Step by step:

Sense of agency for complex action-event sequences

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Between black and white there are all kinds of gray.

A yellow villa – Thank you for the relentless support and intellectual stimulation.

A green ambiance – Thank you for nourishing my curiosity and encouraging me to realize my wildest dreams.

A blue fortitude – Thank you for the patience and for always believing in me.

Step by step – Sense of agency for complex action-event sequences

Summary

From simply ringing a bell to preparing a five-course menu, human behavior commonly causes changes in the environment. Such episodes where an agent acts, thereby causing changes in their environment constitute the sense of agency. In this thesis four series of experiments elucidate how the sense of agency is represented in complex action-event sequences, thereby bridging a gap between basic cognitive research and real-life practice. It builds upon extensive research on the sense of agency in unequivocal sequences consisting of single actions and distinct, predominantly auditory, outcomes. Employing implicit as well as explicit measures, the scope is opened up to multi-step sequences.

The experiments show that it is worthwhile devoting more research to complex action-event sequences. With a newly introduced auditory measure (Chapter II), common phenomena such as temporal binding and a decrease in agency ratings following distorted feedback were replicated in multi-step sequences. However, diverging results between traditional implicit and explicit measures call for further inspection. Multisensory integration appears to gain more weight when multiple actions have to be performed to attain a goal leading to more accurate representations of the own actions (Chapter III). Additionally, freedom of choice (Chapter III) as well as early spatial ambiguity altered the perceived timing of outcomes, while late spatial ambiguity (Chapter IV) and the outcome's self-relevance did not (Chapter V). The data suggests that the cognitive system is capable of representing multi-step action-event sequences implicitly and explicitly. Actions and sensory events show a temporal attraction stemming from a bias in the perception of outcomes. Explicit knowledge about causing an event-sequence facilitates neither feelings of control nor taking authorship. The results corroborate current theorizing on the underpinnings of temporal binding and the divergence between traditional implicit and explicit measures of the sense of agency. Promising avenues for further research include structured analyses of how much inferred causality contributes to implicit and explicit measures of agency as well as finding alternative measures to capture conceptual as well as non-conceptual facets of the agency experience with one method.

Zusammenfassung

Vom Läuten einer Klingel bis hin zum Kochen eines Fünf-Gänge Menüs – menschliches Handeln verändert die Umwelt. Situationen, in denen eine Person handelt und so Veränderungen in ihrer Umgebung bewirkt, konstituieren den Sense of Agency. Diese Arbeit präsentiert vier Experimentalreihen, die die Repräsentation des Sense of Agency in komplexen Handlungs-Ereignis-Sequenzen erforschen und so eine Brücke zwischen kognitiver Grundlagenforschung und Alltagspraxis schlagen. Aufbauend auf umfangreicher Forschung zum Sense of Agency in Sequenzen aus einzelnen Handlungen und eindeutigen, vorwiegend auditiven Handlungseffekten wird der Forschungsbereich durch Einsatz impliziter sowie expliziter Maße auf mehrschrittige Sequenzen erweitert.

Mittels eines neuen auditiven Maßes (Kapitel II) wurden gängige Phänomene wie Temporal Binding und die Abnahme von Agency Ratings nach verfremdetem Feedback in mehrschrittigen Sequenzen repliziert. Müssen mehrere Handlungen ausgeführt werden, um ein Ziel zu erreichen, scheint multisensorische Integration stärker ins Gewicht zu fallen, was zu genaueren Repräsentationen der eigenen Handlungen führt (Kapitel III). Darüber hinaus veränderten Wahlfreiheit (Kapitel III) und frühe räumliche Ambiguität das wahrgenommene Timing von Handlungseffekten, späte räumliche Ambiguität (Kapitel IV) sowie Selbstrelevanz des Handlungseffekts taten dies nicht (Kapitel V). Die Daten deuten darauf hin, dass das kognitive System mehrschrittige Handlungs-Ereignis-Sequenzen sowohl implizit als auch explizit repräsentieren kann. Die zeitliche Kompression von Handlungs-Ereignis-Sequenzen ist auf eine Verzerrung der Wahrnehmung von Handlungseffekten zurückzuführen. Explizites Wissen über die Verursachung von Ereignis-Folgen fördert weder Kontrollerleben noch das Gefühl eigener Autorenschaft. Die Ergebnisse bestätigen den derzeitigen Diskurs über die Grundlagen von Temporal Binding und die Divergenz zwischen den traditionellen impliziten und expliziten Maßen des Sense of Agency. Strukturierte Analysen zum Beitrag von Kausalität zu Sense of Agency sowie die Entwicklung alternativer Methoden zur Erfassung konzeptueller wie nicht-konzeptueller Facetten des Sense of Agency mit einem Maß würden zur Weiterentwicklung des Forschungsbereichs beitragen.

I. Cognitive representations of the sense of agency

A distant memory from my days as a Bachelor student at university includes a coffee vending machine in our library. This is where I left a fair bit of money in-between reading up on Pavlovian conditioning and trying to figure out how to conduct a multi-factorial ANOVA. One peculiarity of the machine was that one selected upfront whether and how much sugar and milk the beverage should contain. Sometimes, I would achieve my goal of enjoying a fairly decent coffee. Other times, the brownish liquid would just taste sweet, even though I never take sugar with my coffee. What had happened? Had I pressed the wrong buttons? Had the machine had an internal error? Who was to blame for the failed goal attainment?

At first glance, one might argue that it must have definitively been my fault as there was no one else involved in the coffee making process. Additionally, I had an intention to prepare coffee prior to pressing the buttons. However, the resultant beverage did not meet my expectations. Thus, I might have argued that it was the machine's fault and not mine. This assessment of control over one's own actions and through these actions changes in the environment is called *sense of agency* (Gallagher, 2000; Haggard & Tsakiris, 2009). Back then, I did not engage in extensive contemplations over my sense of agency in such situations, but grudgingly took the sweet beverage. This is arguably the case for most humans. Whether or not we experience sense of agency usually only comes to our awareness in cases of failure of agency (e.g., Chambon & Haggard, 2012). In the following, I will give an introduction on sense of agency, its facets, and underlying mechanisms from a cognitive psychologist's perspective.

1. Sense of agency framework

The sense of agency is many-faceted and has over the years been described and defined in multiple ways depending on the primary discipline of the scholar, e.g., philosophy or psychology[1]. The commonality between all of these definitions is that there has to be some kind of congruence between multiple cues for a sense of agency to arise. Bayne and Pacherie (2007) for example describe the deliberate experience of actions and their consequences as being caused by oneself as agentic awareness. It includes agentive experiences and agentive judgements. In addition to the objective fact that the action was intended, initiated, and executed by the agent, they must also be aware of their own doing to have agentic awareness (Bayne & Pacherie, 2007). A very similar definition is given by Synofzik and Vosgerau (2008) who assume that sense of agency comprises two distinguishable components: feelings of agency and judgements of agency. In recent years, this has become the most prominent description of sense of agency in cognitive psychology and the study of consciousness. Throughout this thesis, I will, thus, draw on Synofzik and Vosgerau's (2008) terminology to refer to the sense of agency and its aspects. According to them, *feelings of agency* entail a non-conceptual and non-verbalizable low-level of agency which resides on a sensory level. These feelings of agency are inaccessible to consciousness and therefore need to be assessed through behavioral or physiological measures which are discussed in more detail below. In contrast, *judgements of agency* comprise more conscious representations and evaluations about the action-effect episode, i.e., the interpretation of being an agent (Synofzik & Vosgerau, 2008). Consequently, such explicit agency judgements can be collected through explicit questions about the participants' perceived agency or designated agency scales (e.g., Metcalfe & Greene, 2007; Polito et al., 2013; Tapal et al., 2017). In the following, I will outline the current state of research regarding feelings and judgements of agency and how these are traditionally assessed. The emphasis lies on feelings of agency and implicit measures of the sense of agency as these are predominantly used throughout this thesis.

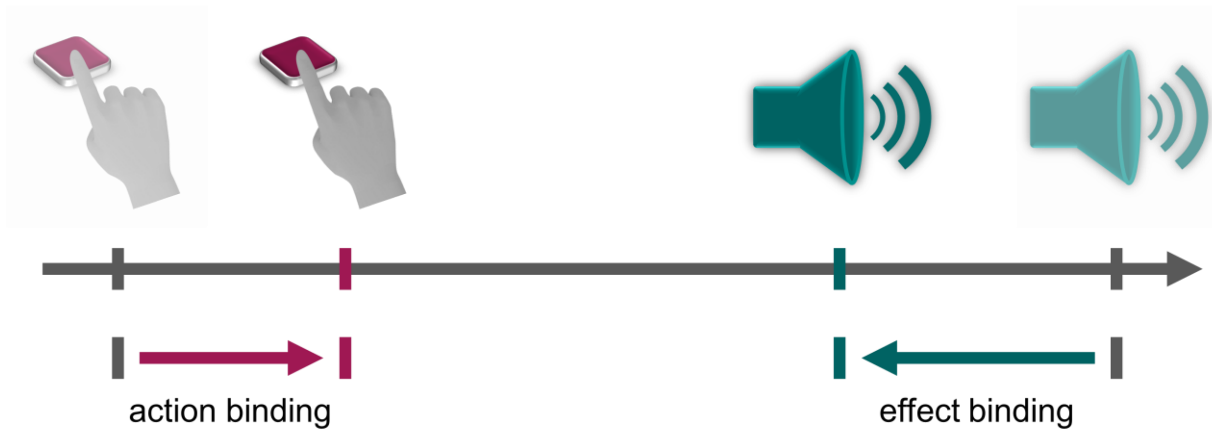
[1] While philosophers often use the term agency to emphasize on the capacity to make free choices and act freely (e.g., Bandura, 1982), cognitive psychology rather focusses on the control of the behavior itself and the resultant changes in the environment (see Haggard, 2017). In this logic, sense of agency requires volitional action (Gallagher, 2007). I will concentrate on this conception of sense of agency throughout this thesis.

1.1. Feelings of agency

Feelings of agency arise from “a multifactorial weighting process of different agency indicators” (Synofzik & Vosgerau, 2008, p. 219) which can stem from motor prediction as proposed by the comparator model (Frith et al., 2000), but also from additional cues such as temporal prediction (G. Hughes et al., 2013) or mere causality (Buehner & Humphreys, 2009). Over the past years, various agency indicators have been discussed and a multitude of measurements developed. The objective to establish an implicit measurement procedure, which is not subject to answer biases or reflective reasoning, brought about multiple instruments, such as temporal binding and sensory attenuation, that all come with their own advantages and disadvantages. Depending on the measurement, the underpinnings of the sense of agency are weighted and interpreted differently. Sensory attenuation describes the phenomenon that self-generated stimuli are perceived as less intensive than identical stimuli that were not elicited by oneself (Blakemore et al., 1998; Gentsch & Schütz-Bosbach, 2011). Over the years, temporal binding has been established as the most prominent implicit measure of feelings of agency. Generally, it describes the phenomenon that the interval between voluntary, self-generated actions and subsequent sensory events is perceived as shortened (Figure 1; for a review see Tanaka et al., 2019). Temporal binding has been studied with a wide variety of tasks including interval estimations (e.g., Cravo et al., 2009; Ebert & Wegner, 2010; G. R. Humphreys & Buehner, 2009; Poonian & Cunnington, 2013), temporal simultaneity ratings (e.g., Arikan et al., 2017; Cravo et al., 2011; Nolden et al., 2012) and interval reproductions (e.g., Gutzeit et al., under review; G. R. Humphreys & Buehner, 2010; Reis et al., 2022). However, to this day, the most widely utilized method to study temporal binding is the Libet clock paradigm (or rotating spot method; e.g. Engbert et al., 2007; Haggard et al., 2002; Klaffehn et al., 2021; Libet et al., 1983) which is based on seminal work conducted by Wilhelm Wundt to study the time course of attention (see Wontorra, 2013). In the initial study, participants judged the position of a rotating clock hand (2560 ms/rotation) at either the time of a voluntary keypress, a transcranial magnetic stimulation (TMS)-induced keypress, or a sine wave tone that occurred in isolation in the baseline conditions and that co-occurred in the operant conditions (Haggard et al., 2002). The debate about the usage of temporal binding as implicit measure for the sense of agency is still ongoing, as more and more studies question whether it really reflects agency (e.g., Kirsch et al., 2019; Reddy, 2022; Saad, 2021; Suzuki et al., 2019). To understand whether it is an appropriate measurement for the sense of agency and how to interpret temporal binding data and results, it is important to understand the mechanisms that underlie temporal binding. I will concentrate on three core mechanisms, namely *motor prediction*, *mere causality*, and *multisensory integration*, postulated to be at the heart of temporal binding to delineate their role in research on the sense of agency.

