather than the popular vote so state-level and national-level outcomes can diverge. For instance a voter could back the eventual national winner while their own state's result went the other way, or vice versa. Consequently, 'personal influence over the outcome' could be between influence over the state result and influence over the presidential result. The latter is arguably the more psychologically and politically meaningful outcome given that it is the presidential race not the state count per se that ultimately determines the object of participants' vote choice. This distinction is therefore addressed explicitly in the measures with separate items assessing personal influence over 'who was elected President' v. 'the outcome... in your state' (See Materials).

A related caveat concerns how specific the present context is. The 2024 US Presidential Election took place within a highly polarised two-party system, with an electorate, media environment and level of institutional trust that differ substantially from those of many other democracies, and against the backdrop of a campaign that was itself unusual (e.g., a late candidate substitution, historically high affective polarisation and widespread pre-election expectation of a contested or delayed result) (Dagorn, 2024; FitzGerald, 2024; Miller et al., 2012; Palmer, 2024; Subramanian, 2024). These features may have amplified or dampened any relationship between electoral outcome and political efficacy, sense of agency in ways that would not necessarily generalise to lower-stakes elections, multi-party systems or countries with different institutional structures. We therefore present this chapter as a case study of political efficacy and sense of agency within one particular high-salience electoral context rather than as evidence for a general relationship between a) political outcomes and b) implicit sense of agency, political efficacy and explicit political agency and we are correspondingly cautious about extrapolating beyond it.

To further complicate the matter, the complex political geography of the United States' Partisan voting is structured above the traditional Red (Republican-leaning) and Blue (Democratic-leaning) states dichotomy. Swing states, which are known to not consistently vote for either of the two parties, play the crucial and ultimate powerful role in shaping the election outcome as their votes often determine the outcome of Presidential Elections. In the run-up to the 2024 US election, polls in the weeks ahead predicted a very close contest, particularly in so-called swing states. As in previous elections, expectation was that outcome would be determined by a few thousand voters in those states. Such variability adds to the sense of control voters in these Swing states might feel, as individuals residing in such states might be aware of how their votes carry significant weight in the electoral outcome of the nation (Darmofal & Strickler, 2019). This heightened sense of political relevance might have psychological consequences. Thus, altogether, this chapter asked if the political context of one's residential state i.e., Red, Blue or Swing and the outcome of a high-stakes Presidential Election could influence individuals' implicit sense of agency measured using the temporal binding effect, the sense of political efficacy pre-election and explicit political agency post-election in the specific US 2024 political context.

We recruited individuals residing in these three state groups, measured their temporal binding effect as the proxy for implicit agency one week before the election (5th November 2024) and after the election (3 days/72 hours after the election result was officially

announced i.e., 6th November 2024). We measured explicit political efficacy before the election. Lastly, the explicit political agency was measured after the election wherein we asked if people felt like they had influenced the Presidential Election outcome. Since prior research has examined the impact of election outcome on wellbeing (with the caveats above), we also included satisfaction with life as an index of wellbeing, to examine how this changed from pre- to post-election, while bearing in mind that as a trait measure it might be less responsive to a single election than the state measures used in prior work.

We expected that individuals in Red and Blue state groups would have a polarised baseline expectations about the likely electoral winner, that is, Red states' residents would be most likely to expect Trump to win and Blue states to be most supportive of Harris's campaign. Note that at the time of the study being conducted, especially when data collection of pre-election phase took place, the US 2024 Presidential outcome was still unknown, thus we could not form certain predictions of whether Red states' residents would experience greater sense of agency (both implicit and explicit) because their preferred candidate won. It could be expected that whoever was going to win the Presidential Election, this would boost the sense of agency - both temporal binding and explicit political agency post-election of those living in the corresponding supporting states. Nevertheless, this relationship is not so straightforward and likely to be moderated by voted candidate, such that the change in sense of agency is likely driven by whether the person's own candidate won, not simply by which state they live in. A Harris voter living in a Red state would for example not be expected to show the same post-election increase in temporal binding or explicit political agency as a Trump-voter resident of that same state, even though both are technically Red-state residents.

For individuals living in Swing states, this might be more complex. With the temporal binding effect, Swing states' residents might show greater magnitude of binding before the election compared with their counterparts, reflecting how their votes bore significant weights on the outcome of the Presidential Election. Change in binding effect could be moderated by whom participants of this group had voted for, that individuals of Swing states who had voted for the actual presidential winner would undergo most significant change in temporal binding effect. This is because, unlike Red and Blue states where their expectation alignment of whom to win the Presidential Election might have been clearly mapped out, there was a higher level of uncertainty across Swing states. Thus, the candidate voted for should bear more weight on moderating the temporal binding effect within the Swing states. Regarding explicit measures, we expected Swing state residents to report the highest level of explicit general political efficacy before the election. We also predicted that, after the election, the

Swing state group would feel most influential in the presidential outcome through their votes i.e., political agency, compared with individuals in Red and Blue states. As with the Red/Blue predictions above, we anticipated that these Swing-state effects across temporal binding and explicit political agency alike would not be uniform across all Swing-state residents. Rather it would instead depend jointly on state group and voted candidate which thus reflects the inherently more complex and less predictable relationship between geography and political allegiance in this group.

Method

Participants

Three groups of participants were recruited entailing individuals currently living in Red states (Texas, Alabama, Oklahoma, Idaho, Wyoming), Blue states (California, New York, Illinois, Massachusetts, Washington) and Swing states (Pennsylvania, Michigan, Wisconsin, Arizona, North Carolina) on Prolific. Participants who were eligible to take part must have also been eligible to vote for the 2024 US election. All these criteria including residency and voting eligibility were all pre-screened prior to data collection on Prolific using the platform's pre-screening function.

A prior power analysis using G*Power (Faul et al., 2009) was conducted to determine the sample size needed to detect a medium effect size f = .25 for a mixed ANOVA with 3 between-subject groups (Red, Blue, Swing states) × 2 between-subject factor (Voted for Trump v. Harris) × 2 within-subject measures (Pre & Post-election), power =.80, α =.05. A sample of 162 was required. Considering that the experiment took place online and it was a two-part study, we aimed at recruiting N = 250 for both parts to account for dropouts, bad and lost data. It is worth noting that exploratory analyses using more complex regression would be likely underpowered given the difficulty of estimating power for specific predictors in regression model as suggested by Brysbaert (2019). At time one of the study, which was made live one week (29th October 2024) before the election date (5th November 2024), 99 Blue-State, 99 Red-State and 100 Swing-State participants completed the pre-election part.

At the second part of the study that took place post-election; the experiment went live on the day after the election result (Trump's win) had been announced i.e., 6th November 2024 and stayed opened for the next 72 hours. 89 Blue-State, 89 Red-State and 92 Swing-State participants completed the post-election part, leaving a total of 270 participants who completed both pre- and post-election parts of the research (See Table 27 for information about participants' demographics). Participants were paid at the rate of £12/hour for their

completion of both parts. This study was granted with ethical approval from the Ethic Committee, School of Psychology at the University of East Anglia.

Table 27.

Demographics of each State Group

	Red (N = 89)	Blue (N =89)	Swing (N = 92)
Age	*M* = 41.5y, *SD* = 12.7y	*M* = 40.0y, *SD* = 12.4y	*M* = 44.4y, *SD* = 14.3y
Gender	42% male 58% female	54% male 45% female 1% other (e.g., non-binary)	39% male 59% female 2% other (e.g., non-binary)
Residence	Texas, Alabama, Oklahoma, Idaho, Wyoming	California, New York, Illinois, Massachusetts, Washington	Pennsylvania, Michigan, Wisconsin, Arizona, North Carolina

In this experiment, for analyses involving temporal binding effect as either the dependent variable or independent variable, the final sample used excluded 16 participants (Red states N = 5; Blue states N = 4; Swing states N = 7) whose values of error of estimation either had z-scores exceeded ±3 or did not covary monotonically with veridical intervals (i.e., $r < .4$) (See data exclusion of Results for details about these exclusion criteria). For analyses that included Voted Candidate as a variable, we only included participants who responded as having voted and either having voted for Harris or Trump and excluded participants responding 'other '(Red states N = 3; Blue states N = 4; Swing states N = 4) or 'Prefer not to say' (Red states N = 9; Blue states N = 13; Swing states N = 6). Thus the majority of primary analyses including both temporal binding and voted candidate as variables would therefore use dataset with N = 217.

Additionally, some regression analyses also included party affiliation as a variable, in these analyses only participants responded as either 'None', 'Republican' or 'Democratic' were included and so excluded participants who responded 'Other' (Red states N = 2; Blue states N = 5; Swing states N = 4). Likewise one regression model included Intended Voting Candidate/Candidate choice as a variable, in this analysis only participants responded intending to vote for either Harris or Trump were included and so excluded participants who

responded ‘Other/Undecided/Prefer not to say’ (Red states N = 6; Blue states N = 11; Swing states N = 10).

Design

This study employed a mixed design whereby the within-subject level was a two-level factor: pre- v. post-election, and the between-subject factors were a) the State Group of participants where they resided: Blue v. Red v. Swing states and b) voted Candidate: Trump v. Harris. The primary aim was to examine the effects of state group and election outcome on the magnitude of temporal binding effect at both pre- and post-election timepoints. Additional outcome variables of interest are individuals’ self-reported political efficacy (pre-election) and personal influence on the election outcome (post-election) as the index for explicit political sense of agency in this context.

The interval reproduction paradigm employed in this experiment was identical to which utilised in Chapter 4. All participants completed an interval reproduction task (Humphreys & Buehner, 2010) with two 40-trial experimental blocks, Operant block and Observational Baseline block. To recap, the temporal binding effect was measured by calculating the difference between the error of estimation (perceived – actual) in the operant condition and the error of estimation in the observational baseline condition (ms). Negative reproduction errors indicated more underestimation of the intervals and the more negative the Temporal Binding measure for an individual was, the greater temporal binding effect they showed and thus had a stronger implicit sense of agency.

Materials

The experiments were built to be conducted online using Task Builder 2 functions of the Gorilla.sc platform, similar to Experiment 3 and 4. They were set up to run only on PCs and laptops with the minimal framerate of 60Hz required and a physical keyboard was required; thus, smartphones and tablets were not compatible. As default, the browser window was set to full screen as soon as the link to the experiment was clicked on by the participants.

Introduction and instruction videos with voiceover and animation that helped explain in detail what to expect from the task in both baseline and operant condition were given. In both operant and baseline conditions, all the tones used always lasted for 100ms with a volume of 1kHZ.

Questionnaires

Pre-election.

Political orientation and voting intention.

In the first part of the study i.e., pre-election, we examined which political party the participants identify with: '*Do you identify with a political party? If yes, which one?*' and the responses were a) No I don't identify; b) Yes, with the Republican Party; c) Yes, with the Democratic Party or d) Yes, with a different party. We also asked if participants were eligible to vote in the 2020 Presidential Election: '*Were you eligible to vote in the 2020 Presidential Election? If yes, did you vote?*' and participants chose from one of the following answers: a) No, I was not eligible to vote; b) Yes, and I did not vote; and c) Yes, and I did vote. Voting intention was measured by asking participants: '*Do you intend to vote in the 2024 Presidential Election?*' with four possible responses: a) No; b) I'm not sure yet; c) Yes; and d) I have already voted (through early/absentee voting). Voting confidence was measured on an 11-point scale: '*If you have not yet voted, how confident are you that you will vote on or before election day?*' wherein 0 = Not at all confident; 11 = Completely confident. Another voting intention measure pertained to which candidate the participants intended to vote for: '*If you have voted or intend to vote, which candidate did you/will you vote for?*' and the choices were a) Kamala Harris; b) Donald Trump; c) A different candidate; d) I haven't decided yet and d) Prefer not to say.

Lastly, election outcome prediction in residential state, popular vote and the election was measured using a 6-item Likert scale from -5 = very unlikely to +5 = very likely whereby participants were asked to rate the likelihood of a) *Kamala Harris/ Donald Trump wins the popular vote in your state;* b) *Kamala Harris/ Donald Trump wins the popular vote in the country overall*; and c*) Kamala Harris/ Donald Trump wins the election.*

Political Efficacy (Pre-election).

Political efficacy was measured using a customised 10-item scale rather than an established generalised measure (e.g., the National Election Studies internal/external efficacy items (Craig & Maggiotto, 1982; Morrell, 2003)) because standard efficacy scales assess a broad, system-level disposition which measures participants' general understanding of politics (internal efficacy: e.g., "I feel like I have a pretty good understanding of the important political issues which confront our society") and sense that the political system is responsive to people like them (external efficacy: e.g., "In this country, a few people have all the political power and the rest of us have nothing to say"). These items are not anchored to a

specific election or electoral mechanism and therefore do not capture the construct central to the present research: participants' belief that *their individual vote in this specific presidential election* could influence the outcome, given the electoral college system and the distinction between swing and red/blue states. The present scale was accordingly designed to be proximal and election-specific with items directly referencing the vote impact in the 2024 U.S. presidential election (e.g., "unless you vote in a 'swing' state, it doesn't really matter whether or not you vote at all"). Participants rated agreement with each of the 10 items on an 11-point scale (-5 = *strongly disagree* to +5 = *strongly agree*); negatively worded items were reverse-scored and all items were averaged into a single composite, with higher scores indicating a stronger belief that one's vote would influence the election outcome (See Appendix L for full questionnaire).

Post-election.

Voting outcome.

After the Presidential Election outcome was established, in post-election part of the study participants were asked about outcome belief i.e., '*Who do you believe has won the 2024 US Presidential Election?'* with the following possible choices a) Kamala Harris; b) Donald Trump; or c) I'm not sure – it can still go either way as we wanted to ensure that participants were all aware of the actual outcome (Donald Trump won the election). Participants were also asked if they had voted in the election: '*Did you vote in the election'* with either a) No or b) Yes.

Regarding voting candidate, whilst some participants had voted and others had not, the wordings were similar: '*Who did you vote for'* (If 'Yes' to 'Did you vote…') or '*If you had voted, who would you have been most likely to vote for?*' (If 'No' to 'Did you vote…') and the categorical responses were a) Kamala Harris; b) Donald Trump; c) A different candidate; d) Prefer not to say and for participants who had not voted, there was an extra choice of e) There is nobody that I would have considered voting for.

Explicit political agency (Post-election).

Following the election, 4 separate one-item measures measure assessing the degree to which they felt they had personally influenced the outcome were utilised. Here we distinguished between two levels of outcome: the *national presidential result* ('how much do you feel you personally influenced who was elected President?') and the *state-level result* ('how much do you feel you personally influenced the outcome of the Presidential election in your state?'), with corresponding counterfactual items assessing the perceived

likelihood that each outcome would have been the same had the participant not voted ('How likely do you think it is that the outcome of the election in your state would have been the same if you had voted?' and 'How likely do you think it is that someone else would have been elected President if you had voted?') (or, for non-voters, had they voted). All items were rated from 0 (*not at all*) to 11 (*very much*); non-voters completed a counterfactual version of the same items, framed around who they would have voted for had they voted. Each of these four items were analysed independently as separate variables.

These measures were conceptually and psychometrically distinct from the Time 1 efficacy scale (see above) and treated throughout as separate constructs: retrospective perceived personal causal influence over a known, specific outcome (assessed separately at the state and national level) rather than prospective efficacy in this specific political context. It is worth noting that because it is the presidential result not the state count per se that determines the practical stakes of voting, the Presidential Winner item was taken as the most direct and construct-relevant index of explicit political agency with the remaining three items (state-level influence and the two counterfactual items) retained as complementary.

Satisfaction with Life.

Level of life satisfaction was measured both pre and post-election using the five-item Likert scale (e.g., '*In most ways my life is close to my ideal*') and participants rated how much they agreed or disagreed with these statements whereby 1 = strongly disagree; 7 = strongly agree. All items' responses were computed into a single mean for each participant and the higher the score, the more satisfied with life one reported (Diener et al., 1985).

Procedure

The first part of the study was made live one week (29th October 2024) before the election date (5th November 2024). Participants signed up on Prolific, which then re-directed them to the experiment on Gorilla.sc. Participants were first presented with the information sheet that described briefly the purpose of the experiment and how their data was handled. An embedded consent form whereby participants who agreed to take part would tick all the boxes was then presented. Afterwards, an audio initialisation that tested the audio condition of the participants was implemented to ensure that their headphones or speakers were on.

Participants were subsequently shown an instruction video that explained the task requirements. During the interval reproduction task, participants were instructed that a tone would occur after a first event: either a self-initiated spacebar press with their index finger (Operant) or after a first auditory tone (Baseline). The stimulus interval was the delay

between the first event and the outcome (randomised between 500ms and 1500ms). Participants were immediately asked to make response intervals by pressing and holding down the space bar with their index finger for the duration which they perceived the stimulus interval to have lasted. Afterwards, a fixation cross was presented for 1000ms, and the trial ended. There were 3 practice trials for each Operant v. Baseline block. For the main trials, a block of 40 trials for each condition was in place: with half of the participants undertaking the Baseline block then the Operant block; and the other half undergoing the Operant block followed by a block of the Baseline condition (counterbalancing). This interval reproduction task took approximately 10 minutes to complete.

Having completed the interval reproduction task, participants were presented with the pre-election questionnaires: political orientation and voting intention; political efficacy (pre-election) and the Satisfaction with Life scale to measure pre-election level of life satisfaction. Finally, demographic questions, including age, gender and state residence, were presented. All participants were thanked for completing part one of the study and were informed that part two would be made online and available on the 6th of November 2024.

Participants who completed the time one pre-election were sent invites to take part in time two of the study. It is worth noting that the part two of the study was only live for 72 hours after the election result was out, after this point the participants would no longer be able to partake. The procedure for the online post-election experiment was similar to which of the pre-election online experiment. We also completed the sound check to ensure that participants had the sound on their devices. Participants also completed the interval reproduction task, with similar counterbalancing procedure and three practice trials for each Operant and Baseline block to refresh memory (we omitted the instruction video for time two) before moving to the main trials with similar block structure and also 40 trials per block. Like time one, participants completed the post-election questionnaires upon completion of the interval reproduction task, including voting outcome, explicit political agency measures and post-election satisfaction with life. All participants were thanked and debriefed at the end of the online session.

Results

Note that for all regression models, all continuous variables were scaled i.e., applying standardisation (also known as z-score normalisation) to all numeric variables in the original dataset since variables entered had considerably different units of measure. This ensured that all numeric variables contributed equally to the analysis and prevented variables

with larger scales from skewing the findings. Additionally, all models had VIFs<10 as a check for multicollinearity.

Political Profile

Table 28.

Party affiliations, voting intentions and actual voted candidates across the State groups

Measure	Response	Red (N = 89)	Blue (N = 89)	Swing (N = 92)
Party	None	16 (18.0%)	17 (19.1%)	22 (23.9%)
	Republican	33 (37.1%)	27 (30.3%)	22 (23.9%)
	Democrat	38 (42.7%)	40 (44.9%)	44 (47.8%)
	Other	2 (2.2%)	5 (5.6%)	4 (4.3%)
Vote Intention	No	2 (2.2%)	10 (11.2%)	3 (3.3%)
	Unsure	4 (4.5%)	8 (9.0%)	5 (5.4%)
	Yes	66 (74.2%)	63 (70.8%)	60 (65.2%)
	Already voted	17 (19.1%)	8 (9.0%)	24 (26.1%)
Intended voting candidate/ Candidate choice	Harris	46 (51.7%)	40 (44.9%)	53 (57.6%)
	Trump	37 (41.6%)	33 (37.1%)	26 (28.3%)
	Other	2 (2.2%)	6 (6.7%)	3 (3.3%)
	Undecided	4 (4.5%)	7 (7.9%)	7 (7.6%)
	Prefer not to say	0 (0.0%)	3 (3.4%)	3 (3.3%)
Actual Voted Candidate	Harris	38 (42.7%)	37 (41.6%)	51 (55.4%)
	Trump	39 (43.8%)	37 (41.6%)	31 (33.7%)
	Other	3 (3.4%)	3 (3.4%)	4 (4.3%)
	Prefer not to say	9 (10.1%)	12 (13.5%)	6 (6.5%)

To evaluate whether the Red/Blue/Swing state groupings reflected the anticipated partisan patterns, we examined party affiliation, candidate choice, and actual reported vote candidate across the three groups (Table 28). Red and Blue states in this sample were closely divided between Harris and Trump voters (Red: 42.7% Harris vs. 43.8% Trump; Blue: 40.4% vs. 40.4%), while the Swing group showed the clearest lean toward a single candidate (55.4% Harris vs. 33.7% Trump). Republican party identification followed a pattern broadly consistent with expectation (highest in Red states, lowest in Swing states), though Democrat identification was similar across all three groups.

Taken together these results suggest that because the state groupings were not unbalanced in terms of raw sample sizes or overall partisan composition, the labels Red, Blue

and Swing should therefore be interpreted as reflecting each state's official electoral classification rather than the ideological composition of the current sample (which showed comparable division across all three state groups). In other words, although the groups are labelled 'Red', 'Blue' and 'Swing' the people in those state groups did not necessarily have the partisan makeup or voting behaviours one would expect based on those labels. Therefore, any differences found between the state groups might have been driven by the particular mix of party affiliation or voted candidate rather than by living in a red, blue or swing state itself.

Table 29.

Summary of (mis)match between candidate choice and actual voted candidate

Transition Category	Red (N = 89)	Blue (N = 89)	Swing (N = 92)
Stable Harris	37 (41.6%)	33 (37.1%)	48 (52.2%)
Stable Trump	33 (37.1%)	29 (32.6%)	25 (27.2%)
Stable Other	2 (2.2%)	3 (3.4%)	2 (2.2%)
Total Actual matching with intended	*80.9%*	*73.0%*	*81.5%*
Harris → Trump	4 (4.5%)	1 (1.1%)	2 (2.2%)
Trump → Harris	1 (1.1%)	1 (1.1%)	1 (1.1%)
Other → Trump/Harris	0 (0.0%)	1 (1.1%)	1 (1.1%)
Undecided → Harris	0 (0.0%)	2 (2.2%)	2 (2.2%)
Undecided → Trump	2 (2.2%)	3 (3.4%)	2 (2.2%)
Undecided → Other	1 (1.1%)	0 (0.0%)	0 (0.0%)
Prefer Not to Say responses	9 (10.1%)	16 (18.0%)	9 (9.8%)

We also looked at whether voting behaviour showed high stability between intended and actual vote across all three state groups, with the large majority of participants actually voted for their intended candidate choice categories and only a small minority switching candidates (≤ 4.5% in any single transition category) (Table 29). Swing-state participants showed the highest correspondence between intended and actual vote (81.5%) comparable to Red states (80.9%) while Blue-state participants showed somewhat lower matching (73.0%) which was largely driven by a higher rate of 'Prefer not to say' responses (18.0%) rather than greater candidate-switching per se.

Data exclusion

Similar to Experiment 2, 3, and 4 , to control for outliers in the interval reproduction task, we calculated the z-scores for each trial of every participant and eliminated the trials a) whose z-scores exceeded ±3, and b) trials with reproductions of less than 100ms (i.e., merely pressing down the space bar rather than holding it down to reproduce the interval), resulting

in 3.11% of total trials removed. We then proceeded to control for outliers across participants that individuals with z-scores of their mean error of estimation greater than ±3 for at least one of the four blocks (Baseline time 1; Baseline time 2; Operant time 1; Operant time 2) were excluded from the primary analysis. There were 9 participants (2 in Red states, 3 in Blue states and 4 in Swing states) whose z-scores of errors of estimation which had exceeded the thresholds.