Figure 1

Schematic depiction of temporal binding



Note. Perceived timepoints of actions in the operant condition in comparison to the baseline condition (grayed-out) are shifted towards the sensory event (action binding). Reported timing of sensory events is shifted towards the action (effect binding).

1.1.1. Motor prediction

Initially, feelings of agency were thought to draw simply on motor prediction and computational models of motor control (e.g., Blakemore et al., 1999; Wolpert, 1997). Such accounts relate to the comparator model where a goal state is defined before a motor program is issued and an efference copy of the to-be-executed motor program is sent forward to a comparator for comparing the actual with the intended state (von Holst & Mittelstaedt, 1950). These comparisons can be used to adjust and correct the executed motor programs on a sensorimotor level (Synofzik & Vosgerau, 2008). Additionally, the signals derived from these comparisons are used as explicit agency cues (Pacherie, 2008). In case of a match between predicted and actual state, no error is detected, and agency is high. Contrary, in case of a mis-match, sense of agency is reduced due to a discrepancy between prediction and actual feedback (Blakemore et al., 1998; Frith et al., 2000). The idea of a forward model's contribution to the sense of agency was backed by studies showing that participants reported their action initiation to have happened 80 - 200 ms prior to the actual movement itself, which is also why temporal binding was initially thought to reflect intentions and thus termed intentional binding (Haggard & Eimer, 1999; Libet, 1985). Additionally, increasing feedback discrepancy, especially through temporal offsets, reduces both agency ratings as well as temporal binding (e.g., Ruess et al., 2017; Sato & Yasuda, 2005; see Baess et al., 2008 for a similar study using sensory attenuation).

However, the comparator model as underlying mechanism of sense of agency has been challenged numerous times (M. S. Christensen & Grünbaum, 2018; Dogge et al., 2019; Synofzik et al., 2013; Synofzik & Vosgerau, 2008; van der Wel et al., 2012; Wong, 2012). Originally, the comparator mode was established to explain the processing of motor signals and their predicted feedback. Researchers have subsequently used it to relate to the pure motor component of sense of agency (e.g., David et al., 2016; Lynn et al., 2014) but also to the more common definition of the experience of causing changes in the environment (Dewey & Knoblich, 2014) or a mix of both (e.g., Caspar, Desantis, et al., 2016; Gallagher, 2000). According to M. S. Christensen and Grünbaum (2018), any experience residing outside the motor realm and the sensory consequences directly related to motor acts is not included in the original idea of the comparator model, or as Wong (2012) puts it: "the sense of agency is beyond comparison" (Wong, 2012, p. 50). Thus, a motor-signal model is proposed. It spares the comparator view as motor commands suffice to explain the control of movement that could then be represented at different levels depending on the to be attained goal (M. S. Christensen & Grünbaum, 2018; Pacherie, 2008).

1.1.2. Mere causality

The debate on whether temporal binding really measures intentions (e.g., Desantis et al., 2012) was kindled by observations such that a causal relation between two external sensory events is sufficient to elicit temporal binding between the two (Buehner, 2015). Buehner (2012) presented participants with pairs of events that could be either an own action and a sensory event or a sensory event caused by a machine. Results showed that intentional actions to elicit a sensory consequence are no prerequisite for temporal binding to occur (but see also Antusch et al., 2020 for a repulsion effect between two causally related external events). Buehner and Humphreys therefore conclude that "[e]vents that are known to be causally related are experienced as closer in time than unrelated events. 'Intentional binding' thus might be a misnomer; the label 'causal binding' might be more appropriate[2]" (2009, p. 1227). In the same vein Suzuki et al. (2019) found temporal binding for temporally perfectly predictable events to match that of self-generated events, thereby supporting a causal binding view rather than an influence of intention. Separating intentionality from causality is also important when it comes to observing other agents. When observing such agents execute intentional actions causing subsequent events, the participants themselves do not have own action intentions, however, temporal binding is still present (Pfister et al., 2014; Sato, 2008; Wohlschläger et al., 2003). Additionally, Desantis et al. (2011) reported that participants showed stronger temporal binding when they believed to have caused sensory events themselves as compared to the same events being caused by someone else.

[2] Just as with intentional binding, the term causal binding contains an interpretation of the phenomenological observation, i.e., a subjective compression of a timeframe. Thus, I refer to this phenomenological observation as temporal binding, as this specifies the observation at hand without interpreting underlying causes. Additionally, the term temporal binding has established as less tainted denomination in the literature.

1.1.3. Multisensory integration

Another competing account seeking to explain typical findings of temporal binding is multisensory integration. It unites ideas of causal views as well as the weighting process suggested by Moore and Fletcher (2012). However, the sense of agency is not at its heart, and it was not developed to explain temporal binding but rather provides an alternative angle to look at the currently employed measurements of sense of agency. As the human organism is constantly overwhelmed with countless sensory information, combining multiple of these to one multisensory event can help to make sense of the environment. Crucially, various information is weighted depending on their relative reliability, which can, amongst others, relate to its intensity (Stein et al., 1996) and perceived timing (Shams et al., 2005). Thus, when participants are asked to judge the timing of individual instances in the multimodal event, their perceived timing for example is influenced by their own relative certainty, but also by the certainty of the other elements in the meta-event (Ernst, 2006; Rohde & Ernst, 2016). This is what is referred to as the reliability-weighting principle (e.g., Ernst & Bülthoff, 2004). Binding or attraction of elements can range on a continuum from complete fusion, i.e., all elements are perceived at the same time, over partial integration to complete segregation. Bringing multisensory integration into agency research, Wolpe et al. (2013) conducted a first study in which they actively manipulated the reliability of the outcome-related signals by varying their intensity relative to the subjects' auditory detection threshold. In line with the idea of multisensory integration, they found the perceived timing of the action to be more biased towards outcome when it was highly reliable compared to less reliable outcomes. In contrast, the perceived timing of the outcome was similarly biased towards the preceding keypress irrespective of the outcome's reliability. The authors concluded that only action binding is based on cue integration, while effect binding is not and, thus, should be studied separately, and not as composite binding (Wolpe et al., 2013). In a more recent study, Klaffehn et al. (2021) systematically varied the certainty of the action (distinct keypress or press on a force sensor) and the sensory event (beep tone or white noise). They again found a stronger influence of the perceptual certainty manipulation on action binding as compared to effect binding. Anecdotal evidence comes also from studies on clinical populations. Patients with corticobasal syndrome (CBS) showed stronger action binding for their alien limb compared to the other one. Wolpe et al. (2014) attribute this difference to lower perceptual certainty for the actions of the alien limb. Consequently, the idea behind multisensory integration that multiple sensory inputs are grouped together to one multimodal event appears principally apt to explain many of the typical findings concerning temporal binding and even causal binding (see also Kirsch et al., 2019). Thus, it seems to be a large contributor to the overall temporal perception bias employed to measure sense of agency implicitly. However, multisensory integration apparently contributes differently to action and effect binding (Klaffehn et al., 2021).

1.2. Judgements of agency

In contrast to the non-conceptual feelings of agency, judgements of agency are "formed as an explicit conceptual, interpretative judgement of being the agent." (Synofzik & Vosgerau, 2008, p. 228) These explicit agency interpretations cannot only use multisensory cues but also explicit expectations and knowledge about causal relations. Judgements of agency are retrospective and rely on a match between prior expectation and actual observation (Wegner, 2002). According to Wegner and Wheatley (1999) three prerequisites have to be met in order to experience a sense of agency: Priority, consistency, and exclusivity (see Figure 2 right side). According to the theory of apparent mental causation, an intention has to exist prior to action execution. Additionally, the experienced output has to be consistent with the initial expectation. And finally, there must not be any other cause for the observed outcome than the agent (Wegner & Wheatley, 1999).

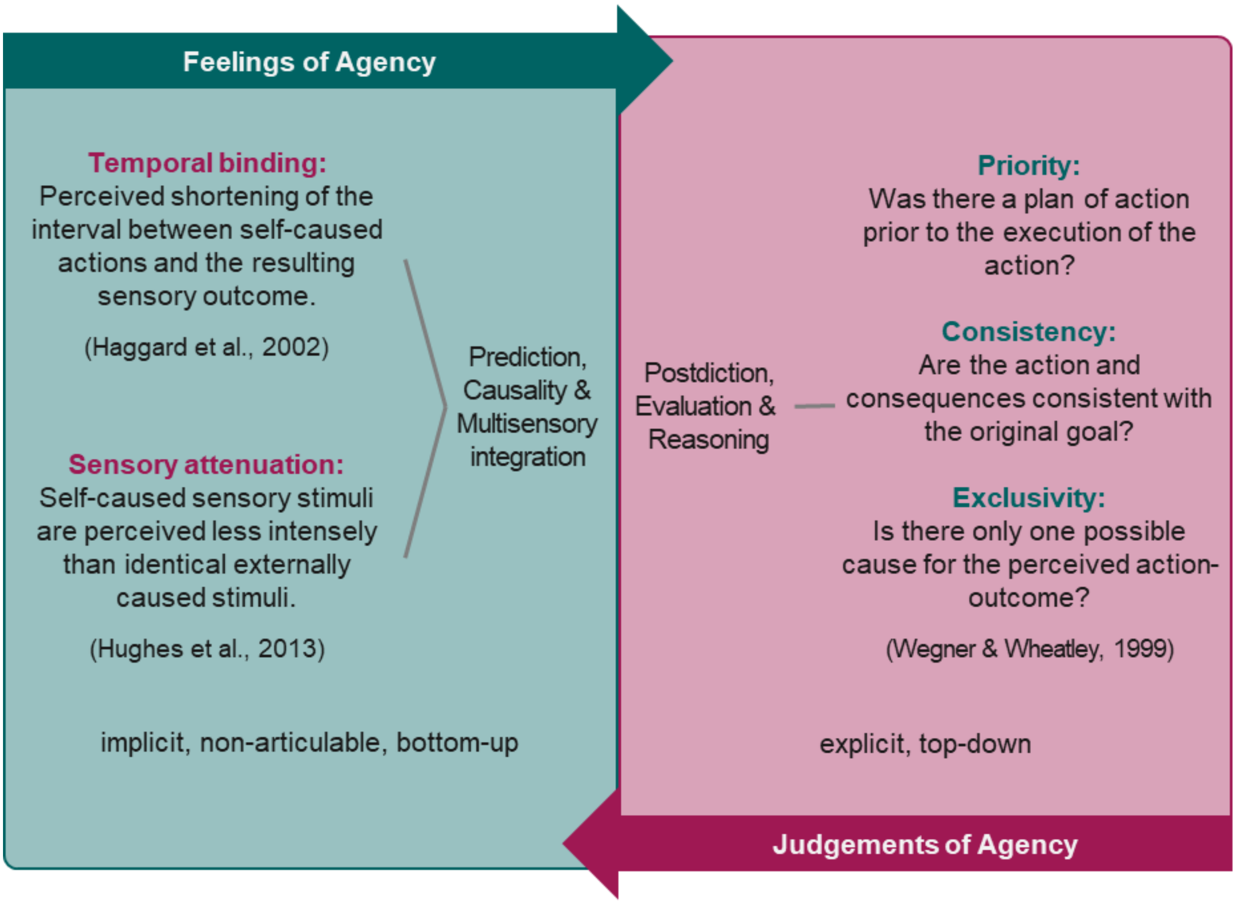
Judgements of agency have been widely used in the study of (un-)willed actions and agency experiences (e.g., Dubynin & Shishkin, 2017; Sidarus & Haggard, 2016). On the one hand, they are a straightforward way to assess and thus an economical measure to study conscious intentions. Typical research targeting judgements of agency does this by directly asking for felt control (e.g., Berberian et al., 2012; Liesner et al., 2020), self-other classifications (e.g., Aarts & van den Bos, 2011; Asai, 2017; Daprati et al., 1997), or more or less elaborate scales assessing multiple aspects of the agentic experience (e.g., Kalckert & Ehrsson, 2012; Polito et al., 2013; Tapal et al., 2017). On the other hand, such direct and explicit measures are often prone to biases and demand characteristics (e.g., Dewey et al., 2010; Moore, 2016). Additionally, humans tend to attribute positive consequences to their own actions, while responsibility for negative outcomes is often externalized. This has been termed the self-serving bias and has been studied extensively predominantly in social psychology (D. T. Miller & Ross, 1975; Shepperd et al., 2008). They are also susceptible to priming such that participants' authorship is enhanced when they are primed with possible outcomes of their own actions (Aarts et al., 2005; for a review see van der Weiden et al., 2013). Additionally, judgements of agency can always only be explicit, retrospective evaluations of past experiences and no online measurement, even though Pacherie (2007) principally distinguishes between an occurrent sense of agency, i.e., the agency experience in the moment, and a long-term sense of agency over a prolonged time period. Thus, asking for the current agency experience potentially reflects other experiences than asking for perceived control over elongated periods of time, still, these reports will always be judgements about the past.