Also similar to Experiment 2, 3, and 4; further following exclusion criteria was applied, consistently with previous studies involving the same methodology (Pascolini et al., 2021, 2025) that participants not producing reproductions covarying monotonically with veridical intervals (i.e., $r < .4$) were excluded and we identified 7 participants (3 in Red states, 1 in Blue states and 3 in Swing states) who failed to meet the criteria. For analyses that included Voted Candidate as a variable, we only included participants who responded either having voted for Harris or Trump and excluded participants responding 'other '(Red states N = 3; Blue states N = 3; Swing states N = 4) or 'Prefer not to say' (Red states N = 9; Blue states N = 12; Swing states N = 6). Analyses including both temporal binding and voted candidate as variables would therefore use dataset with N = 217. Additionally, some analyses also included party affiliation as a variable, in these analyses only participants responded as either 'None', 'Republican' or 'Democratic' were included and so excluded participants who responded 'Other' (Red states N = 2; Blue states N = 5; Swing states N = 4). Likewise, some analyses also included Intended Voting Candidate/Candidate choice as a variable, in these analyses only participants responded intending to vote for either Harris or Trump were included and so excluded participants who responded 'Other/Undecided/Prefer not to say' (Red states N = 6; Blue states N = 11; Swing states N = 10).

State Groups and Voted Candidate groups split

Chi-square tests of independence were conducted to examine a) if the three state groups differed in three major party affiliation responses (None, Republican, Democratic) $\chi^2(4) = 3.66$, $p = .455$; b) if pre-election candidate choice (Harris, Trump) differed between state groups $\chi^2(2) = 3.11$, $p = .211$, and c) actual voted candidate (Harris, Trump), $\chi^2(2) = 3.35$, $p = .187$, differed significantly across state groups. All of these results were non-significant which suggested that the state groups were reasonably balanced on these measures.

Main analysis – Temporal Binding, State Groups and Voted Candidate

Firstly we conducted a mixed 4 (Baseline Time 1 v. Operant Condition Time 1 v. Baseline Time 2 v. Operant Time 2) × 3 (State groups: Red v. Blue v. Swing) × 2 (Voted candidate (Harris v. Trump) ANOVA on the mean error of estimation (ms). Chi-squared tests above showed that the number of participants voted for Harris v. number of participants voted for Trump did not differ significantly across the three state groups, thus the State group × Voted candidate interactions would not have significantly unbalanced groups. For this analysis, assumption checks were assessed that inspection of histograms indicated that the residuals were approximately normally distributed. Levene's tests indicated homogeneity of variance across groups (*p*s $\geq$ .239). Mauchly's test indicated that the assumption of sphericity was not met because $p < .001$ so Greenhouse-Geisser corrections were applied. No extreme outliers were identified as having been removed as above.

There was a significant main effect of Baseline v. Operant and Time 1 v. Time 2 on mean error of estimation $F(2.38, 501.43) = 33.18$, $p < .001$, $\eta p^2 = .14$ (Greenhouse-Geisser corrected). Mean error of estimation was more negative in both Operant Time 1 (M = -373.61, SE = 23.19) and Operant Time 2 (M = -390.06, SE = 22.99) compared with Baseline Time 1 (M = -225.96, SE = 26.56) and Baseline Time 2 (M = -206.52, SE = 27.56) (all *p*s < .001) (See Figure 18). That is, participants underestimated the interval between a voluntary action and the produced tone significantly more than the interval between two observed events in both time points, in line with past research using the interval reproduction paradigm to measure the implicit sense of agency (Humphreys & Buehner, 2010). Meanwhile, the mean error of estimation in Baseline Time 1 did not differ from which of Baseline Time 2 (p = .311) and error of estimation in Operant Time 1 also did not differ from Operant Time 2 (p = .393).

There was a significant main effect of State group $F(2, 211) = 4.76$, $p < .001$, $\eta p^2 =$ .04 that participants residing in Red states (M = -387.43, SE = 35.32) did show significantly more negative error of estimation in general compared with those in Blue states' (M = -264.90, SE = 36.39, p = .017) and Swing states (M = -244.78, SE = 35.23, p = .005). Mean error of estimation did not differ between Blue and Swing states (p = .692). However, there was no significant interaction effect between State Group × Time $F(4.75, 501.43) = .65$, $p =$ .657, $\eta p^2 = .01$ (Greenhouse-Geisser corrected). This suggests that although Red states' participants underestimated the intervals a lot more in both Baseline and Operant conditions compared to the other two state groups, the absence of a significant interaction indicates that the pattern of greater underestimation in Operant compared to Baseline conditions was

consistent across all state groups and time points (See Figure 18). This supports the validity of computing the temporal binding effect as the difference between Operant and Baseline mean error of estimation. Comparing changes in temporal binding between Time 1 and Time 2 across state groups is appropriate as these differences might reflect variations in agency-related binding rather than baseline differences in temporal estimation accuracy. Other main effects and interaction effects were not significant (See Table 30).

Table 30.

Mixed ANOVA State Groups × Voted Candidate × Pre- v. Post-election on mean error of estimations

Effects	*F*(df)	*p*	ηp^2
State Group (Red, Blue, Swing states)	$F(2, 211) = 4.76$	.001	.04
Voted For (Harris v. Trump)	$F(1, 211) = .00$	.997	.00
Time (Pre & Post-election)	$F(2.38, 501.43) = 33.18$	001	.14
State Group × Time	$F(4.75, 501.43) = .65$	.657	.00
State Group × Voted For	$F(2, 211) = .64$	.528	.01
Voted For (Harris v. Trump) × Time	$F(2.38, 501.43) = 2.54$	.070	.01
State Group × Time × Voted For	$F(4.75, 501.43) = .53$	.748	.01

As for the primary analysis, we then calculated temporal binding effect by deriving the difference in mean error of estimation between Operant and Baseline condition (ms). A mixed ANOVA with 3 between-subject groups (Red, Blue, Swing states) × 2-level between-subject factor (Voted for Harris v. Trump) × 2-level within-subject measure (Pre & Post-election) on the temporal binding effect was conducted. Assumption checks were assessed that inspection of histograms indicated that the residuals were approximately normally distributed. Levene's tests indicated homogeneity of variance across groups ($ps \geq .058$). Mauchly's test was not needed here because the within-subject factor only had 2 levels. No extreme outliers were identified. Results showed that there were no significant main effects and interaction effects (Table *31*). Altogether, contrary to our predictions, these results suggested that the temporal binding effect was not modulated by State group or Voted candidate (See Table 32 for full break-down of descriptive statistics).

Table 31

Mixed ANOVA State Groups × Voted Candidate × Pre- v. Post-election on Temporal Binding

Effects	*F*(df)	*p*	ηp^2
State Group (Red, Blue, Swing states)	$F(2, 211) = .81$	.447	.01
Voted For (Trump v. Harris)	$F(1, 211) = 3.35$	.069	.02
Time (Pre & Post-election)	$F(1, 211) = 2.30$	.131	.01
State Group × Time	$F(2, 211) = .16$	.853	.00

Effects	*F*(df)	*p*	ηp^2
State Group × Voted For	*F* (2, 211) =.41	.664	.00
Voted For (Trump v. Harris) × Time	*F*(1, 211) = 3.74	.054	.02
State Group × Time × Voted For	*F*(2, 211) = .73	.484	.01

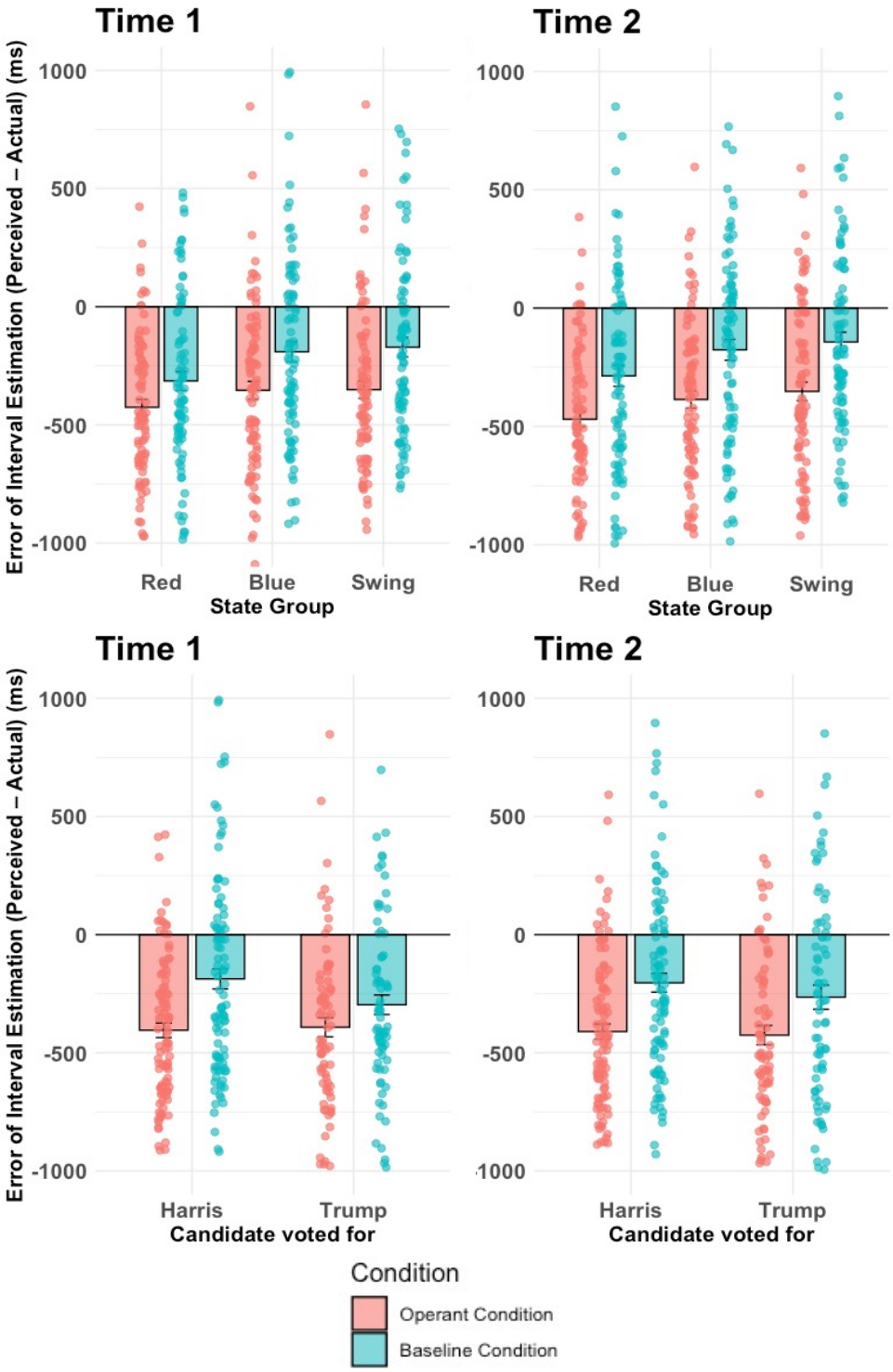


Figure 18. Graphs showing the mean error of estimation (ms) in the Operant and Baseline condition for each State Group (Top) and Voted Candidate (Bottom) and at both Time 1 i.e.,

Pre-election and Time 2 i.e., Post-election. Error bars represent standard errors. Dots represent individual data.

Table 32.

Descriptive statistics for temporal binding effect split by State group and voted candidate

	State group	Voted For	Mean	SD	N
Temporal Binding Time 1 (Operant – Baseline) (M = -147.66, SE = 25.78)	Red	Harris	-115.11	255.32	37
		Trump	-84.25	295.93	35
		Total	-100.11	274.29	72
	Blue	Harris	-262.19	411.29	36
		Trump	-84.47	436.96	32
		Total	-178.56	429.76	68
	Swing	Harris	-253.27	419.67	48
		Trump	-86.66	390.03	29
		Total	-190.52	414.21	77
	Total	Harris	-213.68	377.10	121
		Trump	-85.05	372.14	96
		Total	-156.77	379.49	217
Temporal Binding Time 2 (Operant – Baseline) (M = -183.54, SE = 23.52)	Red	Harris	-162.97	239.09	37
		Trump	-145.33	375.30	35
		Total	-154.40	310.66	72
	Blue	Harris	-216.82	316.78	36
		Trump	-187.25	331.92	32
		Total	-202.90	321.89	68
	Swing	Harris	-220.99	394.99	48
		Trump	-167.88	359.35	29
		Total	-200.99	380.44	77
	Total	Harris	-202.01	328.94	121
		Trump	-166.11	353.17	96
		Total	-186.13	339.54	217

Predictors of change in Temporal Binding Post-Election – Pre-Election

Variables of both pre-election (Time 1) and post-election (Time 2) were entered into the current regression model to test what best predicted the change in temporal binding effect from Time 1 to Time 2. In other words, we explored whether what people reported in pre-election questionnaires would predict the change in the implicit sense of agency, alongside those measured at Time 2. First, the dependent variable would be the change in temporal binding effect, computed by subtracting the temporal binding effect pre-election from the

post-election binding effect. This means that the more negative this value, the more temporal binding effect participants showed post-election compared to pre-election.

In the model, the following variables measured at Time 1 were entered: State groups (Red, Blue, Swing), Party Identification (None, Democratic, Republican), Voting Confidence, Explicit Political Agency, Age, Trump Likelihood Winning Election Prediction, Harris Likelihood Winning Election Prediction. Time 2 i.e., post-election predictors were Voted Candidate (Harris, Trump), Impact of No Vote on State Outcome ("*How likely do you think it is that the outcome of the election in your state would have been the same if you had not voted?*"), Impact of No Vote on Presidential Outcome ("*How likely do you think it is that someone else would have been elected President if you had not voted?*"), and Perceived influence on Presidential Winner ("*How much do you feel that you personally influenced who was elected President?*").

Life Satisfaction was also included in the model. To avoid multicollinearity and overfitting with too many predictors, we computed a single predictor of change in level of life satisfaction by calculating the different between post-election and pre-election satisfaction with life.

Initially, we were going to include Perceived Influence on State Outcome ("*How much do you feel that you personally influenced the outcome of the Presidential election in your state?*") also as a predictor in the model since it might be relevant to understand whether participants felt like they had influenced state outcome could predict temporal binding change; but the correlation coefficient between Perceived Influence on State Outcome and Perceived influence on Presidential Winner was high $r(171) = .826$, $p < .001$ so Perceived Influence on State Outcome was dropped. We retained Perceived Influence on Presidential Winner over Perceived Influence on State Outcome, as the former reflected the final national election outcome rather than a state-level component of it.

State groups, Party Identification (None, Republican, Democratic) and Voted Candidate (Harris, Trump) as categorical variables were dummy-coded before analysis. For State group, Red states was coded as the reference category yielding 2 dummy variables. For Party Identification, Republican was coded as the reference category, also yielding 2 dummy variables. For Voted Candidate, Harris was coded 0 and Trump was coded 1.

The assumptions of regression for the model were checked. Multicollinearity was not problematic, as the correlations between predictor variables were all less than .80 (See Table 33) and VIF was less than 10 (VIF ≤ 3.39). The histogram of residuals showed an approximately normal distribution, which suggests that the assumption was met

(Appendix I). Homoscedasticity was likely met because the scatterplot showed a similar variance in residuals across predicted values (Appendix I). There were no multivariate outliers. The highest Cook's Distance value was .272.

The regression model was not significant $F(14, 196) = 1.12$, $p = .344$, explaining only 7.40% of the variance in how much change in temporal binding post- v. pre-election. The only significant predictor was explicit political agency measured pre-election $\beta = .18$, $t = 2.08$, $p = .039$ but this result should not be overinterpreted given the non-significant omnibus test and the number of predictors included thus not a reliable effect. There were no other significant predictors (See Appendix G).

Table 33.
Correlation coefficients between variables in the regression model with change in Temporal Binding as outcome variable of Experiment 8 (N = 206)

	Voted for (Harris v. Trump)	State group (Red v. Blue)	State group (Red v. Swing)	Party Identification (Republican v. Democratic)	Party Identification (Republican v. None)	Change in TB	Explicit Political Agency	Voting Confidence	Age	Trump Likelihood Winning	Harris Likelihood Winning	Life satisfaction change	Impact of No Vote on Presidential Outcome	Impact of No Vote on State Outcome	Perceived Influence on State Outcome	Perceived influence on Presidential Winner
Voted for (Harris v. Trump)	.	.066	-.114	.011	-.695***	-.139*	-.113	.015	-.139*	.596***	-.648***	.027	-.158*	.321***	.347***	.374***
State group (Red v. Blue)		.	-.501	.003	.010	.005	-.165*	-.057	-.125	.130	-.125	-.036	.051	.113	.082	.040
State group (Red v. Swing)			.	.064	.060	.028	.159*	.085	.182**	-.179**	.207**	.030	.020	-.041	.023	-.046
Party Identification (Republican v. Democratic)				.	-.462	-.066	-.108	-.163*	.014	-.045	-.009	-.086	.077	.074	-.035	-.021
Party Identification (Republican v. None)					.	.124	.079	.087	.061	-.474***	.595***	-.028	.092	-.154*	-.249***	-.274***
Change in TB						.	.185**	.104	.011	-.011	.124	-.002	.011	.025	.055	.000
Explicit Political Agency							.	.337***	.185**	-.091	.139*	-.055	-.220***	.054	.135	.144*
Voting Confidence								.	.187**	.020	.062	-.170*	.025	-.071	-.004	-.016
Age									.	-.156*	.130	-.019	.081	-.083	-.144*	-.129
Trump Likelihood Winning										.	-.672***	-.007	-.136*	.232***	.334***	.307***
Harris Likelihood Winning											.	-.074	.102	-.069	-.223***	-.257***
Life satisfaction change												.	-.093	.035	-.048	.036
Impact of No Vote on Presidential Outcome													.	-.326***	-.340***	-.297***
Impact of No Vote on State Outcome														.	.500***	.498***
Perceived Influence on State Outcome															.	.826***
Perceived influence on Presidential Winner																.

Note. Perceived Influence on State Outcome was not included in the regression model because of high correlation with Perceived influence on Presidential Winner. As state group is a three-level categorical variable, it was dummy-coded (with Red states as the reference category) for inclusion in the correlation matrix. Likewise, party affiliation as a three-level categorical variable was dummy-coded (with Republican as the reference category) * $p < .05$, ** $p < .01$, ***$p < .001$

Pre-election political efficacy and other variables at Time 1

Pre-election political efficacy

A between-subject ANOVA with 3 between-subject group (Red, Blue, Swing states) × 2-level between-subject factor (Voted Candidate: Harris v. Trump) on the level of pre-election political efficacy was conducted. Assumption checks were assessed that inspection of histograms indicated that the residuals were approximately normally distributed. Levene's test indicated homogeneity of variance across groups (p = .444). No extreme outliers were identified. Results showed that there were no significant main effects of State group $F(2, 211) = .81$, $p = .773$, $\eta p^2 = .00$ nor Voted Candidate $F(1, 211) = .711$, $p = .711$, $\eta p^2 = .00$. There was also no significant interaction effect of State group × Voted candidate $F(2, 211) = 2.10$, $p = .074$, $\eta p^2 = .03$ (See Table 34 for descriptive statistics).

Table 34.

Descriptive statistics for political efficacy split by State group and voted candidate

State group	Voted For	Mean	SD	N
Red	Harris	2.99	2.20	37
	Trump	2.50	2.30	35
	Total	2.75	2.25	72
Blue	Harris	2.37	2.03	36
	Trump	3.26	1.99	32
	Total	2.79	2.04	68
Swing	Harris	3.34	1.67	48
	Trump	2.63	2.17	29
	Total	3.07	1.89	77
Total	Harris	2.94	1.98	121
	Trump	2.79	2.16	96
	Total	2.88	2.06	217

Other Time 1 variables

A series of exploratory one-way ANOVAs examined whether all other variables measured in Time 1 differed as a function of state category (Blue, Red, Swing), reported in Table 36. Significant effects were found for expectations that Trump would win the state vote $F(2, 214) = 24.61$, $p < .001$, $\eta p^2 = .19$ and for expectations that Harris would win the state vote, $F(2, 214) = 21.25$, $p < .001$, $\eta p^2 = .17$ in both cases driven by Red states differing from Blue and Swing states (all ps < .001), with no significant difference between Blue and Swing states.

Significant effects were also found for expectations regarding the national outcome: Trump winning the election, $F(2, 214) = 3.35$, $p = .037$, $\eta p^2 = .03$; Harris winning the election, $F(2, 214) = 3.97$, $p = .020$, $\eta p^2 = .04$; and Harris winning the popular vote, $F(2, 214) = 3.24$, $p = .041$, $\eta p^2 = .03$ in each case driven primarily by the Swing vs. Blue comparison (all ps $\leq .045$), with other pairwise comparisons non-significant. Trump winning the popular vote, life satisfaction (Time 1), and voting confidence did not differ significantly by state category (all ps $> .07$) (See Table 35 for descriptive statistics).

Table 35.

Descriptive statistics for all variables measured at Time 1 for state groups

Variables	Overall mean (SD)	Red	Blue	Swing
Trump Wins Popular Vote	-0.01 (3.21)	0.33 (3.19)	0.40 (3.16)	-0.68 (3.20)
Trump Wins State Vote	0.62 (3.49)	2.65 (2.73)	-0.99 (3.57)	0.13 (3.15)
Trump Wins Election	0.62 (3.03)	0.90 (3.00)	1.12 (2.77)	-0.08 (3.17)
Harris Wins Election	0.89 (3.13)	0.50 (3.18)	0.41 (2.88)	1.69 (3.17)
Harris Wins Popular Vote	1.30 (3.15)	1.08 (3.35)	0.74 (3.16)	2.00 (2.83)
Harris Wins State Vote	0.32 (3.63)	-1.71 (3.47)	1.79 (3.38)	0.91 (3.14)
Life Satisfaction (Time 1)	4.58 (1.47)	4.53 (1.50)	4.68 (1.51)	4.55 (1.43)
Voting Confidence	9.94 (2.09)	9.86 (2.16)	9.76 (2.33)	10.18 (1.78)

Table 36

ANOVA Results Comparing State Group Differences on all measured variables in Time 1

Measure	F (2, 214)	p	ηp^2	Blue v. Red	Swing v. Red	Swing v. Blue
Trump Wins Popular Vote	2.66	.073	.02	diff = .06, p = .992	diff = -1.01, p = .132	diff = -1.07, p = .109
Trump Wins State Vote	24.61	.000	.19	diff = -3.64, p = .000***	diff = -2.52, p = .000***	diff = 1.12, p = .088
Trump Wins Election	3.35	.037	.03	diff = 0.21, p = .905	diff = -0.98, p = .115	diff = -1.20, p = .045*
Harris Wins Election	3.97	.020	.04	diff = -0.09, p = .984	diff = 1.19, p = .051	diff = 1.28, p = .036*
Harris Wins Popular Vote	3.24	.041	.03	diff = -0.35, p = .786	diff = 0.92, p = .173	diff = 1.26, p = .041*
Harris Wins State Vote	21.25	.000	.17	diff = 3.50, p = .000***	diff = 2.62, p = .000***	diff = -0.89, p = .249
Life Satisfaction (Time 1)	0.2	.819	.00	Diff = 0.15, p = 829	Diff = 0.02, p = .996	Diff = -0.13, p = .866

Measure	F (2, 214)	*p*	ηp^2	Blue v. Red	Swing v. Red	Swing v. Blue
Voting Confidence	.80	.449	.01	diff = -0.10, p = .960	diff = 0.32, p = .619	diff = 0.42, p = .455

Next, we conducted a further series of exploratory independent-samples *t*-tests examining whether all other variables measured at Time 1 differed as a function of voted candidate (voted for Harris v. voted for Trump), reported in Tables 37 and 38. Significant differences were found for all six expectation-related measures. Participants who voted for Harris expected Trump to be less likely to win the popular vote, the state vote and the election overall relative to those who voted for Trump (all *p*s < .001). Conversely, Harris voters expected Harris to be more likely to win the election, the popular vote and the state vote compared with Trump voters (all *p*s < .001). Life satisfaction (Time 1), political efficacy, and voting confidence did not differ significantly between Harris and Trump voters (all *p*s > .06).

Overall, expectations appeared to be more strongly differentiated by voted candidate (large effects, *d*s = 0.85-1.76, across all six measures) than by state category (smaller effects, ηp^2 = .03-.19, across four of six measures). However, since no factorial ANOVA was run to test both factors together, this comparison should be treated as suggestive rather than conclusive.

Table 37.

Descriptive statistics for all variables measured at Time 1 for Voted for Harris v. Voted for Trump groups

Variables	Overall mean (SD)	Voted for Harris	Voted for Trump
Trump Wins Popular Vote	-0.00 (3.21)	-1.78 (2.63)	2.23 (2.38)
Trump Wins State Vote	0.62 (3.49)	-0.60 (3.49)	2.15 (2.83)
Trump Wins Election	0.62 (3.03)	-1.00 (2.65)	2.67 (2.09)
Harris Wins Election	0.89 (3.13)	2.71 (2.13)	-1.40 (2.65)
Harris Wins Popular Vote	1.30 (3.15)	3.15 (2.04)	-1.03 (2.72)
Harris Wins State Vote	0.32 (3.63)	1.59 (3.43)	-1.28 (3.22)
Life Satisfaction (Time 1)	4.58 (1.47)	4.42 (1.56)	4.79 (1.33)
Political Efficacy	2.88 (2.06)	2.94 (1.98)	2.79 (2.16)
Voting Confidence	9.94 (2.09)	9.90 (2.22)	10.00 (1.92)

Table 38.

T-test results comparing Voted candidate Group Differences on all measured variables in Time 1

Measure	Independent t-test (Voted Harris v. Trump)
Trump Wins Popular Vote	t(215) = -11.64, p = .000***, d = -1.59
Trump Wins State Vote	t(215) = -6.23, p = .000***, d = -0.85
Trump Wins Election	t(215) = -11.09, p = .000***, d = -1.51
Harris Wins Election	t(215) = 12.66, p = .000***, d = 1.73
Harris Wins Popular Vote	t(215) = 12.93, p = .000***, d = 1.76
Harris Wins State Vote	t(215) = 6.28, p = .000***, d = 0.86
Life Satisfaction (Time 1)	t(215) = -1.88, p = .061, d = -0.26
Political Efficacy	t(215) = 0.54, p = .589, d = 0.07
Voting Confidence	t(215) = -0.35, p = .729, -d = 0.05

To better understand the individual differences underlying perceptions of political efficacy, we conducted linear regression analysis with the following pre-election predictors: State groups (Red, Blue, Swing), Party Identification (None, Democratic, Republican), Intended Voting Candidate (Trump, Harris), Voting Confidence, Satisfaction with Life Time 1, Temporal Binding Time 1, Age, Trump Likelihood Winning Election Prediction, Harris Likelihood Winning Election Prediction to examine which variables predicted significantly participants' explicit self-report of political efficacy.

State groups, Party Identification (None, Republican, Democratic) and Intended Voting Candidate (Harris, Trump) as categorical variables were dummy-coded before analysis. For State group, Red states was coded as the reference category yielding 2 dummy variables. For Party Identification, Republican was coded as the reference category, also yielding 2 dummy variables. For Intended Voting Candidate, Harris was coded 0 and Trump was coded 1.

The assumptions of regression for the model were checked. Multicollinearity was not problematic, as the correlations between predictor variables were all less than .80 (see Table 39) and VIF was less than 10 (VIF $\leq$ 4.09). The histogram of residuals showed an approximately normal distribution, which suggests that the assumption was met (Appendix J). Homoscedasticity was likely met because the scatterplot showed a similar variance in residuals across predicted values (Appendix J). There were no multivariate outliers. The highest Cook's Distance value was .071.

Our linear regression model was significant $F(11, 205) = 4.08$, $p < .001$, accounting for 17.94% of the variance observed in the rating of political efficacy. In the model, level of satisfaction with life measured before the election outcome was the most robust predictor of political efficacy that the more one was satisfied with their life, the greater their political efficacy $\beta = .26$, $t = 4.01$, $p < .001$. Similarly, voting confidence was significantly associated with explicit political agency that the more individuals stated feeling confident that they would vote in the upcoming election, the more political agency they also reported $\beta = .16$, $t = 2.43$, $p = .016$. Age was also a significant predictor of political efficacy that older age was predictive of greater perceived political efficacy $\beta = .15$, $t = 2.23$, $p = .027$. Other predictors were not significant (See Appendix G). Contrary to our predictions, after controlling for other variables, state groups did not significantly predict the magnitude of perceived political efficacy.

Table 39.
Correlation coefficients between variables in the regression model with political efficacy as outcome variable of Experiment 8 (N = 217)

	Intended Voting Candidate (Harris v. Trump)	State group (Red v. Blue)	State group (Red v. Swing)	Party Identification (Republican v. Democratic)	Party Identification (Republican v. None)	Political Efficacy	Temporal Binding Time 1	Life Satisfaction Time 1	Voting confidence	Age	Trump Wins Election	Harris Wins Election
Intended Voting Candidate (Harris v. Trump)	.	.075	-.125	-.008	-.719***	-.061	.125	.095	.008	-.123	.640***	-.757***
State group (Red v. Blue)		.	-.481***	-.016	.024	-.132	.036	.076	-.088	-.083	.076	-.124
State group (Red v. Swing)			.	.066	.052	.173*	-.098	-.019	.144*	.148*	-.163*	.207**
Party Identification (Republican v. Democratic)				.	-.464***	-.045	.018	-.071	-.061	-.012	-.018	-.009
Party Identification (Republican v. None)					.	.040	-.160*	-.059	.024	.073	-.505***	.595***
Political Efficacy						.	-.098	.264***	.201**	.213**	-.121	.117
Temporal Binding Time 1							.	-.022	.080	-.028	.113	-.105
Life Satisfaction Time 1								.	.045	.051	.109	-.050
Voting confidence									.	.109	-.034	.024
Age										.	-.165*	.133
Trump Wins Election											.	-.690***
Harris Wins Election												.

Note. As state group is a three-level categorical variable, it was dummy-coded (with Red states as the reference category) for inclusion in the correlation matrix. Likewise, party affiliation as a three-level categorical variable was dummy-coded (with Republican as the reference category) * $p < .05$, ** $p < .01$, ***$p < .001$

Post-election Explicit political agency

Perceived Influence on Presidential Winner

A between-subject ANOVA with 3 between-subject group (Red, Blue, Swing states) × 2-level between-subject factor (Voted Candidate: Harris v. Trump) on the level of Perceived Influence on Presidential Winner was conducted. Assumption checks were assessed that inspection of histograms indicated that the residuals were approximately normally distributed. Levene's test indicated homogeneity of variance across groups (p = .260). No extreme outliers were identified. Results showed that there was no significant main effect of State group $F(2, 211) = .09$, $p = .912$, $\eta p^2 = .00$. There was also no significant interaction effect of State group × Voted candidate $F(2, 211) = 1.89$, $p = .154$, $\eta p^2 = .02$ (See Table 40 for descriptive statistics).

There was however a significant main effect of voted candidate $F(1, 211) = 34.91$, $p < .001$, $\eta p^2 = .14$ that Trump voters reported significantly greater level of perceived influence on presidential winner than Harris voters (Table 40).

Table 40.

Descriptive statistics for Perceived Influence on Presidential Winner split by State group and voted candidate

State group	Voted For	Mean	SD	N
Red	Harris	2.68	2.78	37
	Trump	6.54	3.47	35
	Total	4.56	3.67	72
Blue	Harris	3.19	3.22	36
	Trump	6.13	3.88	32
	Total	4.57	3.82	68
Swing	Harris	3.60	3.79	48
	Trump	5.24	3.48	29
	Total	4.22	3.74	77
Total	Harris	3.20	3.33	121
	Trump	6.01	3.62	96
	Total	4.44	3.73	217

*Perceived Influence on **state-level result***

A between-subject ANOVA with 3 between-subject group (Red, Blue, Swing states) × 2-level between-subject factor (Voted Candidate: Harris v. Trump) on the level of Perceived Influence on state-level result was conducted. Assumption checks were assessed that inspection of histograms indicated that the residuals were approximately normally

distributed. Levene's test indicated homogeneity of variance across groups (p = .074). No extreme outliers were identified. Results showed that there was no significant main effect of State group $F(2, 211) = 1.15$, $p = .318$, $\eta p^2 = .01$. There was also no significant interaction effect of State group × Voted candidate $F(2, 211) = 1.04$, $p = .355$, $\eta p^2 = .01$ (See Table 41 for descriptive statistics).

There was however a significant main effect of voted candidate $F(1, 211) = 30.86$, $p < .001$, $\eta p^2 = .13$ that Trump voters reported significantly greater level of perceived influence on state-level result than Harris voters (Table 41).

Thus, altogether these results suggested that participants who voted for Trump reported greater level of explicit political agency for both national level and state level presidential result than participants who voted for Harris.

Table 41.

Descriptive statistics for Perceived Influence on state-level result split by State group and voted candidate

State group	Voted For	Mean	SD	N
Red	Harris	2.16	2.63	37
	Trump	5.74	3.58	35
	Total	3.90	3.59	72
Blue	Harris	3.64	2.90	36
	Trump	5.88	4.08	32
	Total	4.69	3.65	68
Swing	Harris	3.63	3.71	48
	Trump	5.69	3.59	29
	Total	4.40	3.77	77
Total	Harris	3.18	3.22	121
	Trump	5.77	3.71	96
	Total	4.33	3.67	217

Other Time 2 variables

A series of exploratory one-way ANOVAs examined whether all other variables measured in Time 2 differed as a function of state category (Blue, Red, Swing), reported in Table 43 (See table 42 for descriptive statistics). No significant results were found suggesting that the three groups did not differ in any of the variables measured at Time 2 (See Table 43).

Table 42

Descriptive statistics for all variables measured at Time 2 for state groups

Variables	Overall mean (SD)	Red	Blue	Swing
Life Satisfaction (Time 2)	4.59 (1.51)	4.54 (1.50)	4.65 (1.58)	4.59 (1.48)
Impact of No-Vote on Presidential Outcome	2.93 (3.32)	2.62 (3.11)	3.43 (3.65)	2.78 (3.18)
Impact of No-Vote on State Outcome	8.03 (3.19)	7.79 (3.52)	8.19 (3.27)	8.12 (2.81)
Perceived Influence on State Outcome	4.33 (3.67)	3.90 (3.59)	4.69 (3.65)	4.40 (3.77)
Perceived influence on Presidential Winner	4.44 (3.73)	4.56 (3.67)	4.57 (3.82)	4.22 (3.74)

Table 43

ANOVA Results Comparing State Group Differences on all measured variables in Time 2

Measure	F (2, 214)	p	ηp^2	Blue v. Red	Swing v. Red	Swing v. Blue
Life Satisfaction (Time 2)	0.09	0.915	0.00	diff = 0.11, p = .907	diff = 0.05, p = .980	diff = -0.06, p = .969
Impact of No-Vote on Presidential Outcome	1.15	0.319	0.01	diff = 0.80, p = .327	diff = 0.15, p = .957	diff = -0.65, p = .470
Impact of No-Vote on State Outcome	0.31	0.731	0.00	diff = 0.40, p = .742	diff = 0.33, p = .810	diff = -0.07, p = .989
Perceived Influence on State Outcome	0.83	0.438	0.01	diff = 0.79, p = .415	diff = 0.50, p = .685	diff = -0.29, p = .885
Perceived influence on Presidential Winner	0.21	0.811	0.00	diff = 0.02, p=1.000	diff = -0.33, p = .849	diff = -0.35, p = .838

Next, we conducted a further series of exploratory independent-samples *t*-tests examining whether all other variables measured at Time 2 differed as a function of voted candidate (voted for Harris v. voted for Trump), reported in Tables 44 and 45. Significant differences were found for all Time 2 measures. Participants who had voted for Trump reported significantly greater level of Impact of No-Vote on Presidential Outcome; Impact of No-Vote on State Outcome and Life Satisfaction (Time 2) than those who had voted for

Harris (*ps* ≤ .041). Thus, post-election political agency measuring perceived personal influence on the actual election outcome as well as life satisfaction post-election were also more strongly differentiated by voted candidate (medium to large effects, *d*s = .28-.81) than by state category (small effects, ηp^2 = .00-.01). However, again, since no factorial ANOVA was run to test both factors together, this comparison should be treated as suggestive rather than conclusive.

Table 44.

Descriptive statistics for all variables measured at Time 2 for Voted for Harris v. Voted for Trump groups

Variables	Overall mean (SD)	Voted for Harris	Voted for Trump
Impact of No-Vote on Presidential Outcome	2.93 (3.32)	2.00 (2.83)	4.10 (3.51)
Impact of No-Vote on State Outcome	8.03 (3.19)	8.42 (3.15)	7.54 (3.20)
Perceived Influence on State Outcome	4.33 (3.67)	3.18 (3.22)	5.77 (3.71)
Perceived influence on Presidential Winner	4.44 (3.73)	3.20 (3.33)	6.01 (3.62)
Life Satisfaction (Time 2)	4.59 (1.51)	4.40 (1.61)	4.83 (1.35)

Table 45.

T-test results comparing Voted candidate Group Differences on all measured variables in Time 2

Measure	Independent t-test (Voted Harris v. Trump)
Impact of No-Vote on Presidential Outcome	t(215) = -4.88, p = .000***, d = -0.67
Impact of No-Vote on State Outcome	t(215) = 2.03, p = .044*, d = 0.28
Perceived Influence on State Outcome	t(215) = -5.50, p = .000***, d = -0.75
Perceived influence on Presidential Winner	t(215) = -5.94, p = .000***, d = -0.81
Life Satisfaction (Time 2)	t(215) = -2.06, p = .041*, d = -0.28

Like before, we also conducted regression analysis wherein we entered variables that were measured in Time 2 i.e.. post-election to assess which factors were most predictive of the perceived personal influence on election outcome. These time 2 variables were Voted Candidate (Harris, Trump); Life Satisfaction Post-election, Impact of No Vote on State Outcome; Impact of No Vote on Presidential Outcome; Perceived Influence on State

Outcome and Temporal Binding Time 2. State group, party identification and age were also entered as predictors.

State groups, Party Identification (None, Republican, Democratic) and Voted Candidate (Harris, Trump) as categorical variables were dummy-coded before analysis. For State group, Red states was coded as the reference category yielding 2 dummy variables. For Party Identification, Republican was coded as the reference category, also yielding 2 dummy variables. For Voted Candidate, Harris was coded 0 and Trump was coded 1. The assumptions of regression for the model were checked. Multicollinearity was not problematic, as the correlations between predictor variables were all less than .80 (see Table 46) and VIF was less than 10 (VIF ≤ 2.94). The histogram of residuals showed a slight positive skew, with a small number of larger positive residuals extending the right tail (Appendix K). Homoscedasticity was likely met because the scatterplot showed a similar variance in residuals across predicted values (Appendix K). There were no multivariate outliers. The highest Cook's Distance value was .179.

This model was statistically significant $F(11, 199) = 40.78$, $p < .001$ as it accounted for 69.27% of the variance observed in the subjective perception of personal influence on presidential outcome. Greater life satisfaction level measured post-election was significantly predictive of greater Perceived Influence on Presidential Winner $\beta = .11$, $t = 2.51$, $p = .013$.

The Impact of No Vote on Presidential Outcome ("*How likely do you think it is that someone else would have been elected President if you had not voted?*") was a significant predictor of Perceived Influence on Presidential Winner that participants who perceived their votes to be important were more likely to view that they had personally influenced the presidential outcome $\beta = .11$, $t = 2.19$, $p = .030$. Lastly, one's Perceived Influence on State Outcome ("*How much do you feel that you personally influenced the outcome of the Presidential Election in your state?*") was the most significantly predictive of Perceived Influence on Presidential Winner $\beta = .71$, $t = 14.16$, $p < .001$. No other predictors were significant (See also Appendix G for full regression results). Contrary to our predictions, after controlling for other variables, state groups did not significantly predict the magnitude of perceived Influence on Presidential Winner. Interestingly whilst the t-test results above did show that participants who voted for Trump did show greater level of perceived Influence on Presidential Winner than participants who had voted for Harris (with large effect size d = -.81), here the effect of voted candidate was not a significant predictor when considered alongside other post-election psychological variables, especially the perceived influence on state outcome.

Table 46.
Correlation coefficients between variables in the regression model with perceived influence on Presidential Winner as outcome variable of Experiment 8 (N = 211)

	Voted for (Harris v. Trump)	State group (Red v. Blue)	State group (Red v. Swing)	Party Identification (Republican v. Democratic)	Party Identification (Republican v. None)	Perceived influence on Presidential Winner	Temporal Binding T2	Life satisfaction T2	Impact of No Vote on Presidential Outcome	Impact of No Vote on State Outcome	Perceived Influence on State Outcome	Age
Voted for (Harris v. Trump)	.	.066	-.114	.011	-.695***	.374***	.035	.146*	.321***	-.158*	.347***	-.139*
State group (Red v. Blue)		.	-.501	.003	.010	.040	-.018	.046	.113	.051	.082	-.125
State group (Red v. Swing)			.	.064	.060	-.046	-.049	-.007	-.041	.020	.023	.182**
Party Identification (Republican v. Democratic)				.	-.462***	-.021	-.012	-.173*	.074	.077	-.035	.014
Party Identification (Republican v. None)					.	-.274***	-.070	-.063	-.154*	.092	-.249***	.061
Perceived influence on Presidential Winner						.	.054	.361***	.498***	-.297***	.810***	-.129
Temporal Binding T2							.	.060	-.085	.043	.037	.048
Life satisfaction T2								.	.231**	-.186**	.317***	.030
Impact of No Vote on Presidential Outcome									.	-.326***	.500***	-.083
Impact of No Vote on State Outcome										.	-.340***	.081
Perceived Influence on State Outcome											.	-.144*
Age												.

Note. As state group is a three-level categorical variable, it was dummy-coded (with Red states as the reference category) for inclusion in the correlation matrix. Likewise, party affiliation as a three-level categorical variable was dummy-coded (with Republican as the reference category) * $p < .05$, ** $p < .01$, ***$p < .001$

Discussion

The primary interest of this chapter was to understand how the election outcome could influence three main constructs: a) temporal binding effect, a proxy for the sense of agency; b) pre-election political efficacy and c) explicit political agency i.e., perceived personal influence on presidential outcome; based on which state group (Red, Blue and Swing) participants resided in and which candidate they voted for. Contrary to our predictions, swing states did not have stronger temporal binding effect than other state groups, neither before nor after the election and that voted candidate did not moderate the pre- or post-election magnitude of binding in Swing states. In fact, temporal binding effect was not moderated by either state groups or voted candidate at all. Similarly, regarding the sense of political efficacy measured pre-election, we also found no significant effects of state groups or voted candidates and their interactions. However, with regards to the explicit political agency measured post-election i.e., how much participants felt like they had influenced the election outcome, our findings suggested that Trump voters reported significantly greater level of perceived influence on presidential winner, both at the state-level and national-level, than Harris voters. Contrary to our expectations, state groups did not influence explicit political agency. Considering the rich details and different variables included in the study, we conducted regression models, and these allowed us to test our variables of interest in a holistic approach to understand what best predicted the individual differences in implicit sense of agency, political efficacy and explicit political agency, which would be further discussed.

Temporal binding

Unlike our predictions, the temporal binding effect was not modulated by state group or by voted candidate and by extension the political event. Top-down influences such as age, personality traits (narcissism, meditation) and higher-order beliefs (belief in free will) have previously been shown to modulate the implicit sense of agency measured using the temporal binding effect (Aarts & Van Den Bos, 2011; Hascalovitz & Obhi, 2015; Lush, Naish, et al., 2016; Mariano et al., 2024). However, it is possible that while a presidential election carries substantial psychological and social significance (Ruisch & Ferguson, 2022) this significance might have operated at a level that is too distant from the low-level temporal binding effect indexed by our task to exert a measurable top-down effect. A further possibility is that the Time 2 measurement point was too immediate relative to the timescale over which a presidential election's consequences unfold. The effects of a presidential election might not

have been fully realised within days of the result. The personal as well as socio-political consequences of every presidential term often accumulate gradually over that period rather than being concentrated at the moment the outcome is announced (Arnold et al., 2026; Greer et al., 2025). It is thus possible that any top-down influence on implicit sense of agency might require much longer accumulation of lived experience under a given administration rather than emerging from knowledge of the result alone. Future research could test this by adopting a more longitudinal design, measuring temporal binding at multiple points across a presidential term to examine whether implicit sense of agency is shaped by the cumulative experience of an administration's impact rather than by the initial outcome itself.

Pre-election sense of political efficacy

The explicit measure of political agency at Time 1 i.e., pre-election was an indicator of a person's perceived electoral influence and how much their vote mattered in determining the election outcomes. It reflected how much individuals believed their votes counted, voting had consequences and their personal political actions (like voting) could affect real political outcomes, particularly in this election. Residents of Swing states, where even a narrow margin of victory could deliver a significant number of electoral votes to a candidate, did not report significantly greater political efficacy than those residing in Blue or Red states. Additionally, state group alone was not a significant predictor of political efficacy. Rather, in the exploratory analysis, we found that satisfaction with life and voting confidence were the primary significant predictors of political efficacy. In particular, the more one was satisfied with their life, the greater their political efficacy reported. Likewise, voting confidence was significantly associated with explicit political agency that the more confident participants reported that they would vote in the upcoming election, the more political efficacy they also had. These findings were in line with past research on subjective well-being and vote turn-out based on political sense of efficacy (Abrams et al., 2020; Flavin & Keane, 2012; Frey & Stutzer, 2000). Finally, age was also a significant predictor of political efficacy that older age was predictive of greater perceived political efficacy. This is somewhat in line with prior findings that political efficacy tends to be stable or higher among older adults, but it is worth noting that some studies report a curvilinear rather than strictly linear pattern (efficacy rising through middle age and plateauing or slightly declining in very old age) and age interacts with other factors such as employment too (Jennings & Markus, 1988; P. Marx & Nguyen, 2016; Searing et al., 1976). Therefore, since the finding is cross-sectional and exploratory, no

strong claim is made regarding either genuine developmental or generational difference in political efficacy.

Post-election explicit political agency: personal influence in the electoral outcome

With regards to the explicit political agency measured following the aftermath of the 2024 US election, this measure directly pertained to how much one attributed personal agency to the election outcome. In other words, this post-election measure reflected retrospective sense of political agency, the belief that one's actions had tangible political consequences and thus more outcome-specific focus. We found that for participants who voted for Trump had greater level of perceived personal influence over both state-level and national-level electoral results than Harris voters, notably independent of which state groups the participants resided in. These results might suggest that for individuals who had had prior expectations or endorsement of a candidate aligned with the actual election outcome, such congruence might lead to the greater explicit political agency compared with individuals who had voted for the candidate that did not win. It is worth noting that this effect was not moderated by state group as expected, such that swing states residents who voted for Trump might have reported the strongest perceived personal influence. This is consistent with accounts emphasising the role of outcome-expectation congruency in explicit sense of agency that individuals report greater feeling that they have caused the outcome when the outcome matches with their expectation (Ebert & Wegner, 2010; Hughes, 2018). Crucially, here, we have showed that the benefits of election expectation-outcome alignment on explicit sense of agency might extend to even collective, socio-political outcome. A possible explanation for this pattern lied at how the knowledge that one's preferred candidate to win the election could generate a powerful sense of validation and personal efficacy (Hochschild, 2018). This aligns with theories of motivated reasoning and affective forecasting, proposing that individuals derive emotional and cognitive reinforcement from favourable political outcome (Kassab, 2016; N. Schwarz, 1990). Thus, the findings here highlighted that having favourable political outcome is influential on one's explicit political sense of agency.