1.3. Interplay of feelings and judgements of agency

As mentioned above, feelings of agency and judgements of agency are not mutually exclusive, rather they are thought to reflect a two-step account of the sense of agency. Agentive experiences cannot be described without an implicit agentic awareness that an own body movement caused changes both in body posture as well as the environment. Oftentimes, this low-level agentic awareness, be it caused by multisensory integration or mere causation, is not enough to paint the whole picture of an agent with long-term goals (see Figure 2). This is especially the case when reasoning about more complex changes. Not least because of this, it is worthwhile considering all aspects that play a role in the formation of sense of agency and studying their interplay.

Figure 2

Sense of agency framework

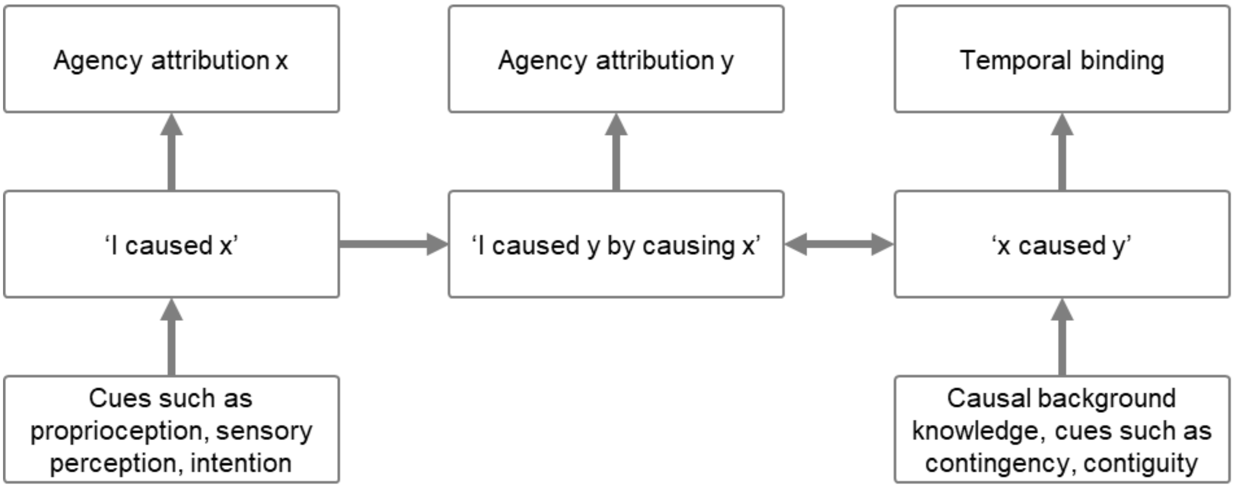


Note. Feelings of agency and judgements of agency contribute to the overall agency experience. Feelings of agency arise (bottom-up) from perceptual representations while judgements of agency are shaped by propositional (top-down) representations. Depending on the context and the task requirements, feelings and judgements of agency contribute to different extents to the overall sense of agency.

In the first two sections of Chapter I, I sketched the foundations of the sense of agency framework including the common distinction between feelings and judgements of agency. Decades of research have brought to light manifold results and help explain how human agents conceive their actions in the environment. Yet, they fail to clarify why feelings of agency and judgements of agency sometimes coincide (Imaizumi & Tanno, 2019) but oftentimes diverge (Garrido-Vásquez & Rock, 2020; Obhi & Hall, 2011; Reis et al., 2022; Schwarz, Weller, Klaffehn, & Pfister, 2019). Based on the idea that the comparator model per se cannot explain either feelings of agency or judgements of agency, Synofzik and Vosgerau (2008) proposed a two-step account of agency. Here, feelings of agency arise bottom-up from purely perceptual representations and they are not articulable, while judgements of agency are the product of postdictive reasoning that is influenced by top-down processes such as explicit knowledge about causal relations. Feelings of agency occur before they are further processed to finally result in agency attributions or not (Synofzik & Vosgerau, 2008). Such a two-step account might even explain how the sense of agency evolves over time. As not all cues are available simultaneously, different information has to be integrated as it becomes available (Pacherie, 2015). Postdictive processes guided by additional information such as task performance can even trump feelings of agency when it comes to explicit agency attributions (e.g., Inoue et al., 2017; Wen et al., 2015b). Observations like this as well as disturbances in the sense of agency in schizophrenic patients inspired Moore et al. (2009) to propose a cue integration approach to the sense of agency. Similar to the multisensory integration account outlined above, here multiple cues arising from both sensory signals as well as prior information such as explicit knowledge are integrated based on Bayesian principles. Depending on the reliability of these cues, their contribution to sense of agency varies. As this model was developed to directly target sense of agency, it allows for inferences about agency experiences. Finally, the cue integration approach states that all other factors, e.g., prior beliefs, being equal weaker temporal binding indicates a decrease in the sense of agency. However, this is only the case, if this implicit agency marker is not outweighed by other cues (Moore & Fletcher, 2012).

Figure 3

Theoretical framework to explain temporal binding as proposed by Hoerl et al. (2020)



Note. Influences on feelings and judgements of agency, explaining how temporal binding can arise both with and without voluntary action. From "Temporal Binding, Causation, and Agency: Developing a New Theoretical Framework", by C. Hoerl et al., 2020, Cognitive Science, 44(5), p. 15 (https://doi.org/10.1111/cogs.12843) Copyright © 2020 by the authors. Reproduced with permission from John Wiley and Sons.

Prior knowledge and external cues play a crucial role in a recently developed framework trying to integrate feelings of agency and judgements of agency (Hoerl et al., 2020; see Figure 3). While they base their framework on the assumption that objective causality and subjective causality judgement underlies all agentive attributions, they set out to explain how temporal binding can arise in both voluntary as well as involuntary actions (Hoerl et al., 2020). In line with previous studies showing that voluntary actions are no prerequisite for temporal binding (e.g., Kirsch et al., 2019; Thanopoulos et al., 2018), the framework suggests that causal information such as contiguity and contingency suffice to evoke a subjective compression of the interval between two events, on the one hand. This results in causality statements such as "the keypress evoked the tone (because it followed shortly after)" (see Cheng, 1997 for a causal power theory). On the other hand, motor sensory cues and knowledge about intentions are used to make active inferences about causal relations in the environment expressed as agentive attributions such as "I pressed the key (because I intended to do so)". When both come together the causal chain is completed and an agentive attribution can also be inferred for the tone as “I caused the tone by pressing the key”. Here, the agentive attributions would be subsumed under judgements of agency and the causality statement would be considered an expression of feelings of agency. This framework also accounts for the interplay and occasional discrepancy between feelings of agency and judgements of agency, as both can arise independently but affect each other (Hoerl et al., 2020). To put it simply, feelings of agency represent the *how* side of agentive experiences while judgements of agency reside on the *what* side. Ultimately, different agency cues occur at different stages of the action-event sequence and thus have to be integrated into the agency experience as it unfolds. This is not trivial, as most everyday action-event sequences continue over a prolonged period. Thus, it is worthwhile studying how the sense of agency evolves over time, especially in more complex action-event sequences. Which factors contribute to what extent to daily agency experiences? Can implicit and explicit measures of the sense of agency capture the complexity? In this section I have laid out three different conceptions of the relation between feelings of agency and judgements of agency. First, the unidirectional relation assumes that feelings of agency are antecedents of judgements of agency. According to this view, judgments of agency arise from feelings of agency, while they both contribute to the overall sense of agency depending on contextual cues and demands (Synofzik & Vosgerau, 2008). Second, the bidirectional relation accounts for influences of feelings of agency on judgments of agency and vice versa (e.g., Hoerl et al., 2020). Variations in one of the two can cause changes in the other. Third, the parallel relation considers feelings of agency and judgements of agency as two completely distinct and detached processes (Dewey & Knoblich, 2014; Saito et al., 2015). For this view the critique of integration of feelings of agency and judgements of agency as two sides of a unique agency experience is strongest.

2. Complex action-event sequences

Psychological research, especially in cognitive psychology, is often criticized because its paradigms are too artificial and its research questions only mildly resemble real-life problems (for a discussion see Berkowitz & Donnerstein, 1982). Especially with regard to the sense of agency, simply studying single action-outcome links seems to cut the problem short. When I refer to such single action-event sequences, I mean instances where exactly one action is required to elicit one causally linked change in the environment. Pressing a key to elicit a tone might transfer to ringing a doorbell, but as soon as the action becomes more complex, single action-event links fall short of many aspects of these more complex behaviors. In real life, actions are often directed at more general overarching goals or multiple actions need to be executed for goal attainment.

As stated above, agentive experiences usually comprise more complex sequences and oftentimes do not even reach our conscious awareness (Bayne & Pacherie, 2007; Chambon & Haggard, 2012). Imagine for example withdrawing cash from an ATM and placing your hand over the keypad to occlude the view for bystanders. Such behavior causes an action sequence for which the final results, i.e., cash or an error message, only becomes known after the final keypress. Ultimately, everyday queries and problems have to be broken down and simplified to make it feasible for them to be examined experimentally. In the following, I will elaborate on such more complex sequences of actions and events to point out the relevance of studying perceived agency in more realistic sequences in addition to single action-outcome episodes.

2.1. Level of representation

Let us think back to the introductory example of getting a coffee from an outdated vending machine in the library. What and when exactly is it that I get the feeling of being responsible for the coffee and in control over the machine's working?