The regression model further suggested greater life satisfaction level measured post-election was significantly predictive of greater Perceived Influence on Presidential Winner. This is also in line with the wider literature on political psychology such that political partisanship and desirable political outcome have been shown to benefit citizen's mental states and enhance cognitive mechanisms (Leeper & Slothuus, 2014; Pierce et al., 2016; Vail et al., 2023). The Impact of No Vote on Presidential Outcome ("*How likely do you think it is*

that someone else would have been elected President if you had not voted?") was a significant predictor of Perceived Influence on Presidential Winner. These findings suggest that participants' sense of having personally influenced the presidential outcome was also explicable by the counterfactual reasoning about the necessity of their vote. This is consistent with counterfactual accounts of causal attribution more broadly that people tend to judge their own causal contribution to an outcome as a function of how they could imagine the outcome having gone otherwise had they acted differently (Kahneman & Miller, 1986). Lastly, one's Perceived Influence on State Outcome ("*How much do you feel that you personally influenced the outcome of the Presidential Election in your state?*") was the most significantly predictive of Perceived Influence on Presidential Winner. This is in line with the electoral college logic outlined earlier in this chapter. Because the presidential outcome is mechanically determined by the accumulation of state-level results rather than the national popular vote, a participant's belief that they influenced their state's result is arguably the more proximal and mechanistically plausible route by which an individual vote could be seen to influence the presidential outcome. Past research has indeed highlighted the benefits and importance of partaking in politics at the local level (Beaumont, 2011; McDonnell, 2020). This suggests that participants' explicit political agency judgments might have been at least partly sensitive to the actual structure of the electoral system because state-level outcomes genuinely are the mechanism through which individual votes aggregate to the presidential result.

Limitations

Although both the Time 1 and Time 2 measures related broadly to beliefs about the impact of one's vote, they were not designed as (and should not be treated as) repeated measurements of the same construct. Time 1 assessed a generalised, prospective belief about vote impact, asked before the outcome was known. Meanwhile Time 2 assessed a specific, retrospective causal attribution about a known outcome and might therefore be susceptible to hindsight bias and outcome-dependent motivated reasoning in a way Time 1 could not be. Time 2 measure is thus accordingly better characterised as a domain-specific instance of retrospective agency judgement related to sense of agency but applied at the level of a collective political outcome rather than political efficacy in the Time 1 sense. This prevented any straightforward test of whether beliefs about vote impact changed from before to after the election. Future work using matched pre- and post-election items on a common scale such as the NES that measures internal and external efficacy would be needed to assess genuine change in efficacy-related beliefs across the electoral cycle.

Furthermore, Time 2 opened on 6th November 2024, after the national presidential result (Trump's win) had been announced, and remained open for 72 hours. This window was chosen partly to accommodate the fact that individual state results are not all called simultaneously: while the national outcome was known to all participants by the time Time 2 opened, some state-level results might not have been finalised until one or more days after election day. Keeping the survey open for 72 hours therefore allowed participants across states to respond after both the national result and for the large majority of states, their own state's result was known. It remains possible that a small number of participants (especially those in the slowest-called states) might have responded before their own state's result was confirmed which is a limitation. As having mentioned future research should take measures at further time points too to help counter this issue and also to establish longitudinal effects.

Chapter Conclusion

In this chapter, the aim was to assess whether being in Swing states compared with Red and Blue states as well as voting behaviour in the 2024 US election would moderate the a) temporal binding effect as the implicit marker of sense of agency; b) political efficacy in this particular election and c) explicit political agency i.e., feeling personally influencing the election result. Contrary to our predictions, participants in Swing states showed neither greater temporal binding both at pre- and post-election nor greater political efficacy compared with Red and Blue states. Voted candidate also did not moderate the temporal binding effect or political efficacy. However, we did find that Trump voters reported significantly greater level of perceived personal influence on the electoral outcome than Harris voters. Thus, explicit political sense of agency was greater for individuals whose candidate they had voted for actually won the election. Our findings showed that the effect of expectation-outcome alignment on enhancing the explicit sense of agency might extend beyond just personal level to even collective, socio-political outcome in a presidential election.

CHAPTER 7

General discussion

This thesis set out to advance our understanding of the temporal binding effect and its role as an implicit marker of the sense of agency. In particular, it aims at adding to the extant literature surrounding the antecedents of the temporal binding effect. It has been demonstrated that the temporal binding effect could be contextual-sensitive and how the magnitude of the effect could differ from person to person, reflecting difference in the implicit sense of agency. Factors at the three levels of analysis were investigated, namely personal, interpersonal and socio-political.

At the personal level, sensorimotor disruption (state), positive mood (state) and life satisfaction (trait) were the primary factors that were assessed. At the interpersonal factor, the spotlight was on the social contexts wherein individuals were coerced to inflict physical pain on others, and whether identifying with the coercer (the experimenter) could moderate the magnitude of binding. Lastly, at the broadest spectrum of individual's level of control, the temporal binding as the index of implicit agency was measured before and after the outcome of Presidential Election in the US 2024. This empirical investigation was novel as it was the first study of date to map the implicit agency onto the socio-political level of control. The factors on different level of control spectrums that moderate (or not) temporal binding are discussed below (see Table 47).

Key Findings

Sensorimotor disruption only impairs temporal binding if one does the easy task first and when one repeats doing the easy task again and again: The role of challenge and novelty

Chapter 3 focussed on moderating level of sensorimotor control as the state factor and how motor control disruption introduced could influence the subsequentially measured temporal binding effect. Engaging in a goal-directed motor control task with a high degree of perturbance (90-270 angular bias motor control task) in movement trajectory did not simply attenuate the temporal binding effect. Rather, it appears that the order in which the easy v. difficulty versions of the motor task was performed had the most influential impact on the magnitude of binding. For individuals performing the easy straightforward version of the motor control task first then engaging in the disruptive motor task, their action binding effect was significantly lower after performing the high-perturbance task than after the easy version of the motor task. It is suggested that performing the easy task then switching to the difficult version might have disrupted the straightforward action-outcome contingency estimates (the motor system had already established for this task), thus making the estimate of how to

compensate for such perturbation difficult to learn. In contrast, for participants who were 'thrown in the deep end' i.e., performing the challenging 90-270^{o} task first then the easier version later, the magnitude of binding was preserved throughout the experiment. After one already adapting and creating new internal models from learning movement errors, switching back to non-disturbance motor reaching task should be effortless, hence the stability of temporal binding effect over time.

The last experiment of this chapter provided complimentary to the importance of novelty and engaging in challenging but rewarding task: those who performed the challenging motor task had temporal binding sustained over time, whilst the control group who repeated the easy full-control task over and over again had the temporal binding effect attenuated as blocks progressed. Such findings could be explained by the phenomenon of experiencing flow which is the pleasurable absorption in task and how flow peaks when task difficulty and skills are in balanced (Vuorre & Metcalfe, 2016). This framework, although this is merely a suggestive/speculative link, might help explain how the Control group's easy task might have led to breakdown in engaged absorption that sustains motivated action, possibly then led to reduced temporal binding effect. By contrast even though the Disrupt group's motor control task was more challenging, the task might have created better flow and it is speculated that such flow helped sustain the temporal binding effect. Similarly, Iso-Ahola (2022)'s theory of conscious-nonconscious processing is also relevant such that introducing variability into routine patterns is important to perform well both demanding and non-demanding behaviours over time. The present findings are broadly consistent with this account such that the Control group's repeated performance of the same easy task without variability or challenge might have undermined the engaged processing that sustains implicit sense of agency. Meanwhile, the Disrupt group's ongoing need to adapt to the challenging 90-270° motor task might have provided the variability and novelty that is necessary for sustained nonconscious engagement (Iso-Ahola, 2022). Altogether, this chapter shows that temporal binding is impaired by sensorimotor disruption only when the easier task is performed initially and subsequently repeated, highlighting the influence of task difficulty and novelty.

Table 47.

Summary of key methods (including Ns), the type of measure used (e.g. binding task), design type (state vs trait vs different trial types) and main results on temporal binding effect across all experiments

	Key methods	Binding task	Design type	Main Results
E1	Mixed-design: Starting with full control (N = 42): Motor control task (Full-control) → TB → Motor control task (90-270°) → TB. Starting with 90-270° (N = 44): Motor control task (90-270°) → TB → Motor control task (Full-control) → TB.	Libet clock	State	Starting with Full-control group had weaker action binding after 90-270° motor block compared with action binding after full-control task. Action binding following each motor task did not differ amongst Starting with 90-270° group. No effects on tone binding/total binding
E2	Between-subject: Control group (N = 41): Baseline TB → [Motor control task (full control) → TB] x 6. Disrupt group (N = 44): Baseline TB → [Motor control task (90-270°) → TB] x 6	Interval Reproduction	State	Control group had TB effect significantly attenuate over time as blocks progressed. Disrupt group showed sustained TB effect across blocks.
E3	Correlational (N = 73): TB + Questionnaires	Interval Reproduction	Trait + State	Significant backward regression model with remaining predictors: life satisfaction (higher life satisfaction scores were predictive of reduced TB effect); positive mood DS scale (higher positive mood on DS scale were predictive of greater TB effect); self-efficacy and fatalistic determinism were not significant predictors of TB but retained in model
E4	Correlational (N = 125): TB + Questionnaires	Interval Reproduction	Trait + State	Linear multiple regression model (enter method) was not significant. Life satisfaction was a significant predictor (higher life satisfaction scores were predictive of reduced TB effect). Positive mood DS scale, self-efficacy and fatalistic determinism were not significant predictors of TB.

	Key methods	Binding task	Design type	Main Results
E5	Between-subject: Control group (N = 42) v. Journal group (N = 45): Questionnaires T1 → 7-day journaling → Questionnaires T2 + TB	Libet clock	Trait	Journal group had weaker tone binding than Control group. No effects on action binding/total binding
E6	Within-subject (N = 34): Watching funny movie clip → TB → after 7 days → watching neutral video → TB (video condition counterbalanced)	Libet clock	State	No difference in TB after watching positive video clip and TB after watching neutral video.
E7	Mixed-design: Low identification with experimenter (N = 31) v. High identification with experimenter (N = 31): TB Prosocially Disobeyed – Did not shock v. TB Obeyed – Gave shock v. TB Obeyed – Did not shock	Interval estimation	Differential trial types	TB for Prosocially Disobeyed – Did not shock outcome was significantly greater than TB Obeyed – Gave shock outcome. TB for Obeyed – Did not shock outcome was significantly greater than TB Obeyed – Gave shock outcome. No difference between TB for Prosocially Disobeyed – Did not shock outcome and TB for Obeyed – Did not shock outcome. No effect of experimenter identification on TB
E8	Mixed-design: Red states + voted for Harris (N = 37) v. Red + voted Trump (N = 35) v. Blue + voted Harris (N = 36) v. Blue + voted Trump (N = 32) v. Swing + voted Harris (N = 48) v. Swing + voted Trump (N = 29) TB Pre-election outcome → TB Post-election outcome	Interval Reproduction	Trait	No effects of state group or voted Candidate on TB

Note. E = Experiment; TB = Temporal binding

Type of journaling with putatively different locus of control orientation might modulate temporal binding effect

In Chapter 4, the first two experiments 3 and 4 show an opposite pattern of the relationships between temporal binding effect with life satisfaction and positive mood, respectively, based on the regression results illustrating that higher life satisfaction was predictive of lower temporal binding effect whilst greater positive mood was predictive of greater temporal binding magnitude. Experiment 5 and 6 aimed to formally establish the causal relationship between life satisfaction and temporal binding (Experiment 5) and between positive mood and temporal binding (Experiment 6). In Experiment 5, the primary aim was to assess whether an increase in life satisfaction would lead to weaker temporal binding by directly manipulating life satisfaction through gratitude journaling over one week. Whilst the intervention did not produce differential changes in life satisfaction between the Control and Journal groups such that both groups showed significant increases in life satisfaction following the intervention, we found that participants in the Journal group showed significantly weaker tone binding compared to the Control group.

A potentially putative explanation for the result is that the two journaling conditions differ in their locus of control: Control as self-focused v. Gratitude journaling as others-focused which could be the key moderator of temporal binding effect. For participants of the Control group, listing out completed daily activities might have enabled them to retrospectively evaluate the outcomes of their actions, which could likely shift the locus of control inwards i.e., the belief that outcomes are determined by self-caused actions (Dong et al., 2022; Golding, Gregory, Ellis, et al., 2017). In contrast, for participants of the Journal group, writing to others to express gratitude means that the things having been achieved would be a result of others, not the self, hence an external locus of control. (Golding, Gregory, Iles-Caven, et al., 2017). Whilst this explanation is suggestive, it points towards a possibility that such external shift in locus of control could possibly explain for the weaker temporal binding effect for those in the Journal group.

Regarding positive mood as the state factor, increase in positive mood did not increase temporal binding effect as we expected and the primary reason for this null effect is likely due to the psychometric measure of ‘mood’ in the first Experiment 3 and 4. That is the allegedly positive mood items from the first two experiments of this chapter were adapted from the European Social Survey Round 3 by Huppert et al. (2009) who adopted the 8 items from the CES-D measuring Depressive Symptoms (DS) and the extra items that might capture affects, thus they were not appropriately capturing ‘positive mood’ but they were

measuring depression or conflating affect, emotions and mood. As a result, it is not surprising that Experiment 6's manipulation of the types of videos being shown did not moderate temporal binding, given that its rationale was based on the problematic/putative link between 'positive mood' measured using DS items and temporal binding in Experiment 3 and 4.

Prosocial disobedience gives rise to stronger temporal binding effect compared to obedience to inflict physical harm

Moving beyond the individual level, the thesis extends temporal binding research to the interpersonal context. The factor chosen at this level of analysis, namely being coerced to harm others, albeit being rather a rarely encountered social situation, has however been one of the primary reasons underlying historical contexts of wars and genocide (Caspar, 2024). Thus, looking at coercion to inflict harm on others, we could understand how the sense of agency unfolds in this extreme context, providing insights into how the subjective experience of agency alters when being coerced to perform immoral acts. Chapter 5 showed that when obeying to harm others, magnitude of the binding effect was significantly reduced compared to obeying to not send shocks. This is in line with past research (Caspar et al., 2016). Being coerced and especially when the outcome is socially harmful to others might modify the subjective experience of being the author of such action, as the perceived temporal association between actions and the consequences attenuate. Consequentially, actions appear to reflect similar properties as if they were passive movements.

However, focussing on specifically whether prosocial disobedience would increase the sense of agency, this has not been shown in past findings. Here, we found that prosocial disobedience led to greater sense of agency compared to obedience to harm others such that when one deliberately chooses to defy immoral orders, this results in an increased implicit sense of agency. Thus, the act of prosocial disobedience may reinstate the subjective experience of authorship and control over one's actions.

Temporal binding effect was not moderated by voting behaviour outcome or state group categories in the US 2024 Presidential election.

The last empirical chapter of the thesis extends beyond personal and interpersonal factors to the socio-political level, aiming to understand how the sense of agency could be embedded within such broad socio-cultural contexts. In this chapter, we aimed to investigate whether participants residing in Swing states compared with others residing in Red and Blue states as well as the presidential candidate they voted for in the 2024 US election would moderate the a) temporal binding effect as the implicit marker of sense of agency; b) political efficacy in this particular election and c) explicit political agency i.e., feeling personally

influencing the election result. Contrary to our predictions, the results suggested that pre-election temporal binding , post-election binding as well as political efficacy did not differ amongst state groups. Voted candidate (i.e., voted for Harris v. voted for Trump) also did not significantly modulate the temporal binding effect or political efficacy.

Nevertheless, results suggested that participants who had voted for Trump reported significantly greater explicit political agency post-election i.e., feeling that they had personally influenced who had been elected President, compared with participants who had voted for Harris. In other words, individuals whose voted candidate ultimately won the election experienced a stronger explicit sense of political agency. These findings suggest that the effect of expectation-outcome alignment on enhancing the explicit sense of agency might extend beyond personal contexts to collective socio-political events, such as a presidential election. Specifically, when individuals having voted for the candidate who eventually won the election, this congruence appeared to be associated with greater explicit political agency compared with those whose voted for candidate did not win.

Contributions

As having reviewed in Chapter 1 and 2, theoretically, this thesis focussed on the external + phenomenal character sense of agency (i.e., feeling of controlling external events) as defined by Grünbaum & Christensen (2020) and the thesis adopted the theoretical rationale of the hierarchy framework of sense of agency (See Figure 2) which highlights how top-down influences could exert impact on sensorimotor sense of agency over otherwise unrelated action-effect. In order to do so, like the studies reviewed in Chapter 2, we aimed to manipulate the antecedents of temporal binding effect i.e, the factors that precede the binding task whilst keeping the action-effect pairing intact (apart from Experiment 7, chapter 5 as having acknowledged). Methodologically, we used both explicit measure (‘how control do you feel’) but especially implicit measure, primarily the temporal binding effect (measured using interval reproduction, interval estimation and Libet clock paradigms) while acknowledging from the outset that these measures might not correlate within individuals. Taken together, the findings across Chapters 3 to 6 offer preliminary but not conclusive support for this framework while also clarifying its boundaries.

Chapter 3’s finding that sensorimotor disruption attenuated the subsequently measured temporal binding effect, over an action and effect otherwise unrelated to the disruption itself, only when a difficult motor control task followed an easy one and only with repeated performance of the easy task, is notable because it cannot be explained by bottom-

up input alone. The order- and novelty-dependence of these effects suggest that even processing at this seemingly low level is actually shaped by top-down influence rather than arising purely bottom-up: whether a given sensorimotor disruption impaired subsequentially measured temporal binding indeed depended on prior task history and putative level of engagement. Both of these are more naturally described as higher-level contextual or motivational factors feeding down into proximal-level processing. This is broadly consistent with the hierarchical and bidirectional structure proposed in Figure 2 in which higher levels shape lower-level processing that gives rise to the feeling of agency (as Synofzik et al. (2013) also suggested, see Figure 1). However it is worth noting that the mechanism proposed here (flow/engagement) is speculative and was not directly measured. Likewise, Chapter 4's finding that journaling type moderated the temporal binding effect and the putative explanation that such results might have reflected a shift in locus of control induced by the journaling task could be mapped onto the metacognitive and global-belief layers proposed above the hierarchy of the core sense of agency over specific action itself (Figure 2) (Seow et al., 2021; Stephan et al., 2016). It is also broadly consistent with Synofzik et al. (2013)'s proposal that cognitive-level factors such as beliefs can feed into the sensorimotor level giving rise to the feeling of agency. However, the explanation of journaling types as differentiating internal v. external locus of control is purely suggestive and was not directly tested so should be interpreted with caution. In Experiment 6, positive mood was manipulated directly and the manipulation check using the BMIS confirmed that positive mood did increase following the positive video compared with after watching the neutral video yet temporal binding was not modulated. This suggests that at least for this specific manipulation and BMIS measure, an increase in positive mood alone might not be sufficient to exert top-down influence on temporal binding.

Chapter 5's findings could also be relevantly linked to the social context/social hierarchy element in the schematics outlined in Figure 2 wherein social roles can influence the sense of agency (K. A. Schwarz et al., 2023). Here we add to the social-hierarchy element by showing that the interpersonal context involving morality and obedience could be nuanced such that the assigned social role of being the 'agent' (receiving orders from the 'experimenter' and making decisions that have actual physical consequences on the 'victim') does not automatically increase or decrease the sense of agency. Rather it was likely the particular decision to obey or not to obey within the specific socially moral context that could influence the feeling of agency putatively measured via the temporal binding effect. Chapter 6 shows that the socio-political factor might not influence the feeling of agency at the

sensorimotor level as temporal binding effect did not differ by state category or voted candidate, whereas explicit political agency was significantly greater amongst participants whose preferred candidate won. This dissociation is consistent with the distinction between implicit phenomenal feeling of agency and explicit judgment-based agency (Synofzik et al., 2008). Here it could be suggested that at the more collective and socio-political level, top-down influences might potentially register in explicit judgment without correspondingly feeding down to the sensorimotor-level implicit measure.

Considered together, some of the thesis findings offer correlational and non-definitive support for the hierarchical account in which top-down factors can modulate the implicit sense of agency without any need to manipulate the action-effect pairing itself (again, apart from chapter 5), consistent with the approach adopted throughout the narrative review in chapter 2. In contrast, null effects (such as of positive mood or election outcome) on temporal binding effect suggest that not all factors are guaranteed to register top-down influence on implicit measures. As discussed in Chapter 2 and revisited below, these theoretical contributions should also be read alongside the reliability and normative-reference limitations of temporal binding as a measure. Furthermore, each of the empirical chapters had inherent related methodological issues which constrain how strongly any single finding here can be taken as evidence that the factors assessed would reliably influence the sense of agency.

Limitations

Limitations specific to each experiment and chapter have been addressed in their respective discussion sections. Here we focus instead on methodological limitations that cut across chapters and have direct influence on the reliability and measurement concerns raised at the end of Chapter 2.

In Experiment 1 (Chapter 3) and Experiment 5 (Chapter 4) temporal binding was not measured prior to the key experimental manipulation which means that any between-group differences observed post-manipulation cannot be straightforwardly and reliably attributed to the manipulation itself rather than to pre-existing differences between groups. Such limitation feeds into the reliability concerns as having discussed in Chapter 2, with given limited existing evidence of the temporal binding effect's test-retest reliability especially of each paradigm (interval reproduction/estimation/Libet clock). Crucially this is directly linked to the reliability paradox raised by Hedge et al. (2020) who show that classic psychological tasks capable of producing robust and replicable group-level effects could nevertheless show poor test-retest reliability when used to index individual differences. This is due to how the

properties that make a task reliable at the group level (a strong and consistent effect present in almost all participants) work against the between-subject variability that individual differences research depends on. Thus the reliability paradox raises the possibility that these measures might be similarly poorly suited to indexing stable trait-like individual differences in temporal binding even where the underlying group-level effect is well replicated. The absence of a baseline measure in Experiment 1 and 5 makes any between-group comparison even more vulnerable to the absence of test-retest reliability, particularly being confounded by unmeasured individual differences in baseline binding. Thus, when we looked at difference in the magnitude of the temporal binding effect between the Journal v. Control group, considering low test-retest reliability of the Libet clock paradigm (De Azevedo et al., 2025), it is challenging to confidently attribute the significant difference to genuine individual differences between the two groups. It is therefore important to note the value of within-subject baseline measurement as a partial safeguard against the reliability paradox rather than a mere nice to have design. This also calls for better attempt in the field to establish psychometric properties including reliability and validity of the temporal binding paradigms, not just Libet clock but also interval estimation and interval reproduction before using them to index individual differences.

Related, the thesis did not use a single consistent paradigm to measure temporal binding throughout (Table 47). Chapters 3 and 4 used the Libet clock and interval reproduction paradigms interchangeably across experiments, while other chapters relied on a single paradigm. As discussed in Chapter 2, these paradigms are not interchangeable. They appear to capture different things (for example the Libet clock separates action and tone components while interval reproduction/estimation paradigm gives one combined judgement) and they would differ in their psychometric properties. Siebertz and Jansen (2022) and De Azevedo et al. (2025) found no significant correlation between binding scores from the Libet clock and interval estimation/reproduction paradigms along with differing reliability between them. This means findings from one paradigm cannot be assumed to generalise to or be compared directly with findings from another paradigm within this thesis. Some apparent inconsistencies across chapters and across the wider literature reviewed in Chapter 2 might thus reflect this paradigm-level difference rather than a genuine difference in the underlying effect. Future work using one consistent paradigm and alternatively modelling paradigm explicitly as a factor, together with more careful attempt to understand the temporal binding paradigms' psychometric properties as having mentioned above, would help clarify this.

Two further issues concern the validity of self-report measures. Firstly, the mood measure used in Experiments 3 and 4 (Chapter 4) was adapted from a Depressive Symptoms scale and in retrospect likely captured depressive symptoms rather than positive mood specifically. This limits how the findings in those two experiments should be interpreted and they should not be read as evidence supporting the role for positive mood in moderating the magnitude of temporal binding effect. This issue was however specific to Experiments 3 and 4. In Experiment 6, positive mood was manipulated directly using a positive v. neutral video and a manipulation check using the BMIS as a better validated mood measure confirmed that positive mood did increase as intended after participants watching the funny movie clip compared with after watching the neutral video. Temporal binding effect was still not modulated in that experiment so this null result is not subject to the same measurement concern and could be read with more confidence. Secondly, the measures of political efficacy and explicit political agency used in Chapter 7 were developed for this thesis and have not been independently validated. Their reliability and construct validity are therefore unknown and as with the binding measure itself, findings based on these unvalidated measures should be treated with some caution until they are replicated with validated instruments or formally evaluated for their psychometric properties.

Taken together with the reliability paradox and the lack of a normative reference point discussed in Chapter 2, these issues do not operate independently. A measure that already has uncertain individual-level reliability becomes harder to interpret when baseline variability cannot be measured, when findings across chapters did not come from one single consistent method of measuring temporal binding and when some of the constructs it is related to are themselves imprecisely measured or not yet validated. This does not invalidate the overall pattern of findings across the empirical chapters which remain broadly consistent with the theoretical framework outlined above. It does mean that how precisely any single finding can be interpreted should be treated as provisional until replicated using more consistent and better validated methods.

Conclusion

This thesis demonstrates preliminary evidence that temporal binding might be a dynamic index of implicit agency which could be moderated by a range of personal and interpersonal factors (but not socio-political factor). By situating temporal binding effect within real-world contexts, the work emphasises the idea that our sense of agency might be responsive to both internal states and external structures of control. In doing so, it advances

the view that temporal binding can be understood as a psychological mechanism supporting adaptive action, learning and engagement with the world with warrants for better validation of reliability of different temporal binding tasks.

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Appendices

Appendix A

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Appendix A

Table 1. Personal factors - Individual Differences - Long-term conditions/personality/traits

Study	Topic	Method	TB Measure	Summarised Findings
Haggard et al. (2003)	Schizophrenia	Between-subject (8 schizophrenic patients v. 8 matched control)	Libet Clock	Schizophrenic patients have stronger TB (total binding)
Franck et al. (2005)	Schizophrenia	Between-subject (24 patients v. 24 control)	Libet Clock	Schizophrenic patients have stronger TB (total binding)
Graham-Schmidt et al. (2016)	Schizophrenia	Between-subject (15 Patients with Schizophrenia with passivity symptoms . 24 without passivity symptomps v. 43 control)	Interval Estimation	Schizophrenic patients with passivity symptoms show longer perceived intervals for active condition than passive, whilst patients without passivity symptoms do not perceive difference in interval between any conditions (active v. passive).
Saito et al. (2017)	Parkinson's	Between-subject (19 PD patients v. 25 Healthy control)	Libet Clock	PD patients have reduced TB (action binding impaired only, tone binding reserved)
Moore et al. (2010)	Parkinson's	Between-subject (PD patients tested both on and off medication v. Control)	Libet Clock	PD patients on medication have stronger TB (total binding) than when off medication
Ricciardi et al. (2017)	Parkinson's with Impulsive Compulsive Behaviours	Between-subject (19 PD patients with impulsive-compulsive behaviours (ICB) v. 19 PD patients without ICB v. 19 control)	Libet Clock	PD patients with ICB have stronger TB (action binding) than control. No difference in TB between PD patients without ICB and control.
Zapparoli et al. (2017)	Tourette syndrome	Between-subject (25 patients with Tourette syndrome v. 25 control)	Interval Estimation	Patients with Tourette syndrome have reduced TB at 200ms interval compared to control.
Kranick et al. (2013)	Motor Conversion Disorder	Between-subject (20 patients with Motor Conversion Disorder v. 20 control)	Libet Clock	Patients with Motor Conversion Disorder have TB (total binding) reduced.
Wolpe et al. (2014)	Corticobasal Syndrome	Between-subject (10 Corticobasal syndrome patients vs 16 control)	Libet Clock	Corticobasal syndrome patients have stronger TB (action binding) in the most affected hand. No difference in the less affected hand. Greater action binding is related to greater severity of alien limb syndrome and lower apraxia scores.
Sperduti et al. (2014)	Autism Spectrum	Between-subject (15 ASD participants vs 17 control)	Interval Estimation	Adults with Autism Spectrum Disorders have reduced TB.

Appendix A

Table 1. Personal factors - Individual Differences - Long-term conditions/personality/traits

Study	Topic	Method	TB Measure	Summarised Findings
	Disorders (ADD)			
Engel et al. (2022)	Anorexia Nervosa	Between-subject (22 participants with active Anorexia v. 30 recovered anorexia participants v. 29 control)	Libet Clock	No difference in TB between active anorexia participants v. recovered anorexia v. control
Vogel et al. (2024)	Major Depressive Disorder (MDD)	Between-subject (MDD patients v. Control matched age & gender)	Interval Estimation	Major Depressive Disorder patients have reduced TB
Moore (2023)	Borderline Personality Disorder (BPD) trait	Correlational (30 participants, aged 19 -43, assessed with the Borderline Personality Questionnaire (BPQ)	Interval Estimation	Higher BPD trait was associated with weaker temporal binding effect
Moore & Bravin (2015)	Schizotypy	Within-subject (healthy) + correlational	Libet Clock	Individuals with higher schizotypy scores show greater variability in W times (intention awareness)
Hascalovitz & Obhi (2015)	Personality: Narcissistic Personality	Between-subject (high v. middle v. low scores on the narcissistic personality inventory (NPI))	Libet Clock	High and moderate Narcissistic Personality have stronger TB (tone binding) than low Narcissistic Personality
Render & Jansen (2019)	Personality: Narcissistic Personality & Drug Use	Correlational (210 participants, aged 17–34. Mix of students and non-students. Assessed for IQ, personality, substance use, and TB)	Libet Clock	Vulnerable narcissistic individuals have reduced TB (action binding). No difference for Grandiose narcissistic personality. Drug users beyond cannabis have reduced TB (tone binding). Ketamine users have reduced TB.
Render et al. (2024)	Personality: psychopathic traits	Between-subject (Sexual arousal group: erotic scene from Love (2015) V. Calm pleasure: romantic scene from Pride and Prejudice (2005) V. Neutral: pedestrian street scene) x Within-subject (Pre v. Post Libet Clock)	Libet Clock	Individuals high in psychopathy are less affected by sexual arousal in terms of action binding

Appendix A

Table 1. Personal factors - Individual Differences - Long-term conditions/personality/traits

Study	Topic	Method	TB Measure	Summarised Findings
Mariano et al. (2024)	Aging	Between-subject (Young adults mean age 22 v. Older ppts mean age 59.5 - both groups with similar mean education level, no neurological/psychiatric illness)	Interval Estimation	Young adults have strong TB, elderly adults have reduced TB
Cavazzana et al. (2014)	Aging	Between-subject (adults 23yrs v. children 10yrs)	Libet Clock (adapted with letters)	Young adults have stronger TB (action binding) than 10 yr. children
Cavazzana et al. (2017)	Aging	Between-subject (Young adults mean age 23 v. Children mean age 10 v. Elder adults 69.75)	Libet Clock (adapted with letters)	Young adults have stronger TB (total binding) than both 10 yr. children and elderly adults
Arts & Van Den Bos (2011)	Belief in Free Will	Between-subject (High v. Low on free will belief)	Libet Clock	High free-will belief individuals have stronger TB (tone binding) than low free-will belief.
Jo et al. (2014)	Meditators	Between-subject (20 mindfulness meditators v. matched control)	Libet Clock	No difference in TB between meditators and control
Lush et al. (2016b)	Meditators	Between-subject (long-term Buddhist meditators v. matched control w/ no experience of mindfulness mediation)	Libet Clock	Long-term Buddist meditators have stronger TB (tone binding) than control
Lush et al. (2016a)	Meditators v. Hypnotisability	Between-subject (long-term meditators v. undergraduates categorized by hypnotisability (high, medium, low))	Libet Clock	High hypnotisability individuals have later W times (delayed awareness of intention). Meditators have earlier W times (metacognitive access to intention awareness)
Lush et al. (2017)	Hypnotisability	Between-subject (High v. Low on Hypnotisability) v. Within-subject (Hypnotic induction manipulation prior to Libet Clock block)	Libet Clock	High-hypnotisable individuals have reduced TB (tone binding) in the posthypnotic condition compared to the voluntary condition. Degree of reported involuntariness feeling is significantly associated with the reduction in tone binding

Table 2. Personal factors – States (Experimentally Manipulated)

Study	Topic	Method	TB Measure	Summarised Findings
Render et al. (2024)	Sexual arousal	Between-subject (Sexual arousal group: erotic scene from Love (2015) V. Calm pleasure: romantic scene from Pride and Prejudice (2005) V. Neutral: pedestrian street scene) x Within-subject (Pre v. Post Libet Clock)	Libet Clock	Being in a high sexual arousal state reduces TB (action binding)
Render & Jansen (2021)	Sexual arousal (manipulated)	Between-subject (Sexual arousal group: Watched an erotic film clip v. Neutral control group with intertrial images: Watched a documentary and saw neutral images between trials v. Neutral control group without intertrial images) x Within-subject (pre- v. post induction)	Libet Clock	Being in a higher general arousal (not sexual arousal) leads to reduced TB (action binding). Being in a lower arousal state leads to stronger TB (action binding).
De Pirro et al. (2020)	Alcohol	Between-subject (Ethanol doses: 0.0. i.e. placebo v. 0.4 v. 0.6g/kg)	Libet Clock	Consuming alcohol leads to stronger TB (total binding)
Obhi et al. (2013)	Depression	Within-subject (no prime v. depression priming v. previous-day priming)	Interval Estimation	Recalling depressive episode leads to reduced TB
Soral et al. (2021)	Learned helplessness (manipulated)	Between-subject (behavioural helplessness training short v. long v. no training at all)	Libet Clock	Being in a control-deprivation state (learned helplessness) reduces TB (total binding)
Driller et al. (2023)	Local anaesthesia (manipulated)	Within-subject (Anesthetised v. intact)	Interval Estimation	Having local anaesthesia applied to finger reduces TB
Moore et al. (2011)	Schizophrenic (Manipulated)/Ketamine	Within-subject (placebo saline v. 100ng/mL plasma of low-dose ketamine: sessions were 1 week apart)	Libet Clock	Being administered Ketamine leads to stronger TB (total binding, driven by action binding). Feeling more disconnected from one's own body is associated with greater TB.
Van Der Westhuizen et al. (2017)	Testosterone (manipulated/Administered)	Within-subject (placebo v. 0.5ms testosterone)	Libet Clock	Being administered testosterone leads to stronger TB (action binding)

Table 3. Interpersonal factors

Study	Topic	Category	Method	TB Measure	Summarised Findings
Spaccasassi et al. (2023)	Moral Judgment (manipulated)	Individual Differences - Long-term traits/conditions/ personality	Within-subject (Participants watched 36 short video scenarios: Intentional Harm: agent deliberately harms victim v. Accidental Harm: agent unintentionally harms victim v. Neutral: no harm occurs) + Correlational	Interval Estimation	People with stronger TB are more likely to view accidental harms caused to others as morally impermissible.
Obhi et al. (2012)	Induced social power (manipulated)	States	Within-subject (no prime v. high-power v. low-power	Interval Estimation	Recalling an episode of having low power over others leads to reduced TB
Edwards et al. (2025)	Psychosocial stress (manipulated)	States	Between-subject (Stress v. Control) x Within-subject (Pre v. Post)	Interval Estimation	Psychosocial stress induction leads to reduced TB.
Malik & Obhi (2019)	Social Exclusion/Inclusion (manipulated)	States	Within-subject (baseline v. social exclusion v. social inclusion)	Interval Estimation	Recalling an episode of being socially excluded leads to reduced TB.
Sun et al. (2023)	Vicarious ostracism (manipulated)	States	Exp 1. Within-subjects (baseline condition (no social context) v. recalling vicarious inclusion episode v. recalling vicarious ostracism episode.) Exp 2. Within-subjects (baseline condition v. observing VR Cyberball game with vicarious inclusion v. VR Cyberball game with vicarious ostracism)	Exp 1. Interval estimation Exp 2. Libet Clock	Seeing someone else being socially excluded leads to reduced TB (total binding)

Appendix B

Pilot Experiments – Chapter 4

The first experiment aimed to assess if sensorimotor control deprivation would lead to similar attenuated temporal binding effect in similar way that cognitive learned helplessness would impair temporal binding as observed in Soral et al. (2021). To induce sensorimotor control deprivation, we adapted the reaching task of Wen and Haggard (2020) whereby participants had to control a white circle using the computer mouse to touch a red cross on the screen with two conditions: 90-270° linear disturbance v. 30° linear disturbances. That is, in the former, the white circle's trajectory was rotated clockwise by a randomly selected angular value between 90 and 270 for each trial relative to the actual mouse trajectory movement. Thus, in this block of motor control, it was challenging to control the white circle and successfully reach the cross with it. In the latter condition, the white circle only rotated clockwise 30° relative to the actual mouse movement, so the white circle was easier to control with little perturbances. Participants completed both conditions of the motor control task and after the end of each motor block, participants' temporal binding effect was measured using the interval reproduction paradigm to capture immediately the binding magnitude following the 'aftermath' of each motor task.

Method

Participants

For Pilot Experiment 1, a power analysis using G*power was conducted to determine the sample size. For a repeated-measure ANOVA, given $\alpha = .05$, a power of 0.80 and a medium effect size ($f = 0.25$); a sample size of 34 was required. We thus aimed at recruiting 40 participants to account for incomplete participation. A total of 41 psychology undergraduate university students (*M*age = 21.93, *SD* = 8.20, 12 Males, 1 Non-binary) were recruited from the University of East Anglia, who completed the experiment in return for course credits or payment at the rate of £12/hour. All experiments in this study were granted ethical approval by the School of Psychology Ethics Committee at the University of East Anglia.

Materials, Design, Procedure

Interval Reproduction Task

Identical to Pilot Experiment 2 of Chapter 4

Motor Control Task

Identical to Pilot Experiment 1 and 2 of Chapter 4

Procedure

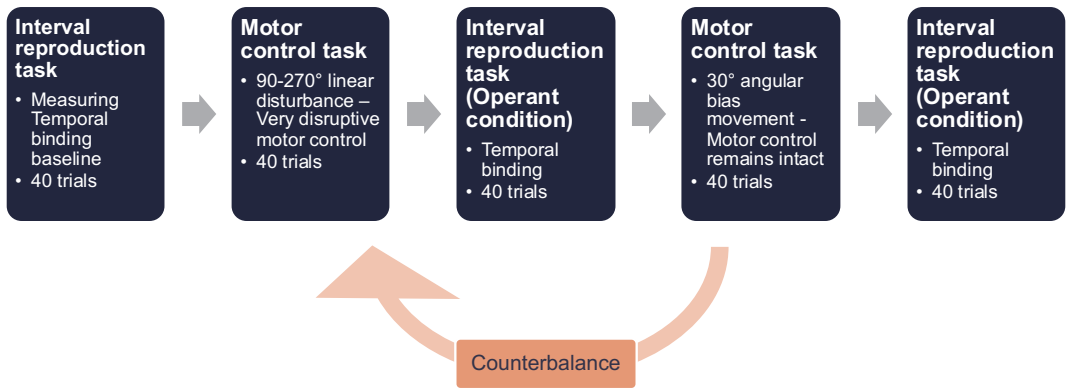


*Figure 1.*Outline of the experimental design of Pilot Experiment 1 (and also of Pilot

Experiment 2, with the exception that the 30° angular bias was replaced with full control i.e., no angular transformation in the second experiment).

Results

Temporal Binding effects after motor control tasks

In the primary analysis, a one-way repeated-measure ANOVA was conducted to investigate the differences in the magnitude of the temporal binding effect in the baseline condition, following the 30° angular motor task and following the 90-270° motor task. We found that there was a significant main effect of the condition in which the interval reproduction operant trials were conducted (baseline i.e., prior to any manipulation/ after 30° motor task/ after 90° motor task) $F(2, 80) = 6.91$, $p = .002$, $\eta_p^2 = .147$.

Post-hoc comparisons showed that the baseline temporal binding effect ($M = -202.67$, $SD = 56.62$) was significantly greater than the temporal binding effect following the 30° motor control task ($M = -100.36$, $SD = 40.83$, $p = .015$) and the temporal binding 90-270° angular ($M = -121.73$, $SD = 43.11$, $p = .021$), respectively. However, the magnitude of the temporal binding effect measured after the completion of the two motor control tasks did not differ between the two conditions ($p = 1.00$)

We conducted further analyses to explore whether the order in which participants performed the motor tasks influenced their temporal binding effect following the end of each motor task. A 2 (Starting with the 30° motor control task; starting with the 90-270° motor control task) × 3 (Temporal Binding baseline; Temporal Binding 30° angular; Temporal Binding 90-270° angular) mixed ANOVA was conducted. While there was a significant main effect of the chronological order in which the temporal binding effect was measured as found in the primary analysis above $F(2, 78) = 7.10$, $p = .001$, $\eta_p^2 = .154$; there was no significant main effect of the motor control tasks' order (Starting with 30° or with 90-270° motor task) $F(1, 39) = .17$, $p = .683$, $\eta_p^2 = .004$. Therefore, the magnitude of the temporal binding effect measured over time did not significantly differ between the group which were assigned to starting with 30° motor task first and the other group starting with 90-270° motor condition.

Table 4.

Mean and (Standard Errors) SE of the temporal binding effect in each experimental condition.

	Temporal binding	Mean (ms)	SE
Participants starting with 30° motor task	baseline	-239.03	88.72
	30° angular	-142.81	63.61
	90-270° angular	-109.58	67.75
Participants starting with 90-270° motor task	baseline	-176.91	74.67
	30° angular	-70.30	53.53
	90-270° angular	-131.04	57.02

There was no significant interaction effect between the order of the motor task and the condition in which the temporal binding effect was measured $F(2, 78) = 1.59$, $p = .211$, $\eta_p^2 = .039$. However, at the within-group level, for participants starting first with the 30° motor task, the temporal binding effect was significantly greater in the baseline condition compared with the temporal binding effect following the end of the 90-270° task ($p = .014$) (for statistical means see Table 4). On the other hand, for participants assigned to starting with 90-270°, their temporal binding effect did not significantly change throughout the experiment ($ps \geq .074$).

Discussion

In Pilot Experiment 1, we found that both temporal binding effects measured subsequently after the block of the 90-270° angular disturbance and after the block of 30° angular motor task reduced significantly compared to the baseline temporal binding effect. Furthermore, we did not find any difference in temporal binding effect measured after the completion of the two motor control tasks. Considering that the angular bias in which the white circle moved in the 30° motor control task, despite not being as disruptive as the 90-270° angular bias condition, there might still be a moderate degree of visual mismatch. Thus, we wished to amend the 30° motor control task so that there would be no rotation in the trajectory of the white circle movement at all. As a result, in the next experiment, we investigated whether the motor tasks with different degrees of disturbances i.e., either extremely disruptive (90-270° angular bias) or entirely not disruptive at all (full-control, no angular transformation) would influence temporal binding effect differently. We would also further include the explicit agency measure by asking participants to rate their feeling of being in control over the white circle. By including the explicit control rating, this would help us provide insight into how participants consciously inferred their subjective agency over the white circle (Wen et al., 2023).

Note that in the exploratory analysis, we found that the magnitude of the temporal binding effect amongst individuals starting first with the 90-270° disruptive task maintained stable across the experiment. We speculated that for these participants, they had already experienced high level of disruption due to their first challenging motor control task, making the subsequent 30° angular task appear to be significantly less disruptive. It is possible that such experience of being 'thrown in the deep end' (performing the 90-270° motor task first) and gaining control again (performing the 30° motor task) helped those assigned to such condition preserve their temporal binding over time across the experiment. On the other hand, Participants starting with the 30° motor task first then subsequently facing with the significantly more challenging 90-270° motor control block showed an attenuated magnitude of temporal binding following this 90-270° linear disturbance task. In other words, switching from easy to the difficult motor control task did more harm than good, impairing one's implicit agency when engaging in the highly disruptive task. In Pilot Experiment 2, we planned to conduct similar analysis to assess this finding.

Pilot Experiment 2

In the previous experiment, the 30° angular bias disruption still had some mismatch between the mouse movements and the actual white circle trajectories. We proposed that this might have resulted in the absence of meaningful difference in the temporal binding effect measured after the 90-270° angular bias versus temporal binding measured after the 30° motor task. Therefore, in the second experiment, in lieu of the 30° angular transformation, participants were given full control over the white circle i.e., there were no disturbances in the mouse movements and thus the trajectory of the white circle was identical to which of the mouse.

Method

Participants

For Pilot Experiment 2, a power analysis using G*power was also conducted to determine the sample size. For a repeated-measure ANOVA, given $\alpha = .05$, a power of 0.80 and a medium effect size ($f = 0.25$); a sample size of 34 was required. We recruited 41 psychology undergraduate university students (*M*age = 20.02, *SD* = 3.11, 8 Males, 3 Non-binaries) who completed the study in return for course credits.

Materials

In the second experiment, we employed similar apparatus and stimuli, except that in the motor task condition which had a lower degree of disturbances, the mouse trajectory was no longer interfered with. This allowed participants to have full control over the white circle whose movements were identical to how the participants controlled the mouse.

We also added an explicit measure of agency by asking individuals to respond to the question 'On a scale of 0 to 100, how much control did you feel over the white circle?' with 0 = '*No control'*, 100 = '*Full Control'* on a slider which did not have a marker at the start and the marker only appeared once the participants had clicked on the scale. The explicit measure of the feeling of being control was asked at the end of each experimental block of the motor task and was displayed below the frustration rating.

Design and procedure

The design and procedure of Pilot Experiment 2 were identical to which in Pilot Experiment 1.

Results

A one-way repeated-measure ANOVA was conducted to investigate the differences in the magnitude of the temporal binding effect in the baseline condition, following the full-control motor task and following the 90-270° motor task. As Pilot Experiment 1, we also found that there was a significant main effect of the condition in which the interval reproduction operant trials were conducted (baseline i.e., prior to any manipulation; after full-control motor task or after 90° motor task) $F(2, 80) = 8.31$, $p < .001$, $\eta_p^2 = .172$.

Post-hoc comparisons showed that the baseline temporal binding effect ($M = -127.95$, $SD = 361.13$) was significantly greater than the temporal binding effect following the full-control motor task ($M = -33.25$, $SD = 306.79$, $p = .003$) and the temporal binding 90-270° angular ($M = -48.27$, $SD = 377.67$, $p = .010$), respectively. However, the magnitude of the temporal binding effect measured after the completion of the two motor control tasks did not differ between the two conditions ($p = 1.00$).