The act of making a coffee can be represented at different levels that entail different forms of intentions. What is usually referred to as intention in common speech is what Pacherie

(2008) terms a distal (D-)intention. It is the general desire to reach a certain goal without having a specific plan on how to attain it, for example "I want to get a coffee". When digging deeper into this D-intention then arises the action plan which is contained in the proximal (P-)intention. At this level, conceptual information about the abstract goal is integrated with perceptual information about the current environment and state of the body, for example, to form a more concrete representation of the action to be executed. That is, "to get the coffee, I have to select the coffee and the sugar level by pressing the respective buttons on the machine". Finally, the most fine-grained form of intention is the motor (M-)intention. The motor intention is in charge of setting and fine tuning the motor programs before and during action execution. An illustration of an M-intention could be "to press the key, I have to apply this much force and contract muscles x and y this specific way". M-intentions can typically not be formulated as they operate on a purely motor level. These representational levels of intentions interact in a top-down fashion so that D-intentions are split into P-intentions which in turn are represented in more fine-grained M-intentions that might be formed and altered as the agent acts (Pacherie, 2008). In Chapter III, I discuss some of the implications that come with these multiple levels of representation when studying sense of agency in multi-step action-event sequences.

In a first attempt to scrutinize the sense of agency for D- and P-intentions, Vinding et al. (2013) found stronger effect binding for tones that resulted from actions which were planned longer in advance than actions that were executed when participants felt the urge to act. In a follow-up study, the authors reported a categorical shift in temporal binding between D- and P-intentions, indicating that there is a qualitative difference between instantaneous action-event sequences and pre-planned ones (Vinding et al., 2015). One undeniable critique of these studies is that D-intentions were operationalized as having a maximum of five seconds which is clearly not the same as forming an intention to have a coffee soon. Thus, considering the different levels of representation, it seems legitimate to ask whether it is even possible to measure agency aspects resulting from explicit D-intentions implicitly or sense of agency for M-intentions, that cannot even be verbalized, explicitly. However, such a differential classification of intentions and motor execution potentially allows to explain phenomena of illusory, aberrant sense of agency and the widely discussed discrepancy between feelings and judgements of agency.

2.2. Action sequences and action grouping

When studying the sense of agency in complex multi-step action-event sequences, it is inevitable to consider how humans perform such rather complex actions. The simple act of drinking from a cup of coffee for example involves complex processes for the coordination of the contraction of arm and finger muscles to grip the cup and guide the arm's trajectory (Flanagan & Wing, 1997). In such action sequences, humans typically exhibit the end-state comfort effect, i.e., end their movement in the most comfortable posture (Rosenbaum & Jorgensen, 1992). The original idea of the end-state comfort effect has since been revised. When humans have a certain goal in mind, they anticipate the action sequences as well as their motor behavior to have optimal control over their extremities when it is needed most. If participants need more precision at the beginning of the movement, they put up with less comfortable end-states (Herbort & Kunde, 2019). Taken together, this body of research shows that participants have the end state of their action in mind when initiating it. This has been shown extensively in studies examining grip posture, e.g., when lifting or turning objects (e.g., C. M. L. Hughes et al., 2012; Mathew et al., 2017; Short & Cauraugh, 1999). Consequently, I argue with Rosenbaum et al. (1992) that planning of behavior reaches beyond the immediate feedback and that people principally plan more than one action in advance. Depending on the position in the sequence at which precision is needed, the scope of action planning can stretch further than three upcoming states of the effector (Haggard, 1998).

While the action sequences used in these paradigms correspond to triggered sequences (see Delevoye-Turrell et al., 2007), the results suggest that humans tend to act economically to reach a desired goal state. If this is true for sequences in which sensory feedback marks the completion of one episode and thereby triggers the next (triggered sequences), such saving of resources can probably also be assumed for planned sequences. In sequences where multiple movements are performed within a sub second interval, humans plan the complete sequence prior to movement execution (Kennerley et al., 2004). Such action chunking again reflects an economic perspective on motor behavior, where not every single motor command is represented individually, but rather as part of a cluster of motor commands. The execution of action sequences consists of several subcomponents where the elaboration of the specific timepoint for a movement initiation precedes the concrete planning of the movement structure itself (Bortoletto et al., 2011; Bortoletto & Cunnington, 2010). Additional evidence from task switching paradigms suggests that even when humans can freely choose whether to perform two separate tasks in succession (blocked) or interleaved, a substantial number of participants chooses to preplan the reactions to both tasks and then execute them in short succession. These participants are referred to as response groupers and they exhibit consistent grouping behavior across blocks differing in task demand (Brüning et al., 2021; Reissland & Manzey, 2016).

In contrast to the studies reported above that focused primarily on the timing of motor output and its planning, e.g., by analyzing reaction times and readiness potentials prior to movements, research has shown that performing multiple actions also influences time perception. When participants had to continue a timed sequence by tapping their finger, perceived duration of visual stimuli oscillated, i.e., apparent time dilated between two movements while it compressed when nearing any of the two taps (Tomassini et al., 2018). To add to this, it is not only the number of movements that has an impact on time perception. For example the duration of an action biases the perceived duration of the subsequent action-effect such that tones following longer movements are also judged to be longer than the same tones following shorter movements (Yon et al., 2017). Temporal distortion was also observed during hand movements in such a way that they were accompanied by a compression in time perception (Tomassini et al., 2014; Yokosaka et al., 2015).

3. The case of agency in action-event sequences

Against the backdrop of the aforementioned observations, it is interesting that little research has investigated the relationship between complex action-event sequences and the sense of agency so far. However, as Pacherie (2015) put it, “the picture is likely to become more complicated when we consider complex, extended-action sequences” (Pacherie, 2015, p. 15). When the cause of an outcome becomes ambiguous, boundary conditions have to be met for a sense of agency to arise (van der Weiden et al., 2013). The reasons for causal ambiguity are manifold. In social situations, for example, when more than one agent has control over a manipulandum, own causality might be blurred, or when external (unintended) events coincide with the agent’s action. Then boundary conditions include the perception of a causal relationship between action and outcome as well as an explicit representation of the own behavior as to how and why the action was performed (van der Weiden et al., 2013; see also Vallacher & Wegner, 1987). First studies have tried approaching sense of agency in complex action-event sequences for multiple actions (Garrido-Vásquez & Rock, 2020), multiple outcomes (Ruess, Thomaschke, Haering, et al., 2018) or both (Imaizumi et al., 2019; Yabe et al., 2017) with mixed results.

On the action-side, Garrido-Vásquez and Rock (2020) manipulated whether one or two keypresses were needed to produce an action-effect. They collected both explicit agency ratings and sensory attenuation of the outcome as implicit agency measure. As in many other studies, the two measures diverged. While sensory attenuation was stronger for tones which were produced by two keypresses rather than one, explicit agency ratings reflected the opposite. They were higher when one keypress sufficed to produce an outcome (Garrido-Vásquez & Rock, 2020). These diverging results seem to be in line with the underlying principles. While performing multiple keypresses will increase temporal resolution and predictability of the upcoming event, and thereby increase sensory attenuation, having to press a key twice to bring about change in the environment plausibly reduces inferred causality. Imagine pressing a doorbell and then having to press again because there was no sound at the first keypress. This might cast doubt whether the bell that eventually sounded was really provoked by the own keypress or something else.

On the outcome-side, Ruess, Thomaschke, Haering, et al. (2018) conducted a classical temporal binding task using the rotating spot method. Instead of presenting only one sensory event as action consequence, participants hear a second, task-irrelevant tone after the initial action-effect. In line with the idea of multisensory integration and even the causality approach – given the first tone is construed as causing the second – both first and second tone were perceived to have happened earlier when preceded by an action compared to a condition with no prior action, i.e., in operant compared to baseline blocks. However, this temporal shift was only observed when the second tone followed the first with a predictable and constant delay and it decreased with increasing delay between the tones (Ruess, Thomaschke, Haering, et al., 2018).

The two other studies scrutinized temporal binding for interleaved actions and events with the Libet clock and interval estimations. Yabe et al. (2017) studied effect binding either in action-event dyads or in triplets comprised of either two actions and one event or one action flanked by two events. When a sensory event, i.e., a tone, preceded the action, the perceived timing of these tones was shifted to a later point in time, while it was shifted to an earlier point in time when the tone followed the action. That is, the perceived timing of both sensory events was shifted towards the single action in case of event-action-event triplets. Interestingly, in case of action-event-action sequences, there was no systematic shift of the sensory event neither to the preceding nor to the ensuing action. The authors thus conclude that temporal binding in complex action-event sequences is in essence a concatenation of single action-event links where actions constitute indispensable anchors for binding sensory events (Yabe et al., 2017; for an alternative explanation based on attentional processes see Schwarz & Weller, under review). If this is the case, longer sequences consisting of multiple actions interleaved with multiple sensory events should be perceived as shortened. Precisely this is what Imaizumi et al. (2019) report. In their study participants encountered keypress-tone alterations that could either be caused by voluntary actions or passive finger movements and they were asked to reproduce either the timing of one of the dyads within this sequence or the duration of the entire sequence. Counter to the expected compression of action-tone-intervals, participants judged the duration of single action-event-intervals to be longer (local dilation) while the overall length of the sequence was perceived as shortened (global compression) (Imaizumi et al., 2019). It appears inaccurate to directly compare the time reproduction of long and filled intervals with that of short unfilled intervals. Especially as participants seemed to reproduce similar intervals for all three short delays. In line with this, previous research has shown a regression to the mean, i.e., that long intervals are rather underestimated while shorter intervals tend to be overestimated (e.g., G. R.

Humphreys & Buehner, 2009; Reddy, 2022; Reis et al., 2022). The same accounts for unfilled compared to filled intervals (Bangert et al., 2019). While the results outlined above might be an artefact of the temporal reproduction method, it nevertheless shows that actions and more specifically multiple actions in succession have an impact on time perception (De Kock et al., 2021; Hagura et al., 2012; Wiener et al., 2019). Consequently, mixed findings call for a systematic and theory-driven approach to studying sense of agency in complex action-event sequences.

In light of the inconclusive and in parts contradictory results, I aim to answer key questions as to which factors influence implicit and explicit measures of the sense of agency in multi-step action-event sequences throughout this thesis. In Chapter II, I introduce an alternative to the Libet clock method for *evaluating when* events are perceived in visually more demanding tasks. Here, an auditory timer is utilized as time referent resulting in reduced visual load during task execution. With the auditory timer, typical temporal binding results were replicated, making it a suitable alternative measure. This method is subsequently employed in Chapter III and IV to measure temporal binding for action-event sequences of two actions followed by two visual outcomes. Does *choosing how* a goal is reached have an influence on sense of agency? Preceding studies suggest that it increases with the number of available choice options compared to forced choice. Once a goal and the path towards this goal have been selected, the path to goal attainment can be tracked. Goal attainment is sometimes imperfect, thus the question whether *knowing where* a sensory consequence occurs affects the sense of agency. Does spatial ambiguity have an influence on the sense of agency? Here, implicit and explicit measures of the sense of agency are contrasted and diverging results discussed. Finally, action outcomes can be of varying self-relevance for the agent. Thus, Chapter V assesses temporal binding with regard to *processing what* the identity of an action-effect is. Does self-relevance of action effects influence temporal binding in both single action-event sequences as well as two-step sequences? Contrasting previous studies, I contest the assumption that self-relevant action effects increase temporal binding as implicit measure of sense of agency. To conclude, Chapter VI provides a theoretical integration of the empirical findings into the current debate. Additionally, it critically reflects on the current body of literature and points out avenues for further research.