Analyses to examine whether there were significant effects of the motor task order on individuals' temporal binding following the end of each motor control condition as observed in Pilot Experiment 1. A 2 (Starting with the full-control motor task; starting with the 90-270° motor control task) × 3 (Temporal Binding baseline; Temporal Binding Full-Control motor; Temporal Binding 90-270° angular bias) mixed ANOVA was conducted. While there was a significant main effect of the chronological order in which the temporal binding effect was measured as found in the primary analysis above $F(2, 78) = 8.56$, $p < .001$, $\eta_p^2 = .18$; there was no significant main effect of the motor control tasks' order (Starting with full-control or with 90-270° motor task) $F(1, 39) = .16$, $p = .620$, $\eta_p^2 = .01$. Therefore, the magnitude of the temporal binding effect measured over time did not significantly differ between the group which were assigned to starting with full-control motor task first and the other group starting with 90-270° motor condition.

Table 5.

Mean and SE of the temporal binding effect in each experimental condition.

	Temporal binding	Mean (ms)	SE
Participants starting with full-control motor task	Baseline	-113.751	83.85
	Full control	-15.47	71.17
	90-270° angular	5.61	86.95
Participants starting with 90-270° motor task	Baseline	-140.22	77.92
	Full control	-48.61	66.14
	90-270° angular	-94.81	80.81

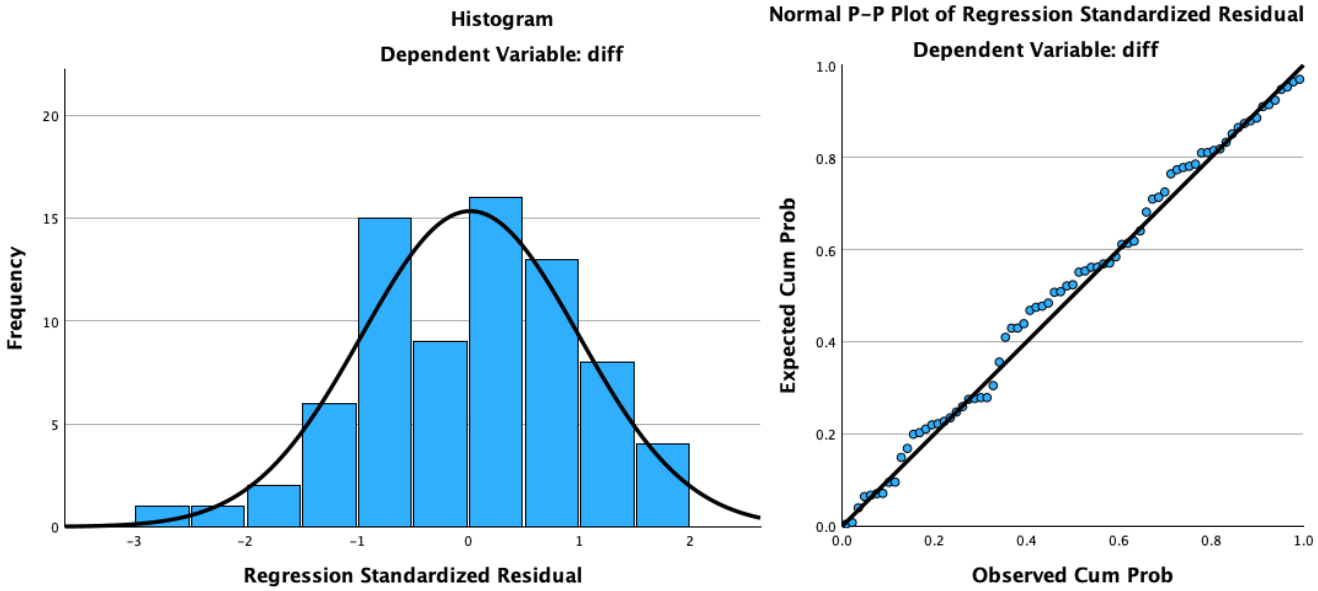
Similar to Pilot Experiment 1, there was no significant interaction effect between the order of the motor task and the condition in which the temporal binding effect was measured $F(2, 78) = 1.35$, $p = .266$, $\eta_p^2 = .033$. That said, we also replicated the results shown in the first experiment that at the within-group level, for participants starting first with the full-control motor task, their temporal binding effect was significantly greater in the baseline condition than the temporal binding effect following the end of the 90-270° task ($p = .008$) (for statistical means see Table 5). On the other hand, for participants assigned to starting with 90-270°, their temporal binding effect did not significantly change throughout the experiment ($ps \geq .054$).

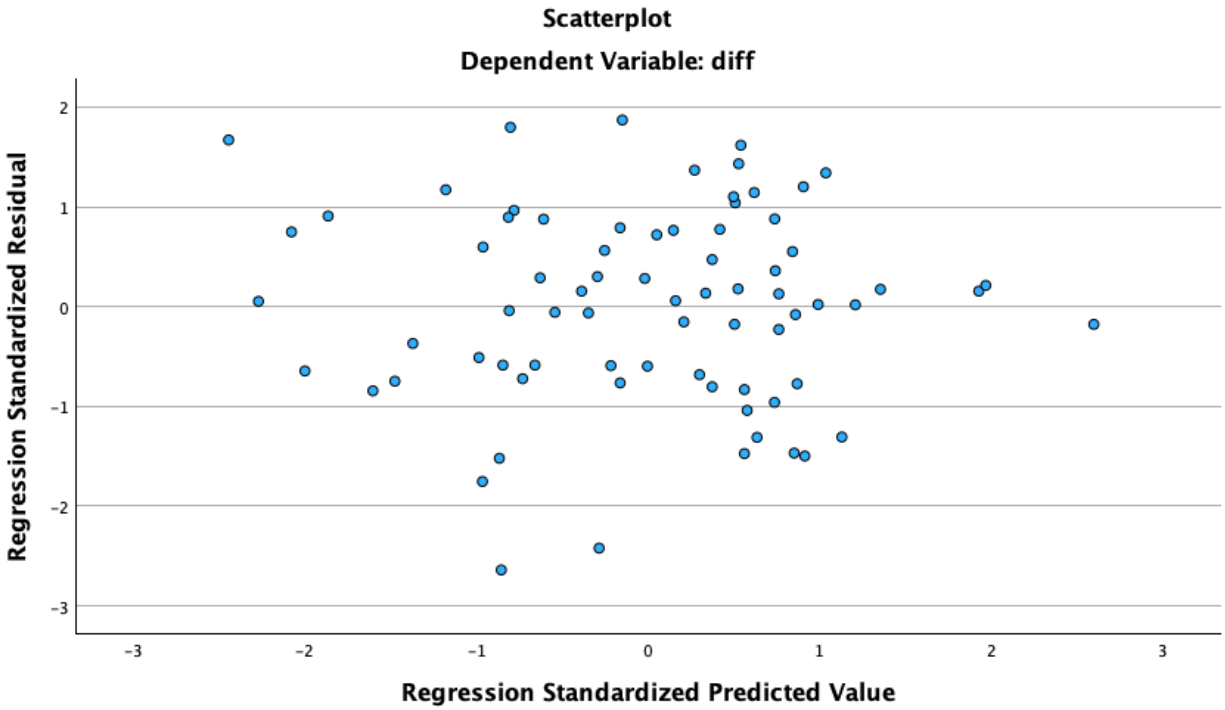
Discussion

In Pilot Experiment 2, we found similar results to which of Pilot Experiment 1. both temporal binding effects following the 90-270° angular disturbance and 30°/full-control motor task reduced significantly compared to the baseline binding effect; and that the magnitude of the temporal binding effects following each motor control block did not differ.. Importantly, we observed similar pattern of starting order of task difficulty on the subsequent magnitude of temporal binding. In particular, temporal binding only significantly weakened following the 90-270° angular disturbance motor block compared to the baseline temporal binding effect for those full-control motor task. On the contrary, similar to Pilot Experiment 1, binding did not differ between conditions and compared to baseline amongst participants performing the 90-270° angular disturbance task first. Both experiments showed such pattern: being ‘thrown in the deep end’ (performing the 90-270° motor task first) and gaining control again (performing the 30°/Full-control motor task) helped those assigned to such condition preserve their temporal binding over time across the experiment. In contrast, beginning with a controllable task and subsequently facing a significantly more challenging assignment impaired one’s implicit agency when later engaging in the highly disruptive task.

Appendix C

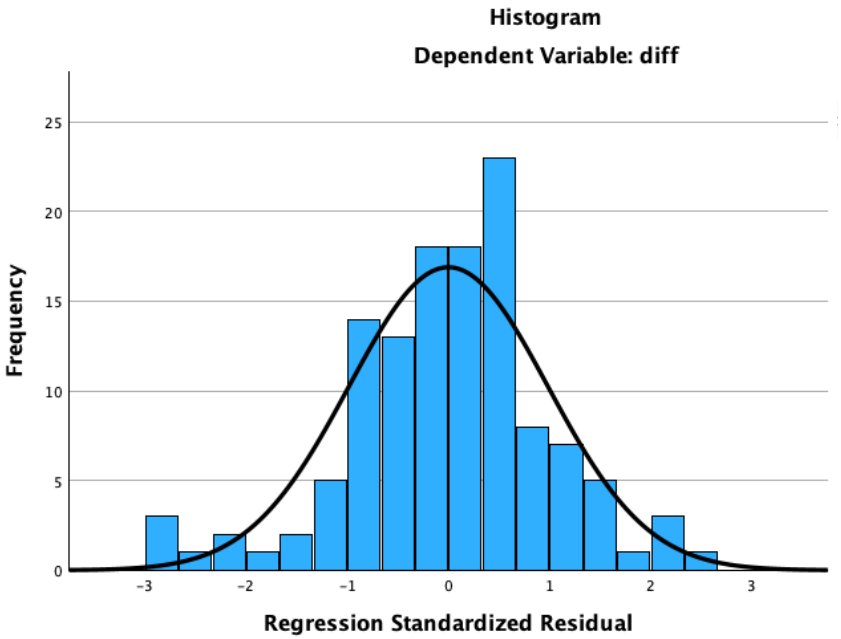
Histogram and Scatter plot – linear regression final model Experiment 3

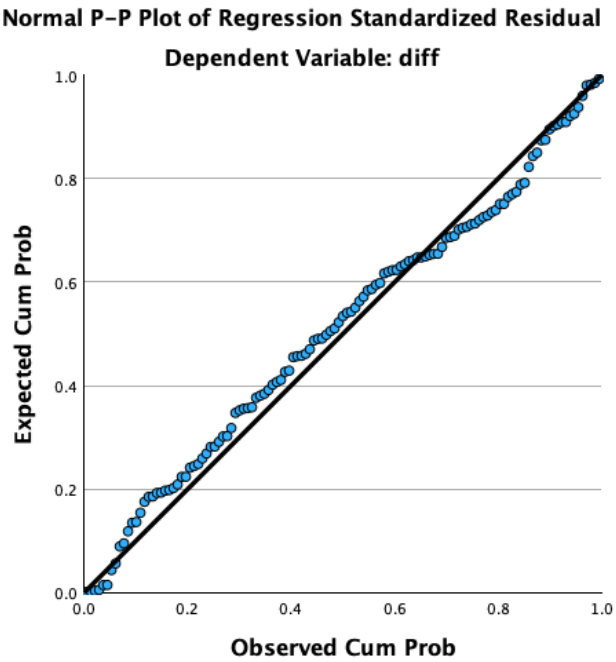


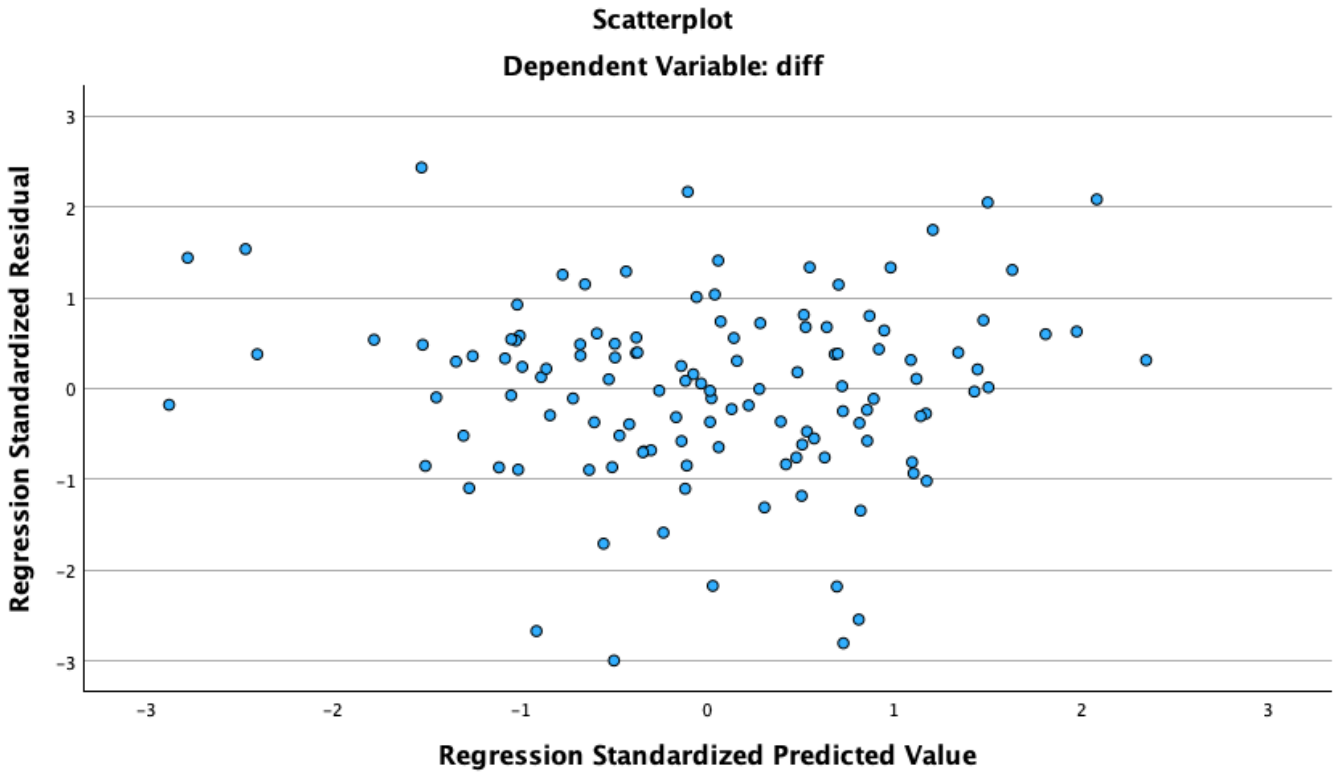


Note. Dependent variable (diff) = Mean (Perceived – Actual) (temporal binding effect).

Histogram and Scatter plot – linear regression model Experiment 4

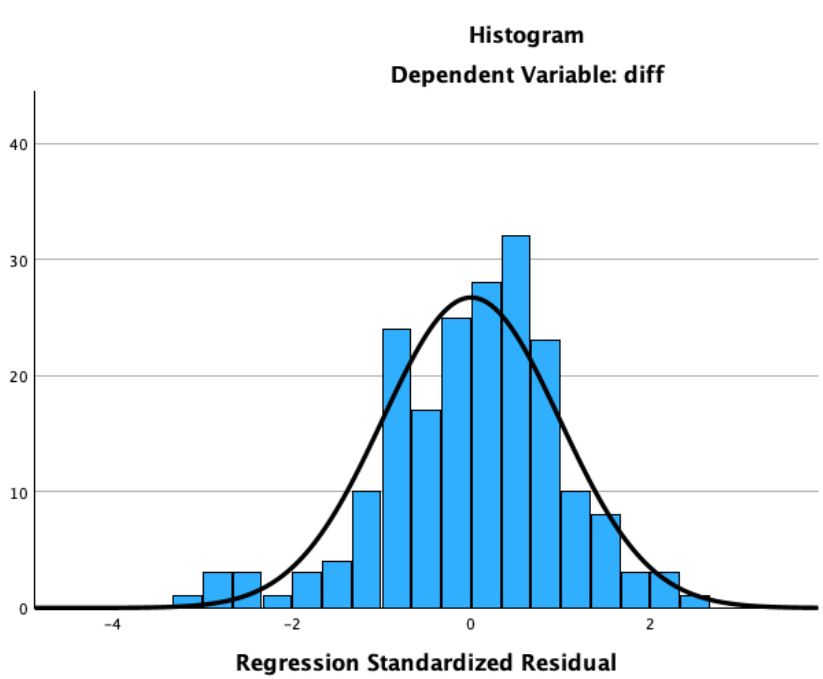


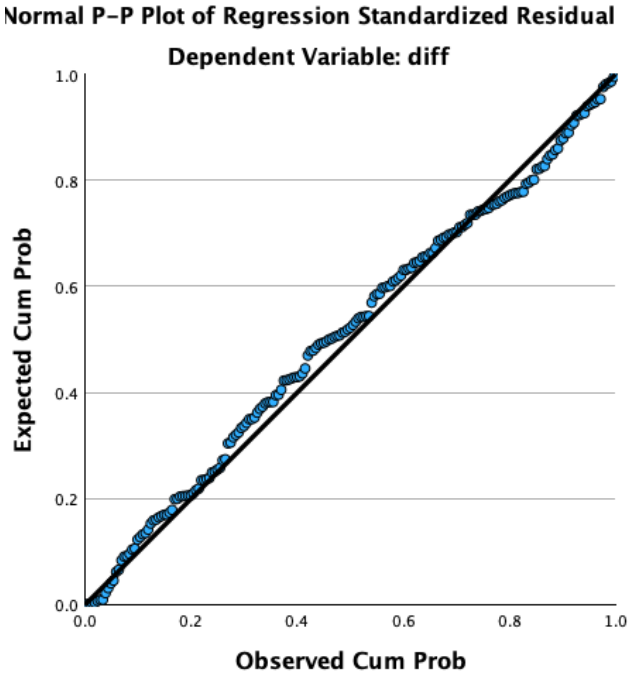


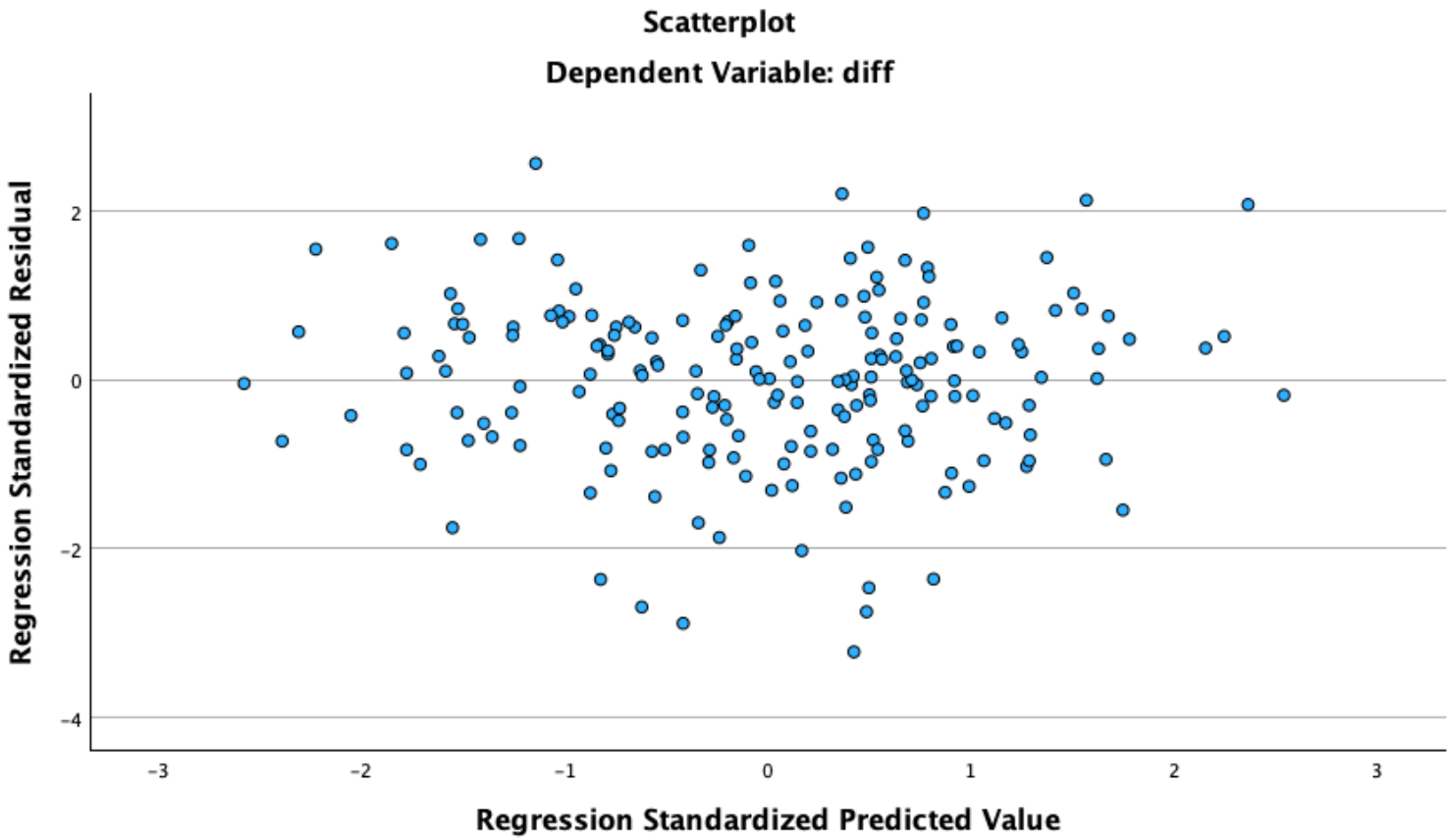


Note. Dependent variable (diff) = Mean (Perceived – Actual) (temporal binding effect).

Histogram and Scatter plot – linear regression combined data Experiment 3 and 4







Note. Dependent variable (diff) = Mean (Perceived – Actual) (temporal binding effect).

Appendix D
Journalling Instructions

Control Group:
Please take a moment to think about what you did yesterday. That is, create a mental outline of what you did during that time. Now please write these activities out in a list format.

Use the instructions below to help guide you through this process:
Use whatever writing format you like.
Try to leave out emotions, feelings, or opinions pertaining to your plans.
Focus on exactly what you did.
Do not worry about perfect grammar and spelling.
Remember: Anything you write will remain strictly confidential. In fact, we do not want you to show what you write to anyone else. For the purposes of this study, this is a private document in which you can write freely without intent to share it with anyone.

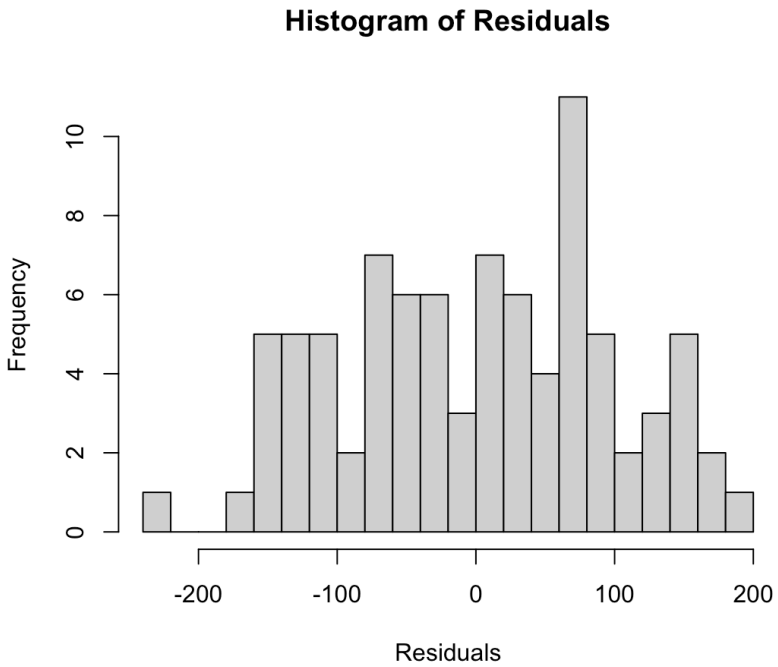
Please write your journaling on a notebook or papers and make sure to add time stamps i.e., the date of the journaling. When you attend the in-person lab session after 7 days of journaling, you will be asked to show us your notebook/papers on which you journaled, together with the time stamps of each journaling day for 7 days. Please note that we won't be reading the content of the journal but rather we will only wish to see the proof of your everyday journaling.