II. Empirical synopsis: Evaluating when

4. Temporal binding past the Libet clock: Testing design factors for an auditory timer

Voluntary actions and causally linked sensory stimuli are perceived to be shifted towards each other in time. This so-called temporal binding is commonly assessed in paradigms using the Libet Clock. In such experiments, participants have to estimate the timing of performed actions or ensuing sensory stimuli (usually tones) by means of a rotating clock hand presented on screen. The aforementioned task setup is however ill-suited for many conceivable setups, especially when they involve visual effects. To address this shortcoming, the line of research presented here establishes an alternative measure for temporal binding by using a sequence of timed sounds. This method uses an auditory timer, a sequence of letters presented during task execution, which serve as anchors for temporal judgements. In four experiments, we manipulated four design factors of this auditory timer, namely interval length, interval filling, sequence predictability, and sequence length, to determine the most effective and economic method for measuring temporal binding with an auditory timer.

4.1. Introduction

Opening an app on an outdated smartphone typically comes with a slight and sometimes barely noticeable time interval between tapping the screen and opening of the app. However, the perceived time interval between tap and the presentation of the app's content is shortened. More precisely, when the tap opens the app, the tap is judged to occur later, and the app is judged to flash earlier as compared to situations, where there is only a tap or only a flashing of an app. This so-called temporal binding phenomenon (also referred to as intentional binding) is widely employed in research on voluntary actions and their subsequent effects (Haggard et al., 2002; Moore & Obhi, 2012). It describes the finding that an action and a causally linked sensory event are perceptually shifted towards each other in time, as compared to either of the events happening in isolation. That is, if you tapped on the icon on your smartphone but the app did not open, you would have a more accurate temporal estimate of your action than when the app actually opened (though prediction of what will happen induces a small shift in perceived action time as well, Moore & Haggard, 2008). Likewise, if you watched your screen and an app would open without your involvement, you would have a more accurate estimate of the time the app opened compared to when you actively pressed an icon to open the app.

Due to the lack of explicit awareness of such perceptual shifts, temporal binding is an implicit measure for the sense of agency, i.e. the conception of the self as being responsible for our actions and through these changes in the environment (Haggard & Tsakiris, 2009; Moore, 2016; Tsakiris & Haggard, 2003). This sense of agency is informed by predictive and retrospective processes that reflect peoples' feelings of agency and peoples' judgements of agency respectively (Sidarus et al., 2017; Synofzik et al., 2013). Temporal binding, which is sensitive to intentions but does not require explicit reflections regarding agency, is supposed to reflect predictive processes based on the agent's internal sensorimotor models (Synofzik & Vosgerau, 2008). Contrary, G. Hughes et al. (2013) argue that temporal binding is rather driven by temporal expectancy and not intentional causation.

Beyond the fact that temporal binding is sensitive to intentions and thus often referred to as intentional binding (e.g. Haggard & Tsakiris, 2009; Moore & Obhi, 2012), it has been shown that temporal binding is also informed by causality which is why intentions are no necessary perquisite for it to arise (Buehner, 2012; Suzuki et al., 2019). It is a widely employed measure for time estimations in both healthy participants as well as clinical populations, such as schizophrenia patients and patients with Parkinson's disease (Buehner & Humphreys, 2009; Haggard et al., 2003; Kirsch et al., 2019; Moore et al., 2010). Despite the common use of temporal binding as a measure, as of yet there are not many ways of studying it. Temporal binding is commonly assessed with two paradigms: Interval estimation and the Libet Clock (Engbert et al., 2007; Tanaka et al., 2019). They are both based on the phenomenon that the perceived interval between voluntary self-generated actions and causally linked sensory events is shortened. However, the major difference is that in studies employing the interval estimation method, participants have to estimate the length of the interval between action and effect, while with the Libet clock both the timing of the action as well as the timing of the effect have to be estimated independently.

In studies using the Libet Clock participants have to estimate the timing of their actions and subsequent events by means of a so-called Libet Clock, which is presented on screen. This clock is designed in a way that a full rotation of the clock hand takes about 2560ms rather than 60 seconds. During the experiments, participants view the rotating clock hand while performing voluntary button presses and experiencing their effects (usually sounds). Subsequently, they report the position of the clock hand at specific occurrences. These occurrences are either the participants' actions or the ensuing effects (for more details see Figure 4; Libet et al., 1983; Ruess et al., 2017). Results show that voluntary actions are systematically perceived as having happened later, shifted towards the effect, when occurring in combination with a sensory event compared to when occurring in isolation (action binding). The same accounts for time estimations of effects following voluntary actions. Subsequent to self-generated actions, effects are judged to have occurred earlier, shifted towards the action, as compared to effects that happened in isolation (effect binding). Consequently, the interval estimation method can only make inferences about the overall binding while the other method is capable of disentangling action binding and effect binding.

However, the use of the Libet Clock has several limitations as well. Pockett and Miller (2007) focused on different factors which might influence results obtained with this method. The authors emphasize that instructions whether to report the onset or end of the own movement influence participants' estimations. They also suggest that the luminance of the clock hand as well as its size might have an influence on the effects found. Additionally, tasks employing the Libet Clock are visually demanding, as participants have to follow the clock hand with their eyes to make their temporal judgements accurately. Thus, the setup is ill-suited for many conceivable settings, especially when they involve tasks with visual effects.

To reduce the task's inherent visual load and to introduce more flexibility in the experimental task, Cornelio Martinez et al. (2018) proposed an "auditory Libet Clock". This method uses spoken letters, which are presented over headphones rather than the visual clock hand to determine the perceived timing of the actions or events. To the best of our knowledge, at this point, this is still the first study using an auditory timer to measure temporal binding, and the obtained results remain to be replicated and extended. Thus, a thorough and reliable approach to systematically studying temporal binding by means of an auditory timer is required. The seemingly trivial setup of timed auditory cues has various obvious and less obvious design factors that might affect experimental results and the overall aptness of the method. In this line of research, we varied four design factors that we consider most important and substantial for the design of an auditory measure for temporal binding. Therefore, we systematically manipulated the factors *interval length*, *interval filling*, *sequence predictability*, and *sequence length* of an auditory timer to study temporal binding in a task with visual effects.

First, interval length, that is the length (duration) of the presented letters, is of utmost importance, as it determines the temporal resolution of the timed auditory stimuli. The shorter the interval, the higher the resolution, however, this resolution gain can come at the cost of discernibility of the individual letters. Hence, we ask: *What is the optimal interval length?*

Second, interval filling also plays an important role in the configuration of an auditory timer, as it contributes to its temporal resolution. Additionally, it also provides anchors for temporal estimations. Previously and subsequently used letters can be used as temporal cues and therefore serve as anchors for participants' estimations. The salience of these anchors varies with the filling of the interval. Finally, filling time intervals with auditory stimulation can potentially increase the accuracy of duration estimation (Rammsayer & Lima, 1991). Thus, we seek to answer the question: *How should intervals be filled?*

Third, predictability of the letter sequence appears to be an important factor, as it might influence participants' estimation strategies. With decreasing sequence predictability, participants might focus more on auditory anchors while relying less on strategies (e.g., always acting on the same auditory cue). Thus, we ask: *Should the sequence of auditory cues be predictable?*

Ultimately, the amount of letters that constitutes the auditory scale most likely has an influence on participants' task load. With increasing length of the letter sequence, it should become more difficult to remember it and therefore draw more cognitive resources. Therefore, we aim at answering the question: *What is the optimal amount of auditory cues?*

The presented experiments introduce a thorough, theory driven approach to establishing an auditory timer for measuring temporal binding. Within this context, the four aforementioned factors are systematically manipulated in successive experiments to find the most suitable timing configuration. All experiments were preregistered on the Open Science Framework and were approved by the Ethics Committee of the psychological department of the Julius-Maximilians University Würzburg (GZ 2019-09). All raw data and analysis scripts are available at the project repository (https://osf.io/d3vz5/).

4.2. Experiment 1: Manipulation of interval length

Experiment 1 tested for the ideal presentation length of letters that constitute the auditory timer for measuring temporal binding. This is what we will refer to as interval length. Letters were either 250 ms, 500 ms, or 750 ms long (for more detail see Apparatus and stimuli). According to the study by Cornelio Martinez et al. (2018) we expected to find temporal binding in the

250 ms condition. Additionally, we were interested to find out, how variations in the interval length influence temporal binding as an objective measure. As manipulation check, both action binding as well as effect binding should be similar to both types of binding found in previous studies using the Libet Clock. Additionally, we collected participants' perceived task load in order to determine whether there are differences in the subjective quality of the auditory timer depending on the interval lengths.

4.2.1. Methods

Participants. 48 participants (11 male, 8 left handed, mean age = 24.1, *SD* = 6.3) recruited over the university's participant pool (SONA) took part in the experiment. Prior to data collection a power analysis for paired sample t-tests was performed using G*Power 3.1 (Faul et al., 2009). Previous studies found medium effect sizes for action binding (Ruess et al., 2017), thus, we conducted the power analysis with $d = 0.40$, $\alpha = .05$. With these parameters, a sample size of 41 would have sufficed to ensure high power (.80). However, in order to counterbalance the conditions, we set the sample size to 48. Prior to the experiment, participants signed an informed consent form and they received either monetary compensation or partial course credit for their voluntary participation. All participants were naïve to the purpose of the study and were debriefed afterwards.

Apparatus and stimuli. *Visual effect task* | The visual effect task was a single choice task with a visual effect, i.e., the movement of a cursor. It was completed on an iPad 2 which participants operated with the index finger of their right hand. The iPad's LED screen, with a 9.7'' diagonal and a resolution of 1024 x 768 px, was used in landscape mode. Compared to normal keyboards, a touch device gives the user more unambiguous feedback as to when the finger touched the surface. In contrast, with a standard keyboard, there are at least two events that might shape the experienced point in time of that keypress, namely the point in time the finger hit the key, and the point in time the key was completely pressed. Additionally, this addresses the pitfall of other sensory input such as clicking sounds elicited by the keypress that usually accompanies the use of computer keyboards. Thus, touchscreen devices seem to be suitable to study temporal binding[3]. During the experiment, a 3 × 3 grid of circles with a diameter of 100 px was presented on the left half of the screen (see Figure 4). Next to the grid on the right, was a keypad with eight spatially arranged arrow keys, each of which measured 100 × 100 px. At trial onset, the center circle (start area) was filled in blue (to illustrate a moveable cursor) and displayed the German word for start (“Start”). Simultaneously, one of the other eight circles in the grid displayed the German word for goal (“Ziel”) and was connected to the start area with a straight orange line. The goal location indicated which keypress participants had to perform.

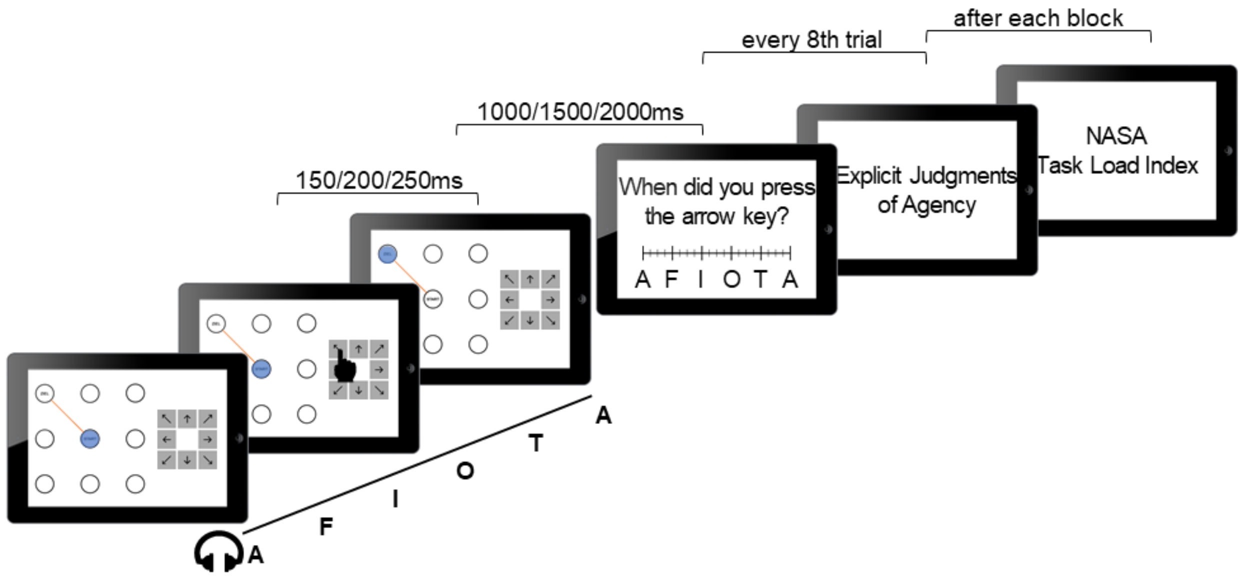
Auditory timer task | During trials, participants repeatedly heard five timed letters over headphones at a preset volume. This letter sequence, consisting of the German letters A, F, I, O, and T, served as auditory timer to reference the perceived timing of actions and effects. In the first experiment, we decided to use a sequence of five letters to ensure that participants would be able to store the entire sequence in their working memory while executing the visual task. Moreover, the selected number of auditory stimuli provided a good temporal resolution when transferred to the visual scale on the iPads where one pixel represented 2.5 ms (for a systematic manipulation of the number of letters, see Experiment 4: Manipulation of sequence length). The timed auditory letter sequence was designed so that the offset of one letter constituted the onset of the next, so there was no pause in between. In Experiment 1, we varied the length of each letter on three levels[4] (250 ms, 500 ms, 750 ms) between blocks. This resulted in continuous streams of letters that varied only in the broadness of the pronunciation. A representative example of the auditory stream is accessible at the project’s OSF page (https://osf.io/2746f/).

[3] Another question when employing such rather novel hardware for experimental setups pertains to their input lag, i.e., the systematic delay between the physical input and the device registering said input. Every technical device has input lag, and USB keyboards, which are mostly used in experimental setups, come with an input lag of up to 8ms. The devices that we used employed a touch sampling rate of 60Hz, which equals up to 16ms of input lag (even though newer devices improve on this). That said, in previous experiments we have successfully employed touchscreen devices in response time experiments (e.g. Dignath et al., 2020; Kunde et al., 2017; Wirth et al., 2019; Wirth, Dignath, et al., 2016; Wirth, Pfister, et al., 2016), showing high measurement precision with sufficient trials. Finally, as binding is computed as the difference between experimental- and baseline-conditions, which are both recorded using the same device, any systematic latencies should be cancelled out by subtraction.

[4] Throughout this work, interval length describes the lengths of the letters used as auditory measure rather than the delay between two events in the experimental task. Action-outcome delay denotes the time frame between an action and a subsequent outcome.

Figure 4

Trial procedure in the experiments



Note. The figure shows an example for a trial in the action experimental condition. Participants saw a 3 × 3 grid of circles on the left side of the screen and were asked to perform keypresses according to the directions given by the indicated goal area. During the trials they heard a sequence of German letters over headphones, which were subsequently used to report the timing of either the keypress or the cursor movement. After every eighth trial, participants had to answer three questions to give explicit agency ratings. Finally, participants completed the NASA Task Load Index (Hart & Staveland, 1988) at the end of each block.

Procedure. Participants encountered four different estimation conditions throughout the experiment (see Figure 5): (1) Action experimental: Cursor movements followed participants' keypresses and the perceived timing of the keypress was assessed. (2) Action baseline: Participants keypresses were not followed by a cursor movement and the perceived timing of the keypress was assessed. (3) Effect experimental: Cursor movements followed participants' keypresses and the perceived timing of the cursor movement was assessed. (4) Effect baseline: After a random interval of 2500-5000 ms a cursor movement occurred without participants' keypresses and the perceived timing of this cursor movement was assessed. These conditions were used to calculate temporal binding (see Results for more detail). As temporal binding is calculated as the difference between participants' estimation errors in the experimental compared to the baseline condition, absolute estimation errors will not be reported here, but can be retrieved from the OSF repository (https://osf.io/d3vz5/).

At trial onset, participants saw the grid on the left side of the screen and the keypad on the right side while hearing the letter sequence. The first letter of the letter sequence was selected at random. The circle in the middle of the grid was colored in blue and displayed the German word for start. Simultaneously, one of the other eight circles showed the German word for goal. These two circles were connected with a straight orange line, informing participants which key to press. Participants were asked to press the corresponding arrow key to move the cursor from the start area to the goal area. Additionally, participants received the instruction to wait at least three letters until they performed the keypress. They were also discouraged from pre-planning the time of their keypress and received the explicit information that this was not a speeded task, but rather that they could perform the keypresses at their leisure.

In the experimental conditions, their keypress was followed by the respective cursor movement after a random delay of 150, 200, or 250 ms. These delays were chosen in accordance with previous studies (e.g. Haggard et al., 2002; Ruess et al., 2017; Weller et al., 2020). We used varying delays so participants could not compute the timing of their action by simply subtracting a fixed interval from the perceived timing of the effect and vice versa. This way, they had to concentrate more thoroughly on the event in question. In the action baseline condition, participants only performed a keypress which did not cause the cursor to move. In the effect baseline condition, participants were asked not to press a key. In this condition, the cursor moved after a random delay between 2500-5000 ms after trial onset.

After the last event in each condition (i.e., cursor movement in the experimental conditions and effect baseline condition; keypress in the action baseline condition) the spoken letters presented over the headphones continued for another 1000, 1500, or 2000 ms. Subsequently, participants were asked to report the perceived timing of either their action or the cursor movement by locating it on a visual scale displaying the letter sequence (A-F-I-O-T-A), with the first and last letter being the same to ensure that the entire range of possible estimations was covered. The scale was presented in the center of the screen with a width of 1000px and a height of 100 px. It had six anchors for each letter that had three subdivisions each (see Figure 5). Participants could press any point on the scale to make their temporal judgement. Subsequently, this was translated into a continuous dependent variable reflecting participants' temporal estimation, 1 px = 2.5 ms, for further analyses. Following correct responses, the next trial started with an inter-trial interval of 2000 ms with the presentation of the grid, the start and a new goal area and the keypad. In cases when participants' keypresses did not correspond to the predefined path, the cursor followed participants' keypresses rather than the orange line and an error message was displayed. After such commission errors, participants received an error message in form of the German word for error (”Fehler") in red font in the center of the circle grid. If participants pressed a key in the effect baseline condition, they were informed not to press a button in the same way. This feedback was displayed after the cursor movement was completed and before participants had to give their time estimations.

In addition to the perceived timing, participants made explicit agency judgements on a continuous 100-point scale from -50 to 50. Participants rated their perceived authorship ("The dot moved as I wanted it to"), control ("I controlled the dot's movement"), and causation ("I caused the dot's movement") over the cursor movement. These ratings were given after every 8th trial in the experimental blocks.

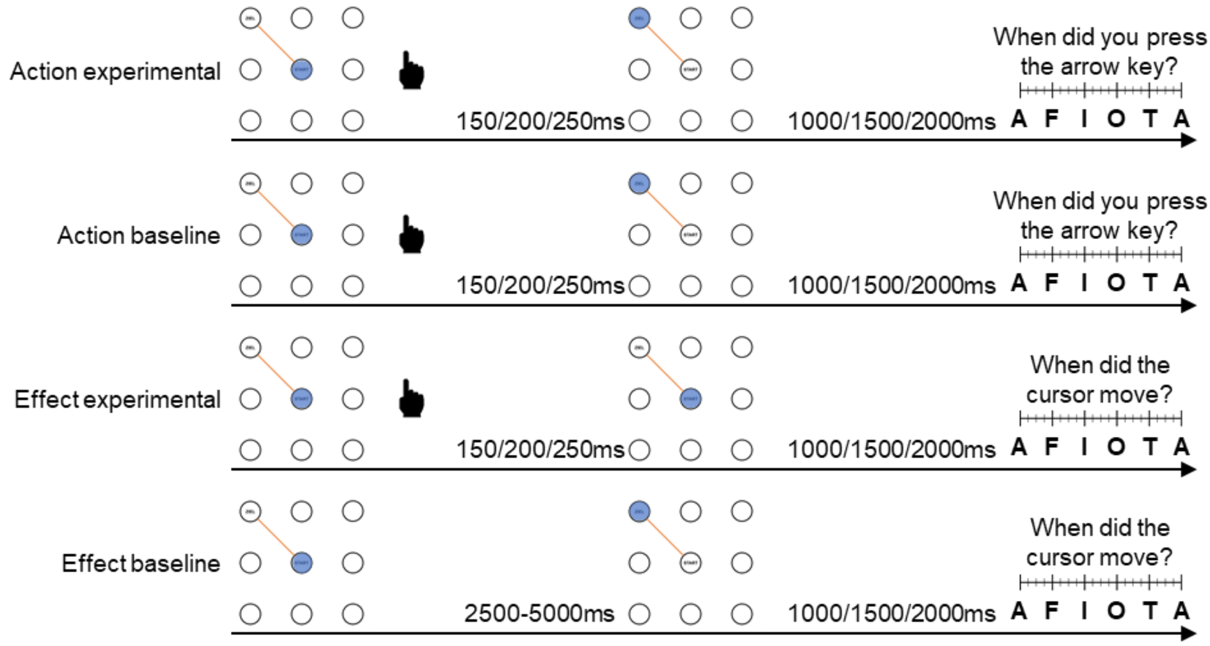
As the variable of interest for this experiment was the interval length, this factor was manipulated within-subjects. For counterbalancing, we divided the experiment into thirds and assigned a specific interval length (250, 500 and 750 ms) to each of them. The sequence of the four estimation conditions was also counterbalanced across participants with the perquisite that they always had to start with the baseline blocks before completing the experimental blocks. The sequence of conditions remained the same throughout all experimental thirds. Overall, participants completed 12 blocks (2 baseline blocks, then 2 experimental blocks for every interval length) of 40 trials each.

At the end of each third, participants filled out a German version of the NASA Task Load Index (TLX) consisting of six items to investigate subjective task load (Hart & Staveland, 1988).

It assesses mental demand, physical demand, temporal demand, performance, effort, and frustration on a continuous 10-point scale from low to high. The experiment took about 90 minutes. Raw data and analyses scripts are available on the Open Science Framework, https://osf.io/d3vz5/.

Figure 5

Schematic overview of the four conditions including timing



Note. (1) In the action experimental condition, participants' keypresses make the cursor move. Subsequently, participants report the timing of their keypress on the scale. (2) In the action baseline condition, participants again press an arrow key. However, this time it does not cause the cursor to move; rather, the cursor stays at the same position after a delay of 150, 200 or 250 ms. Afterwards, participants report the timing of their keypress. (3) In the effect experimental condition, participants' keypresses cause the cursor to move. At the end of the trial, participants are asked to report the timing of the cursor movement. (4) In the effect baseline condition, participants do not perform a keypress. However, after a random delay of 2500 - 5000 ms, the cursor moves from the start area into the goal area. Subsequently, participants report the timing of the cursor movement.

Design. The study used a 3 × 4 repeated-measures design with interval length (250 ms vs. 500 ms vs. 750 ms) and condition (action experimental vs. action baseline vs. effect experimental vs. effect baseline) as within-subjects factors.