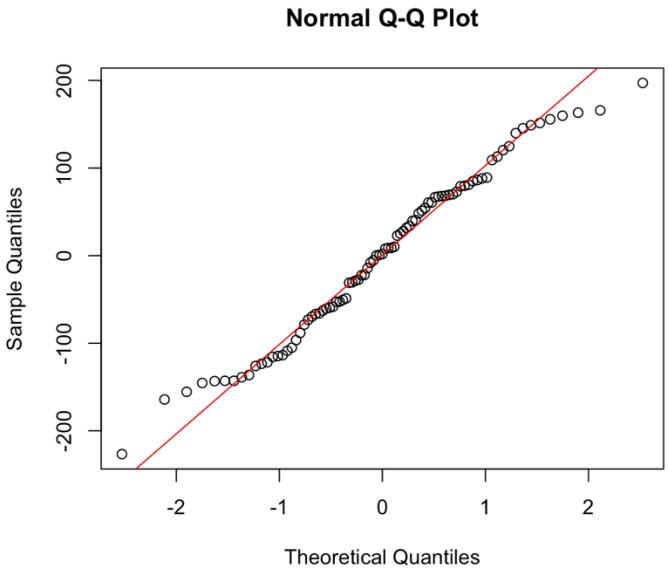
Journal Group:
There are many people in our lives, both close and distant, that we might be grateful about. Please take a moment to think back over the past several years of your life and remember an instance when someone did a kind act (or acts) for you for which you are extremely grateful. Think of the people – parents, children, spouses/partners, relatives, friends, neighbours, teachers, employers, and so on – who have been especially generous and thoughtful towards you. For example, you may feel grateful for a friend who gave you a compliment or a parent who helped you. Now, write a letter to one of these individuals.

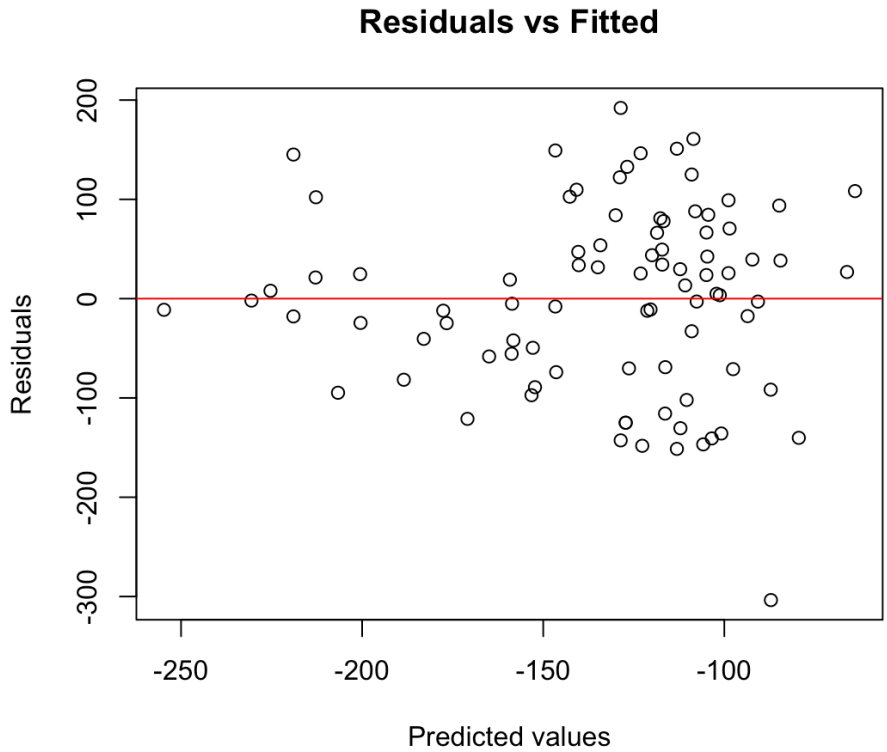
Use the instructions below to help guide you through this process:
Use whatever letter format you like, but remember to write as though you are directly addressing the individual you are grateful to. If it is helpful to head the letter "Dear so- and-so," or end with "Sincerely, XXX," feel free to do so.
Do not worry about perfect grammar and spelling.
Describe in specific terms the kind act this person bestowed upon you and how the kind act affected your life.
Describe what you are doing now and how you often remember their efforts.
Remember: Anything you write will remain strictly confidential. In fact, we do not want you to show what you write to anyone else. For the purposes of this study, this is a private document in which you can express your gratitude freely without intent to deliver it to anyone.
Please write your journaling on a notebook or papers and make sure to add time stamps i.e., the date of the journaling. When you attend the in-person lab session after 7 days of journaling, you will be asked to show us your notebook/papers on which you journaled, together with the time stamps of each journaling day for 7 days. Please note that we won't be reading the content of the journal but rather we will only wish to see the proof of your everyday journaling.

Appendix E

Histogram and Scatter plot – exploratory linear regression model Experiment 6

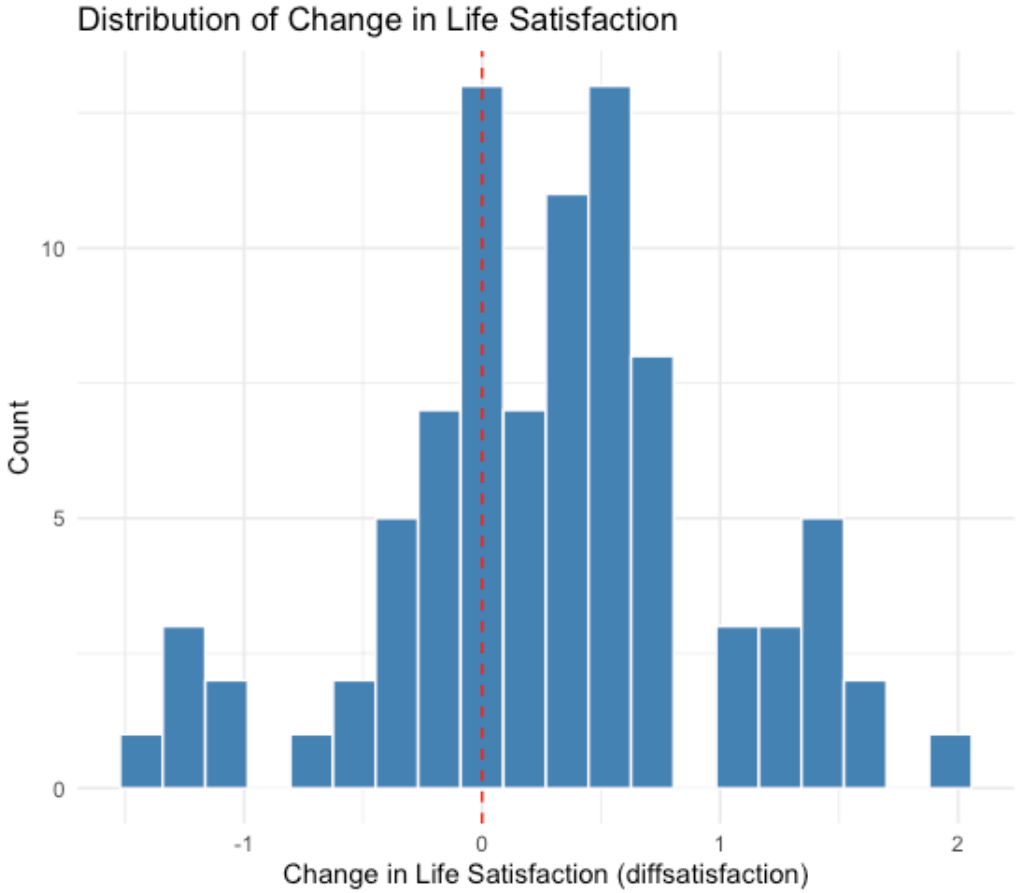


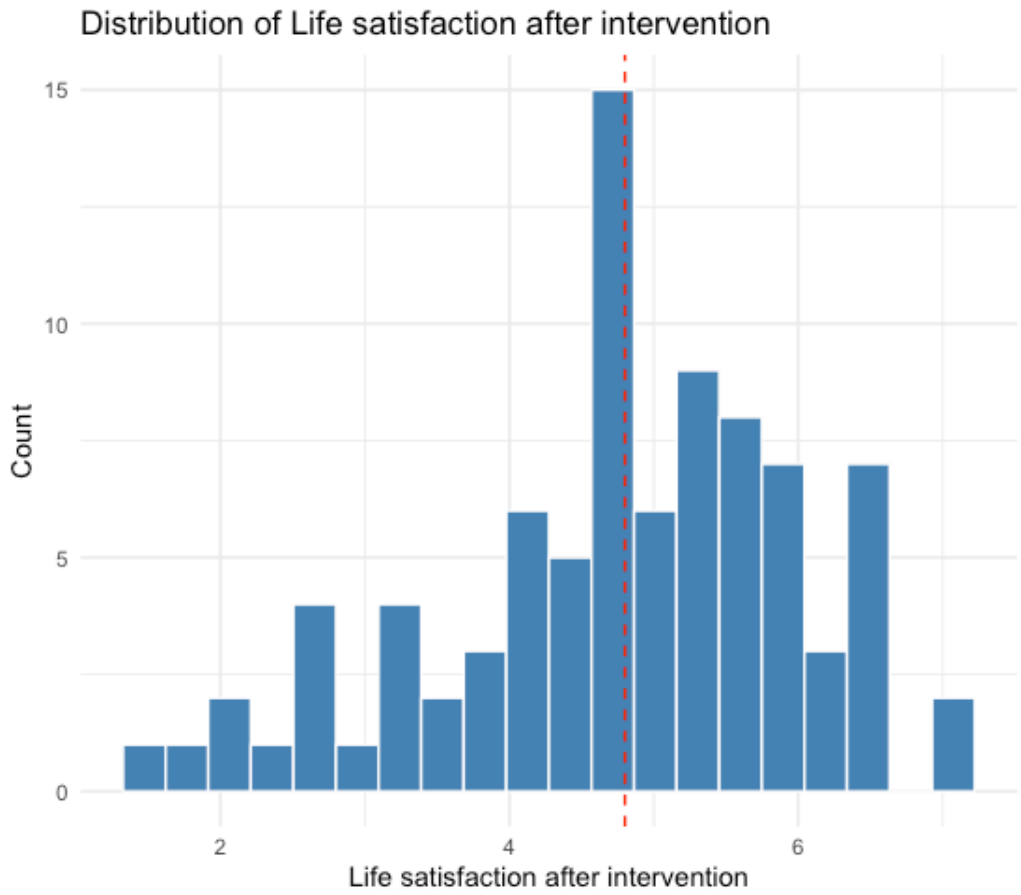




Note that plots here were produced using R (whilst plots in Appendix C were produced using SPSS).

Distributions of change in life satisfaction and level of life satisfaction after intervention:





Appendix F

Low v. High Identification group differences in attitudinal and individual differences measures

A) Descriptive statistics

Variable	**Low Identification (N = 34) Mean (SD)**	**High Identification (N = 36) Mean (SD)**
Experimenter's Competence	87.41 (11.73)	93.00 (11.75)
Experimenter's Favourability	83.59 (20.78)	89.97 (20.86)
Experimenter's Warmth	84.00 (22.62)	88.56 (19.99)
Identification with the Experimenter	4.46 (0.44)	5.78 (0.50)
Inclusion of Experimenter	6.01 (0.99)	6.41 (0.50)
Life Satisfaction	4.69 (1.20)	4.45 (1.50)
Negative Mood	1.95 (0.41)	1.91 (0.49)
Positive Mood	2.39 (0.53)	2. (0.53)
Harm/Care	4.76 (0.61)	4.84 (0.61)
Fairness/Reciprocity	4.65 (0.49)	4.81 (0.54)
Ingroup/Loyalty	3.16 (0.78)	3.62 (0.68)
Authority/Respect	3.38 (0.90)	3.61 (0.84)
Purity/Sanctity	3.32 (0.60)	3.75 (0.78)
Patriotism	3.09 (0.65)	3.34 (0.67)
Self-efficacy	3.00 (0.29)	2.98 (0.40)
RWA	3.08 (0.64)	3.20 (0.60)
SDO	3.65 (1.26)	3.57 (1.23)

B) Independent Samples t-Test with Effect Size

Variable	t	df	p (two-tailed)	Mean Diff	95% CI Lower	95% CI Upper	Cohen's d	BF10
Experimenter's Competence	-1.99	68	.051	-5.59	-11.19	0.02	-0.476	1.08
Experimenter's Favourability	-1.28	68	.204	-6.38	-16.32	3.55	-0.307	0.38
Experimenter's Warmth	-0.89	68	.374	-4.56	-14.72	5.61	-0.214	0.26
Identification with the Experimenter	-11.67	68	<.001	-1.32	-1.55	-1.10	-2.79	>1000
Inclusion of Experimenter	-2.12	68	.037	-0.39	-0.76	-0.02	-0.508	1.37
Life Satisfaction	0.75	68	.455	0.24	-0.41	0.90	0.179	0.23
Negative Mood	0.30	67	.764	0.03	-0.18	0.25	0.072	0.19
Positive Mood	-0.32	67	.754	-0.04	-0.30	0.21	-0.076	0.19
Harm/Care	-0.53	68	.599	-0.08	-0.37	0.21	-0.126	0.21
Fairness/Reciprocity	-1.30	68	.196	-0.16	-0.41	0.09	-0.311	0.39
Ingroup/Loyalty	-2.62	68	.011	-0.46	-0.81	-0.11	-0.626	3.75
Authority/Respect	-1.10	68	.278	-0.23	-0.65	0.19	-0.262	0.31
Purity/Sanctity	-2.55	68	.013	-0.43	-0.76	-0.09	-0.611	3.26
Patriotism	-1.53	68	.130	-0.24	-0.56	0.07	-0.366	0.53
Self-efficacy	0.17	68	.868	0.01	-0.15	0.18	0.04	0.18
RWA	-0.81	68	.419	-0.12	-0.42	0.18	-0.195	0.25
SDO	0.28	68	.780	0.08	-0.51	0.68	0.067	0.19

Appendix G

Regression coefficients predicting change in Temporal Binding Post-Election – Pre-Election

Predictor	β	SE	t	p	VIF
Intercept	0.19	0.27	0.70	0.486	
State group (Red v. Blue)	0.07	0.18	0.41	0.685	1.04
State group (Red v. Swing)	0.02	0.17	0.14	0.889	1.04
Party Identification (Republican v. Democratic)	-0.12	0.25	-0.47	0.636	1.35
Party Identification (Republican v. None)	-0.04	0.28	-0.15	0.883	1.35
Voted Candidate (Harris v. Trump)	-0.35	0.26	-1.36	0.176	3.39
Explicit Political Agency	0.18	0.09	2.08	0.039*	1.35
Voting Confidence	0.07	0.10	0.66	0.507	1.26
Age	-0.04	0.07	-0.54	0.590	1.13
Trump Likelihood Winning	0.15	0.10	1.52	0.130	2.12
Harris Likelihood Winning	0.11	0.11	0.95	0.342	2.64
Life satisfaction change	0.06	0.15	0.40	0.689	1.07
Impact of No Vote on Presidential Outcome	0.06	0.09	0.68	0.499	1.66
Impact of No Vote on State Outcome	0.06	0.08	0.74	0.461	1.25
Perceived influence on Presidential Winner	0.00	0.09	-0.06	0.956	1.55

Regression coefficients Predicting Political Efficacy (Time 1)

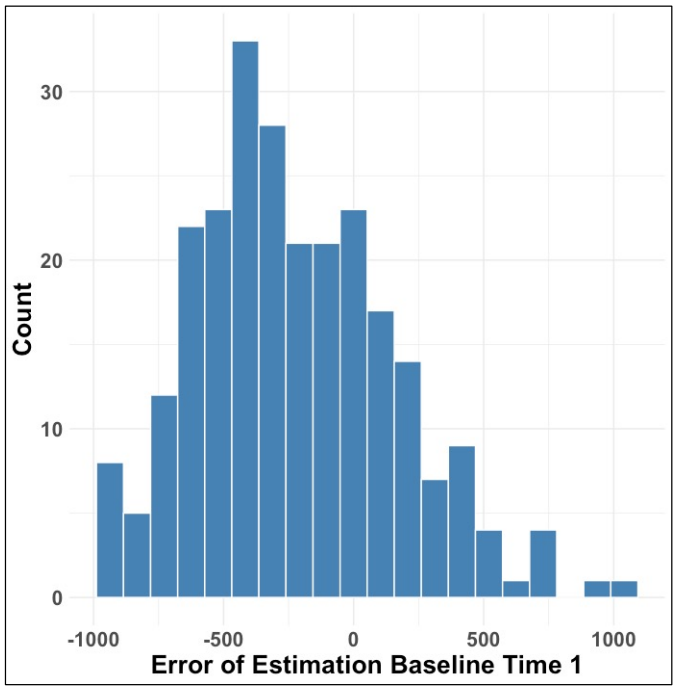
Predictor	β	SE	t	p	VIF
Intercept	0.12	0.25	0.49	0.629	
State group (Red v. Blue)	-0.16	0.16	-0.98	0.327	1.03
State group (Red v. Swing)	0.15	0.16	0.95	0.343	1.03
Temporal Binding T1	-0.09	0.06	-1.35	0.179	1.05
Party Identification (Republican v. Democratic)	-0.17	0.23	-0.71	0.478	1.34
Party Identification (Republican v. None)	-0.18	0.26	-0.71	0.481	1.34
Voted Candidate (Harris v. Trump)	-0.01	0.26	-0.03	0.978	4.09
Life satisfaction (T1)	0.26	0.06	4.01	0.000***	1.04
Voting confidence	0.16	0.06	2.43	0.016*	1.05
Age	0.15	0.07	2.23	0.027*	1.06
Trump Likelihood Winning Election Prediction	-0.10	0.09	-1.07	0.286	2.10
Harris Likelihood Winning Election Prediction	0.06	0.11	0.53	0.598	2.92

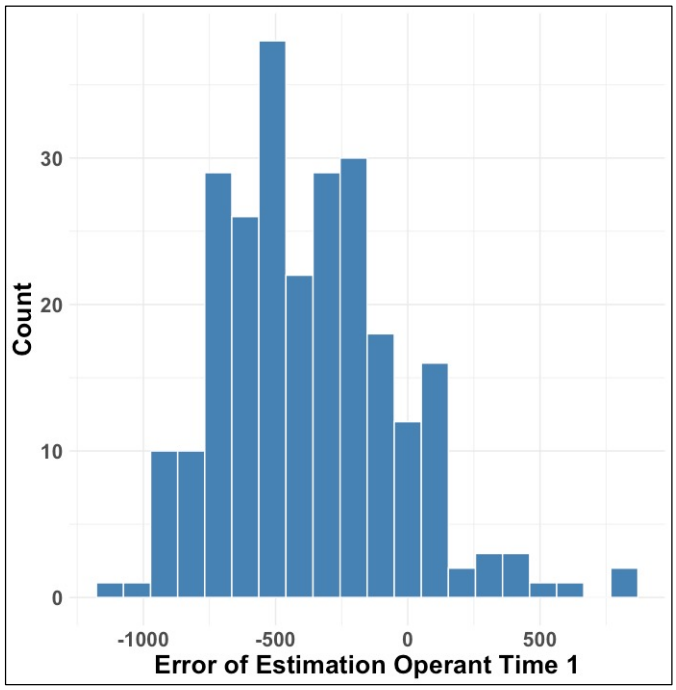
Regression coefficients Predicting Political Efficacy (Time 2)

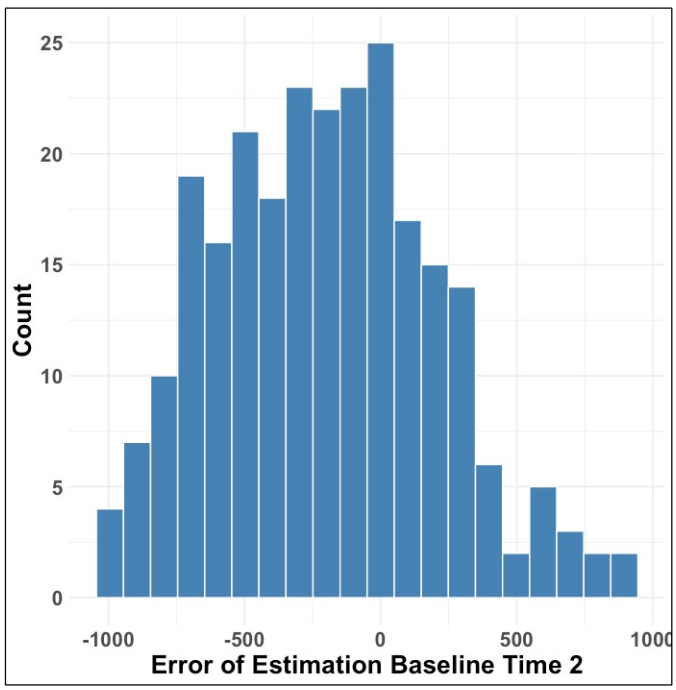
Predictor	β	SE	t	p	VIF
Intercept	0.13	0.15	0.84	0.401	
Temporal Binding T2	0.02	0.04	0.52	0.603	1.04
State group – Blue	-0.18	0.10	-1.84	0.068	1.03
State group – Swing	-0.19	0.10	-1.94	0.054	1.03
Party Identification (Republican v. Democratic)	0.02	0.14	0.12	0.902	1.31
Party Identification (Republican v. None)	-0.05	0.15	-0.35	0.728	1.31
Voted for Trump (Harris v. Trump)	0.11	0.14	0.82	0.411	2.94
Life satisfaction T2	0.11	0.04	2.51	0.013*	1.19
Impact of No Vote on State Outcome	0.11	0.05	2.19	0.030*	1.59
Impact of No Vote on Presidential Outcome	0.02	0.04	0.36	0.719	1.21
Perceived Influence on State Outcome	14.16	14.16	14.16	<.001***	1.62
Age	-0.20	-0.20	-0.20	.843	1.08

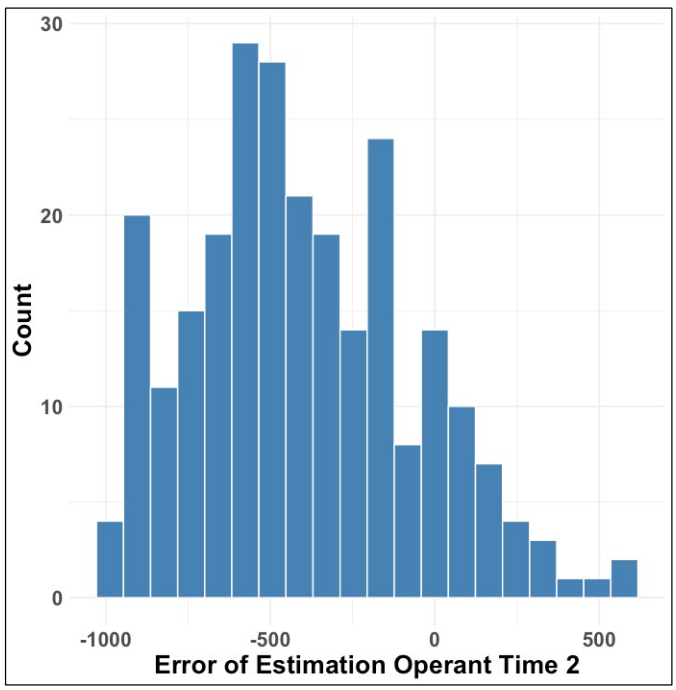
Appendix H

Histograms showing distributions of error of estimation for baseline time 1, operant time 1, baseline time 2 and operant time 2