Data analysis. To assess temporal binding, we first calculated estimation errors as the difference between participants' temporal estimates and the actual timing of the respective event ($timing_{estimation} – timing_{actual}$). For example, if participants pressed a key 100 ms after they heard the letter "I" but reported this key press as having occurred in the middle between "I" and "O" (i.e., 250 ms after the onset of letter "I", the estimation error for this particular trial was (250 ms-100 ms) 150 ms. We discarded erroneous trials and trials in which the temporal binding exceeded 2.5 SDs of the participant's cell mean in the respective condition (baseline vs. experimental; 250 ms vs. 500 ms vs. 750 ms). Subsequently, we calculated means for each estimation condition and interval length separately. These were then used to calculate the action binding and the effect binding for each interval length. Therefore, participants' estimation errors in the baseline conditions were subtracted from those in the respective experimental conditions (temporal binding = estimation $error_{exp}$ – estimation $error_{base}$). Positive values indicate that an occurrence in the experimental condition was perceived to have happened later than in the baseline condition, while negative values indicate an earlier perception of an occurrence in the experimental compared to the baseline condition.

To test our hypothesis, we first conducted separate two-tailed *t*-tests for all types of action binding as well as effect binding to see whether the differences between experimental and baseline conditions differed significantly from zero, that is, whether participants showed temporal binding. Then, we conducted two one-factorial analyses of variance (ANOVAs), one for action binding and one for effect binding, with interval length (250, 500, 750 ms) as within-subjects factor to uncover specific differences between the individual interval lengths. Follow-up analyses were conducted via two-tailed, paired *t*-tests. Effect sizes for all paired *t*-tests were calculated as $d_z = \frac{t}{\sqrt{n}}$.

For explicit agency judgements, we calculated mean scores for explicit agency ratings (authorship, control, causation) for each condition (action experimental, effect experimental) and each interval length individually. Then, a one-way ANOVA with condition (action vs. effect) as within-subjects factor was conducted to uncover differences in participants' subjective judgements of agency between conditions in which participants focused either on the action or the effect. Ultimately, three repeated-measures ANOVAs with interval length (250 ms vs. 500 ms vs. 750 ms) as within-subjects factor were conducted.

To assess participants' task load with different interval lengths, mean scores for each scale of the NASA TLX were calculated and compared between the three interval lengths. A repeated-measures ANOVA with interval length (250 ms vs. 500 ms. vs. 750 ms) as within-subjects factor was conducted for each scale separately. Follow-up analyses were analyzed via two-tailed, paired *t*-tests. Effect sizes for all paired *t*-tests were calculated as $d_z = \frac{t}{\sqrt{n}}$.

Additionally, for non-significant results, we used post-hoc Bayes analyses to further examine the evidence for and against the null hypothesis. We calculated Bayes factors using the computer software JASP (JASP Team, 2018). As stated in the preregistration, we expected medium to large effects. Thus, we used a scale parameter of 0.25 for the analyses. This corresponds to a probability of 80% that the effect lies between -0.8 and 0.8. As per convention, a Bayes factor of $BF_{10} < \frac{1}{3}$ can be interpreted as evidence in favor of the null hypothesis while Bayes factors (BF_{10}) greater than 3 yield at least moderate evidence for the alternative hypothesis (Dienes, 2014). As we tested for equality, however, we used the inverse BF_{01} (with $BF_{01} = \frac{1}{BF_{10}}$) and thus, the inverse decision criteria apply (see also Janczyk & Pfister, 2020).

4.2.2. Results

Temporal binding. Erroneous trials (0.8%) and trials in which temporal binding exceeded 2.5 SDs of the participant's cell mean (2.6%) were excluded from the analyses. Errors occurred mainly in the first trials of effect baseline blocks in which participants were asked not to press a key. Nevertheless, error rates showed obvious floor effects. Therefore, error rates will not be analyzed further (see P. Dixon, 2008 for comments regarding floor and ceiling effects in the analysis of error data).

Action binding. Data showed significantly larger estimation errors for experimental conditions compared to baseline conditions for all comparisons except for the action binding in the 750 ms condition, $t_{250}(47) = 2.57$, $p = .013$, $d_z = 0.37$, $\Delta = 23.06$ ms, $t_{500}(47) = 4.10$, $p < .001$, $d_z = 0.59$, $\Delta = 51.89$ ms, $t_{750}(47) = 1.46$, $p = .151$, $d_z = 0.21$, $\Delta = 39.22$ ms. That is, the action was overall reported to be shifted towards the effect, while this was not the case in the 750 ms condition. Participants did indeed judge actions to have occurred later in time when they were followed by a cursor movement than when they were executed in isolation.

The ANOVA for action binding with interval length (250 ms vs. 500 ms vs. 750 ms) as within-subjects factor did not show any significant difference in the magnitude of action binding between the three interval lengths, $F < 1$, $BF_{01} = 7.71$ (see Figure 6A).

Effect binding. Estimation errors of effect differed significantly between experimental and baseline conditions for all three interval lengths, $t_{250}(47) = -8.21$, $p < .001$, $d_z = 1.18$, $\Delta = -159.77$ ms, $t_{500}(47) = -6.26$, $p < .001$, $d_z = 0.90$, $\Delta = -132.74$ ms, $t_{750}(47) = -3.08$, $p = .003$, $d_z = 0.44$, $\Delta = -83.97$ ms. Cursor movements were reported to have happened earlier when a keypress preceded this cursor movement.

The ANOVA for effect binding with interval length (250 ms vs. 500 ms vs. 750 ms) as within-subjects factor revealed a significant difference in binding size between the different interval lengths, $F(2,94) = 5.15$, $p = .008$, $\eta_p^2 = .10$. That is, effect binding increased significantly between the 750 ms and the 250 ms condition, $t(47) = -2.79$, $p = .008$, $d_z = 0.40$, as well as between the 750 ms and the 500 ms condition, $t(47) = -2.08$, $p = .043$, $d_z = 0.30$. There was no clear evidence for or against a difference between the short and medium interval length, $t(47) = -1.28$, $p = .206$, $d_z = 0.18$, $BF_{01} = 1.49$.

Explicit agency judgements. Explicit judgments of agency did not differ between conditions (i.e. action experimental vs. effect experimental), $F(1,47) = 1.25$, $p = .270$, $\eta_p^2 = .03$, $BF_{01} = 9.38$, so explicit agency judgements were calculated across conditions. In general, agency ratings were high for all three types of judgement, authorship (M = 25.23, SD = 19.54), control (M = 22.59, SD = 20.72), and causation (M = 35.17, SD = 13.85).

Subsequently, three repeated-measures ANOVAs with interval length (250 ms vs. 500 ms vs. 750 ms) as within-subjects factor were conducted. Explicit authorship ratings differed significantly between the different interval lengths, $F(2,94) = 4.75$, $p = .011$, $\eta_p^2 = .09$. This effect was mainly due to participants' significantly lower authorship ratings in the 250 ms condition compared to the 500 ms condition, $t(47) = -2.73$, $p = .009$, $d_z = -0.39$, while their ratings in the 500 ms and the 750 ms condition did not show clear evidence for or against a difference, $t < 1$, $BF_{01} = 2.58$. Explicit agency judgements for control and causation were not influenced by interval length, $F_{control}(2,94) = 1.51$, $p = .226$, $\eta_p^2 = .03$, $BF_{01} = 4.18$, $F_{causation} < 1$, $BF_{01} = 7.41$ (see Figure 6B).

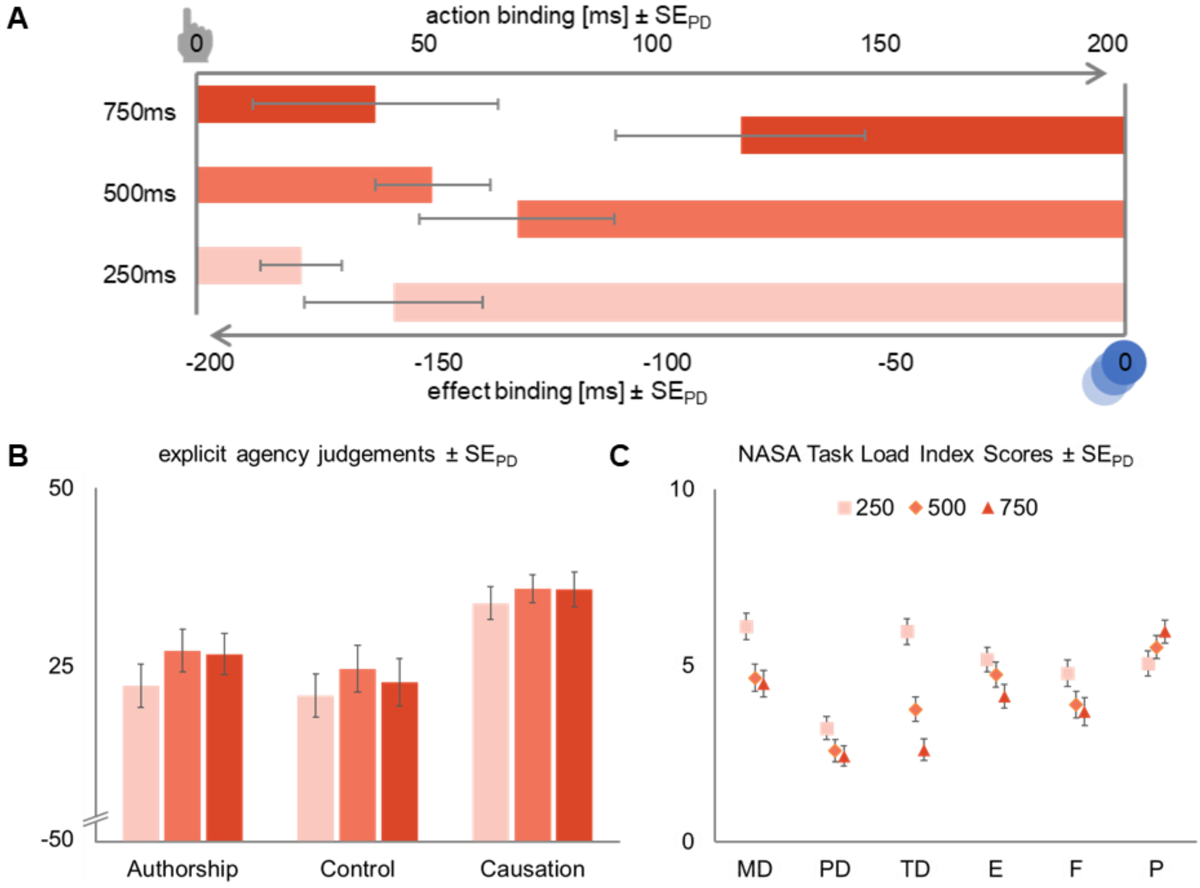
NASA Task Load Index. Participants filled out the NASA Task Load Index to see whether the manipulation of interval length had an effect on perceived task load. Here we only report the subscales on with interval length had an influence (see Figure 6C). All other results can be found on the OSF repository (https://osf.io/d3vz5/).

Data showed a significant effect of interval length on mental demand (MD), $F(2,94) = 16.19$, $p < .001$, $\eta_p^2 = .26$. Mental demand decreased significantly between the 250 ms and the 500 ms condition, $t(47) = 4.61$, $p < .001$, $d_z = 0.67$, and between the 250 ms and the 750 ms condition, $t(47) = 5.56$, $p < .001$, $d_z = 0.80$, while it there was no clear evidence for or against a difference between the two longer intervals, $t < 1$, $d_z = 0.07$, $BF_{01} = 2.45$.