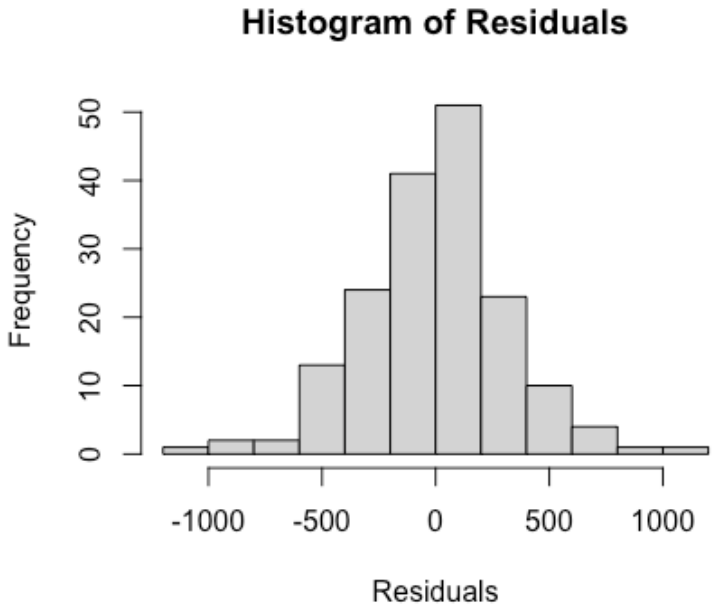


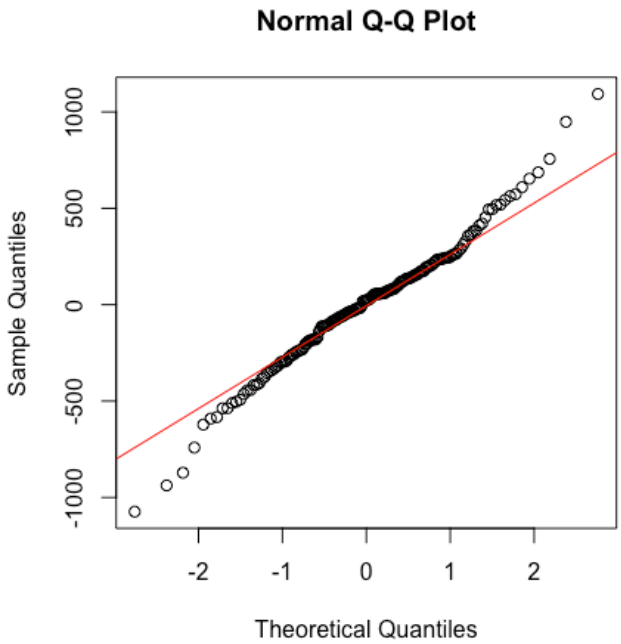


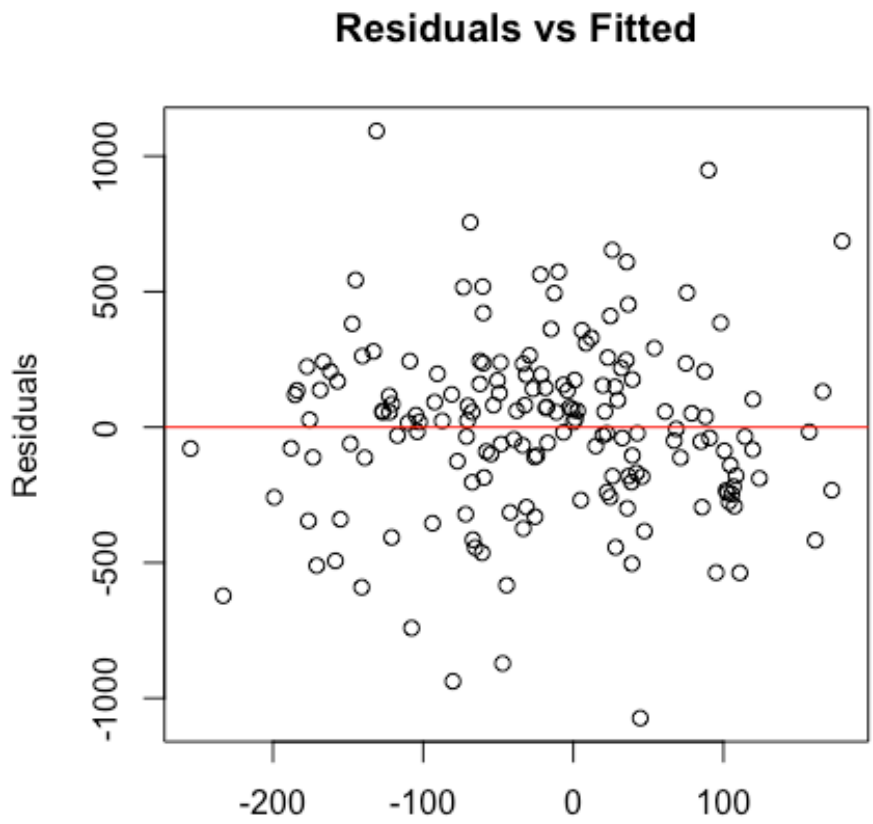


Appendix I

Histogram and Scatter plot – Regression model predicting change in Temporal Binding Post-Election – Pre-Election Experiment 8

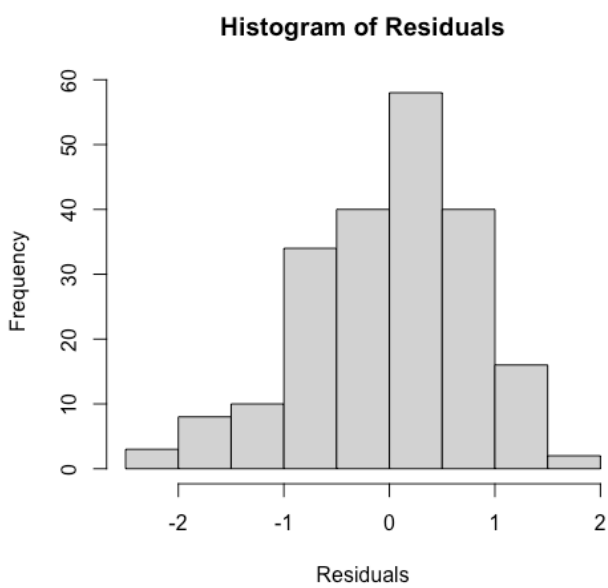


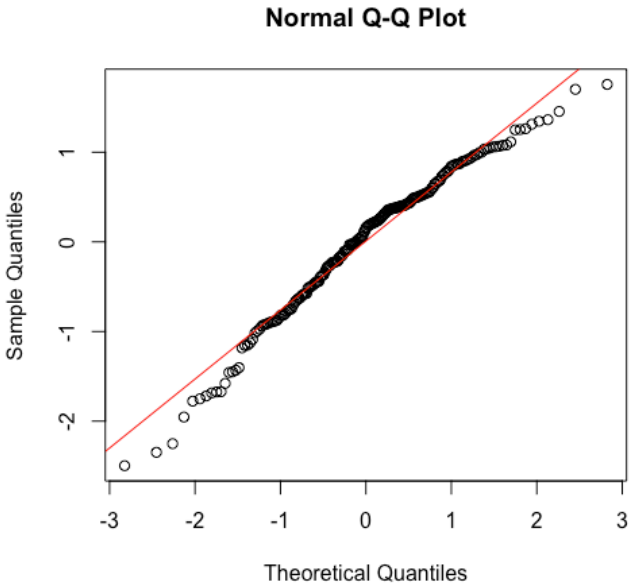


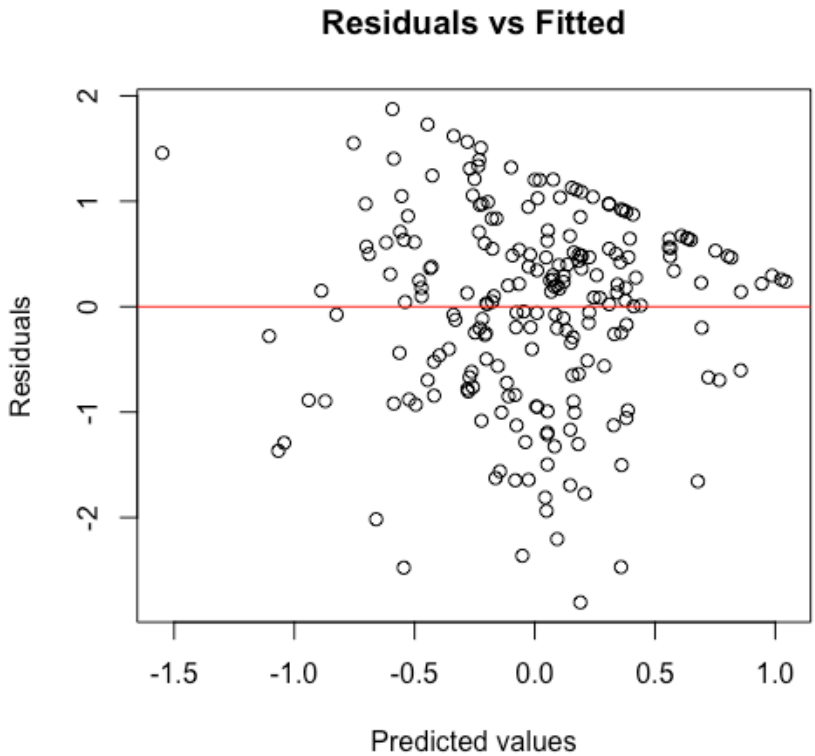


Appendix J

Histogram and Scatter plot – Regression model predicting explicit political efficacy Experiment 8

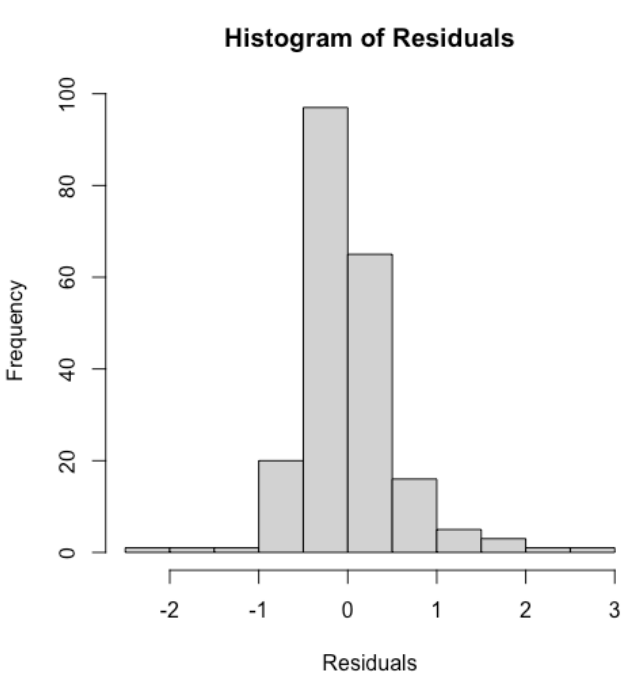


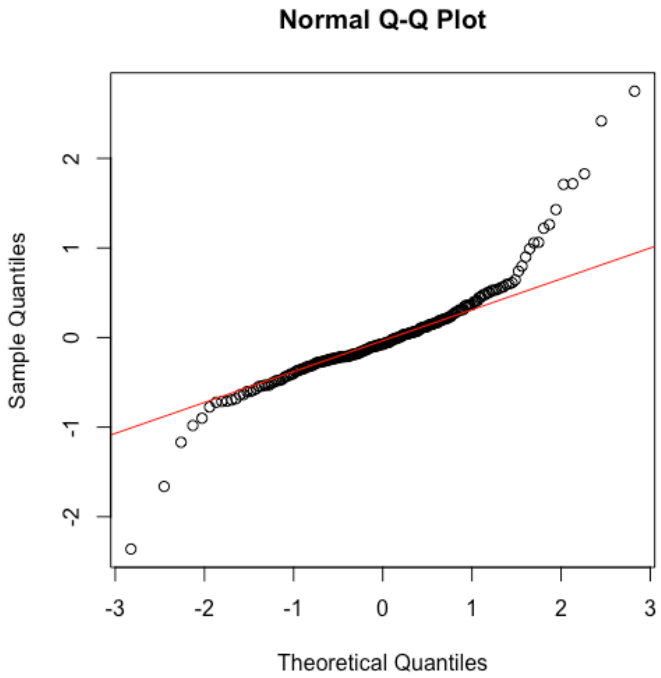


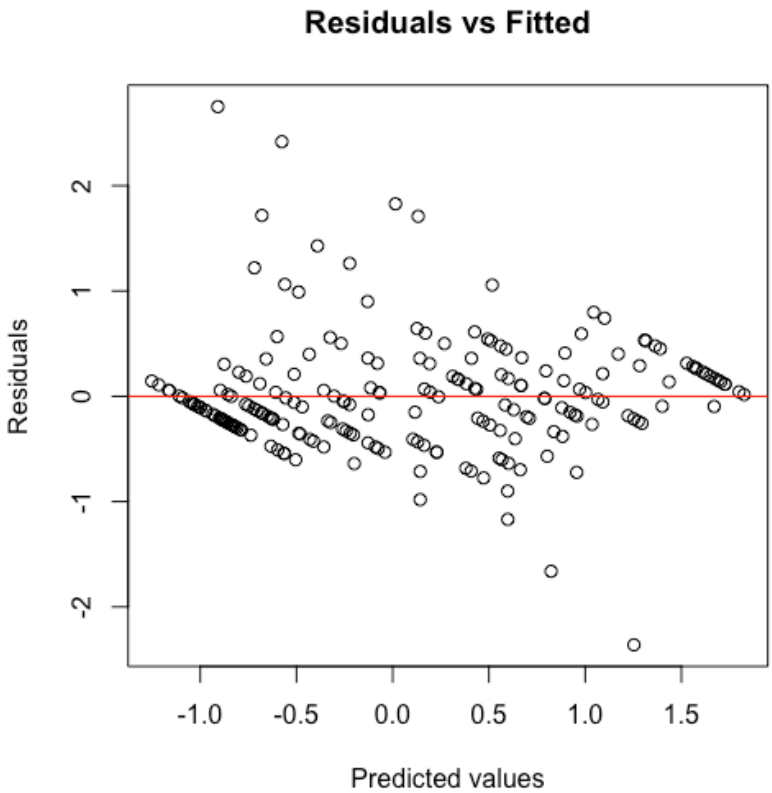


Appendix K

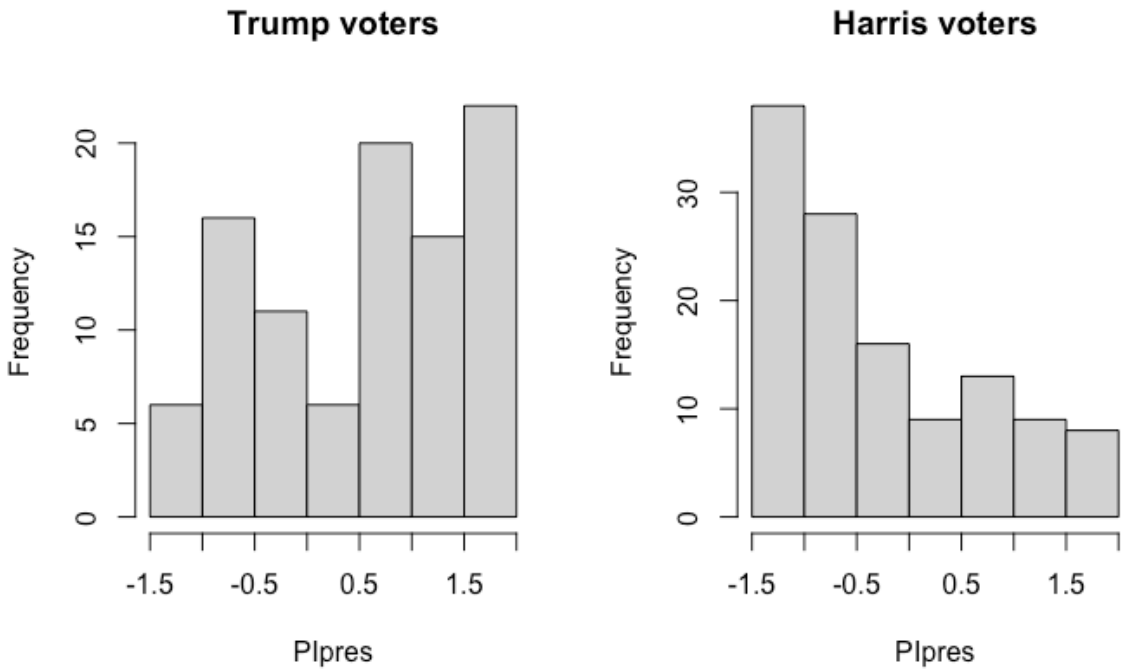
Histogram and Scatter plot – Regression model predicting perceived Influence on Presidential Winner Experiment 8







Note that the histogram of residuals showed concentration at 0. This is likely because of the bimodal distribution of Perceived influence on presidential outcome between those voted for Trump v. Harris. Indeed the histograms of distribution for Perceived influence on presidential outcome showing left skewed for Trump voters whilst right-skewed distribution for Harris voters (PIpres = Perceived influence on presidential outcome):



Since voted candidate had already been added to the model, this would not be a serious violation.

Appendix L
Questionnaires for Experiment 8

Time 1

a. **Political orientation and voting intentions**

Do you identify with a political party? If yes, which one?
- No I don't identify
- Yes, with the Republican Party
- Yes, with the Democratic Party
- Yes, with a different party

Were you eligible to vote in the 2020 Presidential election? If yes, did you vote?
- No, I was not eligible to vote
- Yes, and I did not vote
- Yes, and I did vote

Do you intend to vote in the 2024 Presidential election?
- No
- I'm not sure yet
- Yes
- I have already voted (through early/absentee voting)

If you have not yet voted, how confident are you that you will vote on or before election day?
0 (Not at all confident) – 11 (Completely confident)

If you have voted or intend to vote, which candidate did you/will you vote for?
- Kamala Harris
- Donald Trump
- A different candidate
- I haven't decided yet
- Prefer not to say

Please rate the likelihood of the following outcomes (-5 very unlikely to +5 very likely)

Kamala Harris wins the popular vote in your state
Donald Trump wins the popular vote in your state
Kamala Harris wins the popular vote in the country overall
Donald Trump wins the popular vote in the country overall
Kamala Harris wins the election
Donald Trump wins the election

b. **Political Efficacy**

Please rate your agreement with the following statements (from -5 = strongly disagree to +5 = strongly agree)

Every vote in every state has an impact on the outcome of the election.
If a person cares about the outcome of the election, it's important that they vote.
Every vote has an equal weight in determining the outcome of the election.
Unless you vote in a 'swing' state, it doesn't really matter whether or not you vote at all.
Every vote in my state has an impact on the outcome of the election.
Voting in my state has little or no impact on the outcome of the election.
My vote could have an impact on the outcome of the election.
Whether or not I vote has little or no impact on the outcome of the election.
If/when I vote, it is because I believe it can change the outcome of the election.
My decision to vote or not has little or no effect on who wins the election.

Time 2

Did you vote in the election?

- No
- Yes

Then branch depending on their answer –

If yes:

Who did you vote for?
- Kamala Harris
- Donald Trump
- A different candidate
- Prefer not to say

From 0 (not at all) to 11 (very much)
How much do you feel you personally influenced who was elected President?
How much do you feel you personally influenced the outcome of the Presidential election in your state?
How likely do you think it is that the outcome of the election in your state would have been the same if you had not voted?
How likely do you think it is that someone else would have been elected President if you had not voted?

If no:

If you had voted, who would you have been most likely to vote for?
- Kamala Harris
- Donald Trump
- A different candidate
- Prefer not to say
- There is nobody that I would have considered voting for

From 0 (not at all) to 11 (very much)
How much do you feel you personally influenced who was elected President?
How much do you feel you personally influenced the outcome of the Presidential election in your state?
How likely do you think it is that the outcome of the election in your state would have been the same if you had voted?
How likely do you think it is that someone else would have been elected President if you had voted?

If no:

If you had voted, who would you have been most likely to vote for?
- Kamala Harris
- Donald Trump
- A different candidate
- Prefer not to say
- There is nobody that I would have considered voting for

From 0 (not at all) to 11 (very much)
How much do you feel you personally influenced who was elected President?
How much do you feel you personally influenced the outcome of the Presidential election in your state?
How likely do you think it is that the outcome of the election in your state would have been the same if you had voted?
How likely do you think it is that someone else would have been elected President if you had voted?

Appendix M

Full items of the positive/negative mood questionnaire included in Chapter 4

CARD 34 I will now read out a list of the ways you might have felt or behaved during the past week. Using this card, please tell me how much of the time during the past week…**READ OUT…**

		None or almost none of the time	Some of the time	Most of the time	All or almost all of the time	(Don't know)
E8	… you felt depressed?	1	2	3	4	8
E9	… you felt that everything you did was an effort?	1	2	3	4	8
E10	… your sleep was restless?	1	2	3	4	8
E11	… you were happy?	1	2	3	4	8
E12	… you felt lonely?	1	2	3	4	8
E13	… you enjoyed life?	1	2	3	4	8
E14	… you felt sad?	1	2	3	4	8
E15	… you could not get going[48]?	1	2	3	4	8
	And please tell me how much of the time during the past week…					
E16	… you had a lot of energy?	1	2	3	4	8
E17	… you felt anxious?	1	2	3	4	8
E18	… you felt tired[49]?	1	2	3	4	8
E19	… you were absorbed[50] in what you were doing?	1	2	3	4	8
E20	… you felt calm and peaceful?	1	2	3	4	8
E21	… you felt bored?	1	2	3	4	8
E22	… you felt really rested when you woke up in the morning?	1	2	3	4	8

[48] "Could not get going" in the sense of 'felt lethargic and lacked motivation'.

[49] "felt tired" in the sense of 'fatigued'.

[50] "absorbed" means 'deeply involved'.

The question items above were adapted from the European Social Survey Round 3, Card 34, link: https://www.europeansocialsurvey.org/sites/default/files/2023-06/ESS3_source_main_questionnaire.pdf on page 49 of the documentation. The authors, Huppert et al., (2009) also published the items and analysed in the paper, their appendix too which has identical items:

I will now read out a list of the ways you might have felt or behaved in the past week. Please tell me how much of the time during the past week:
(a) ... you felt depressed
(b) ... you felt that everything you did was an effort
(c) ... your sleep was restless
(d) ... you were happy
(e) ... you felt lonely
(f) ... you enjoyed life
(g) ... you felt sad
(h) ... you could not get going
(i) ... you had a lot of energy
(j) ... you felt anxious
(k) ... you felt tired
(l) ... you were absorbed in what you were doing
(m) ... you felt calm and peaceful
(n) ... you felt bored
(o) ... you felt really rested when you woke up in the morning

[Response code: 1 – None or almost none of the time, to 4 – All or almost all of the time]

(Retrieved from Huppert et al., 2009's appendix)

Here, in the thesis, groupings for each score are as follow:
1. Positive Mood: mean score of items (d), (f), (i), (l), (m), (o)
2. Negative Mood: mean score of items (a), (b), (c), (e), (g), (h), (j), (k), (n)