The same held true for physical demand (PD). It differed significantly between the three interval lengths, $F(2,94) = 5.24$, $p = .007$, $\eta_p^2 = .10$. While there was a slight decrease in physical demand between the 250 ms and the 500 ms condition, $t(47) = 2.11$, $p = .040$, $d_z = 0.30$, and between the 250 ms condition and the 750 ms condition, $t(47) = 4.71$, $p < .001$, $d_z = 0.68$. There was no clear evidence for or against a difference between the medium and the long interval, $t < 1$, $d_z = 0.08$, $BF_{01} = 2.39$.

Figure 6

Temporal Binding, explicit agency judgements, and NASA TLX scores in Experiment 1



Note. (A) Action binding and effect binding relative to the baseline condition. The y-axis intercept denotes the perceived timing of the action (top) and the perceived timing of the effect (bottom) in the respective baseline conditions. Action binding is shown as bars from left to right to indicate the perceived delay of the action. Effect binding is shown as bars from right to left to indicate the perceived advancement of the effect. Error bars depict standard errors of paired differences for the factor interval length (Pfister & Janczyk, 2013). (B) Explicit agency judgments for authorship, control, and causation of the cursor movement. Agency judgments were made on a scale from -50 to 50 after every eighth trial in all experimental conditions. (C) Perceived task load as measured with the NASA Task Load Index (Hart & Staveland, 1988). MD: mental demand, PD: physical demand, TD: temporal demand, E: effort, F: frustration, P: performance. Squares represent participants' judgments with letters of 250 ms, diamonds 500 ms, and triangles letters with a length of 750 ms. Error bars in both panels depict standard errors of paired differences for the factor interval length (Pfister & Janczyk, 2013).

The ANOVA for temporal demand (TD) revealed significant differences between the three conditions, $F(2,94) = 37.04$, $p < .001$, $\eta_p^2 = .44$. Temporal demand decreased significantly from the 250 ms to the 500 ms condition, $t(47) = 5.59$, $p < .001$, $d_z = 0.81$, as well as from the 250 ms to the 750 ms condition, $t(47) = 7.80$, $p < .001$, $d_z = 1.13$. Temporal demand in the 500 ms condition was also significantly higher than in the 750 ms condition, $t(47) = 3.20$, $p = .003$, $d_z = 0.46$.

Data showed a significant effect of interval length on performance (P), $F(2,94) = 4.48$, $p = .014$, $\eta_p^2 = .09$. Performance gradually increased with increasing interval length. However, there was neither evidence for nor against a difference between either the 250 ms and the 500 ms condition, $t(47) = -1.60$, $p = .115$, $d_z = 0.23$, $BF_{01} = 1.07$, or the 500 ms and the 750 ms condition, $t(47) = -1.39$, $p = .170$, $d_z = 0.20$, $BF_{01} = 1.34$. Performance in the 250 ms condition was rated significantly higher than in the 750 ms condition, $t(47) = -3.02$, $p = .004$, $d_z = 0.44$.

Data showed a significant effect of interval length on effort (E), $F(2,94) = 4.36$, $p = .016$, $\eta_p^2 = .09$. Effort gradually decreased with increasing interval length. Effort was significantly lower in the 750 ms condition compared to the 250 ms condition, $t(47) = 3.00$, $p = .004$, $d_z = 0.43$. Further analyses did not show any clear evidence for or against a difference between the 250 ms condition and the 500 ms condition, $t(47) = 1.15$, $p = .258$, $d_z = 0.17$, $BF_{01} = 1.67$, and between the 500 ms condition and the 750 ms condition, $t(47) = 1.78$, $p = .081$, $d_z = 0.26$, $BF_{01} = 0.86$.

The ANOVA revealed a significant effect of interval length on frustration (F), $F(2,94) = 4.31$, $p = .016$, $\eta_p^2 = .08$. Frustration decreased significantly from the 250 ms and the 500 ms condition, $t(47) = 2.58$, $p = .013$, $d_z = 0.37$, and from the 250 ms condition to the 750 ms condition, $t(47) = 2.58$, $p = .013$, $d_z = 0.37$, while there was no clear evidence for or against a difference between the two longer intervals, $t < 1$, $BF_{01} = 2.46$.

4.2.3. Discussion

We investigated whether varying lengths of the letters constituting the auditory timer have an influence on temporal binding. Experiment 1 served the purpose of determining the optimal interval length for our setup. Participants executed a navigation task on an iPad while hearing timed auditory stimuli over headphones. These stimuli were five German letters with three different interval lengths (250, 500, 750 ms). All interval lengths produced effect binding and the perceived timing of actions in all conditions tended to be shifted towards the effect. However, action binding did not differ significantly from zero in the condition with letters of 750 ms. These results are in line with previous studies using temporal binding as a measure which also report smaller action binding than effect binding (Beck et al., 2017; Ruess et al., 2017). Thus, we conclude that our setup is in principle capable of measuring temporal binding and of replicating previous findings on temporal binding.

All interval lengths showed medium to large effects for effect binding. This, as well as the absolute magnitude of the estimation errors, replicates previous studies examining temporal binding by means of a visual Libet Clock (Ruess et al., 2017; Schwarz, Weller, Klaffehn, & Pfister, 2019; Wolpe et al., 2013). As effect binding did not differ significantly between short and medium intervals, it seems that there is not one ideal interval length to measure temporal binding with an auditory timer. Rather, it appears that auditory stimuli with short to medium length, remaining below a certain threshold (in this case 750 ms) seem to be suitable for revealing temporal binding. The same applies for action binding; both effect sizes and absolute estimation errors replicated previous studies at least for the two shorter interval lengths. Therefore, our recommendation is that the auditory stimuli should be no shorter than 250 ms but not longer than 500 ms.

Contrary to the implicit temporal binding measures, the length of the presented auditory stimuli did not influence explicit agency judgements. Throughout the experiment, participants rated their sense of agency to be high in almost all conditions. The only condition in which explicit sense of agency was slightly diminished was when participants had to rate their authorship over the cursor movements in the 250 ms condition. Previous studies with predictable action-outcome delays have shown that increasing these delays (> 200 ms) produce lower explicit agency ratings (Wen et al., 2015a). In the present study, action-outcome delays varied on a trial by trial basis between 150 ms and 250 ms. Additionally, agency ratings were recorded after every 8th trial rendering it impossible to map agency ratings to specific action-outcome delays. Therefore, it is plausible that participants made an overall judgement across the previous mini-block resulting in less differentiated judgements of agency. To sum it up, interval length does not seem to have a great influence on participants' explicit agency judgements, which can therefore be neglected when designing the auditory timer. Researchers should however also bare participants' task load and frustration during task execution in mind, as this is often detrimental to their concentration and task irrelevant thoughts over the course of the experimental session.

Over the course of the experiment, there was a trend that task load decreased with increasing interval lengths. This was also the case for participants' perceived effort and frustration that decreased as the length of the presented letters increased. This pattern reversed for participants' self-ratings of performance. They judged to be doing better on task completion when interval length increased. Consequently, we recommend the utilization of intervals with a medium length for the auditory timer. This way, researchers can ensure low to moderate task load while also maintaining participants' self-image as being competent on the task.

To sum it up, with regard to the temporal estimation measure, we decided to use an interval length of 500 ms for subsequent studies. This interval length appeared to create the most robust action binding while also producing reasonably large effect binding. Additionally, considering participants' task load ratings, the 500 ms interval seemed to evoke a tolerable task load while even shorter intervals unnecessarily increased task load and at the same time lowered subjective performance ratings descriptively. This design decision is emphasized by participants' explicit agency judgments, which tended to be slightly lower in the 250 ms condition compared to the 500 ms condition.

4.3. Experiment 2: Manipulation of interval filling

In Experiment 2 we systematically manipulated the factor interval filling, that is the way in which the spoken letters were presented. This design factor was chosen as it contributes to the temporal resolution of the auditory timer. Letters were presented in three different ways: filled, half-filled, and sequenced. We expected half-filled intervals to be a poor measure for temporal binding, as the silence in the second half of the interval does not provide temporal information. Contrary, sequenced intervals should provide participants with more anchors and therefore make temporal judgements easier. The addition of temporal information should however also lead to increased task load.

4.3.1. Methods

Participants. A new set of 48 participants (15 male, 4 left-handed) with a mean age of 28.42 years (*SD* = 9.70) were recruited and fulfilled the same criteria as in Experiment 1.

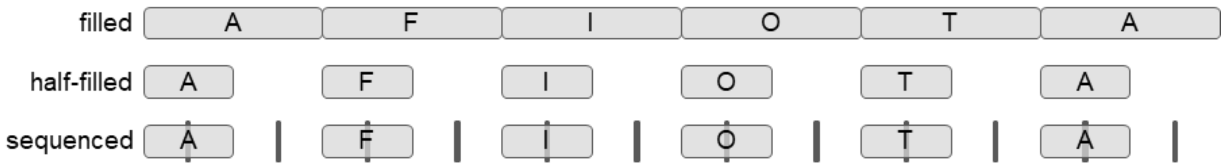
Apparatus and stimuli. The visual task was left unchanged from Experiment 1. For the auditory timer, participants again heard the German letters A, F, I, O, and T over headphones. But this time we varied the filling of the letter intervals on three levels (filled, half-filled or sequenced) between blocks. In the filled condition, the entire 500 ms interval was filled with a spoken letter. In the half-filled condition, intervals consisted of spoken letters (250 ms) followed by 250 ms of silence until the end of the interval. In the sequenced condition, there was a steady metronome-like timer consisting of short clicks with a speed of 4 clicks per second. This timer was synchronized with the spoken letters in a way that there was a click in the middle of the spoken letter (at 125 ms) and one click half way through the silence following the letter, that is at 375 ms after the letter onset. Figure 7 shows the three different interval fillings.

Procedure. The procedure of Experiment 2 followed that of Experiment 1. As the variable of interest in Experiment 2 was the interval filling, this factor was manipulated within subjects and we divided the experiment into thirds and assigned a specific interval filling (filled, half-filled, and sequenced) to each third. The order of interval fillings was counterbalanced across participants.

Design. The study used a 3 × 4 repeated-measures design with interval filling (filled vs. half-filled vs. sequenced) and condition (action experimental vs. action baseline vs. effect experimental vs. effect baseline) as within-subjects factors.

Figure 7

Manipulation of the interval filling in Experiment 2



Note. In the filled condition, letters were 500 ms long, and the offset of one letter marked the onset of the next. In the half-filled condition, spoken letters were 250 ms long and were followed by 250 ms of silence before the onset of the next letter. The sequenced condition consisted of spoken letters of 250 ms and a 250 ms pause. Additionally, metronome-like clicks (depicted here by the dark lines) were presented after 125 ms and 375 ms in order to aid participants' temporal resolution. Representative examples can be found on the project's OSF page (https://osf.io/d3vz5/).

4.3.2. Results

Data analyses in Experiment 2 followed that described in Experiment 1.

Temporal binding. Erroneous trials (0.8%) and outliers, trials in which temporal binding exceeded 2.5 SDs of the participant's cell mean (2.7%), were excluded from the analyses.

Action binding. Participants showed action binding irrespective of the interval filling. That is, actions were perceived to have happened later in the filled condition, $t(47) = 2.48$, $p = .017$, $d_z = 0.36$, $\Delta = 33.27$ ms, as well as the half-filled condition, $t(47) = 2.19$, $p = .033$, $d_z = 0.32$, $\Delta = 20.95$ ms, and the sequenced condition, $t(47) = 2.41$, $p = .020$, $d_z = 0.35$, $\Delta = 23.07$ ms. Participants did indeed judge actions to have occurred later in time when they were followed by a cursor movement than when they were executed in isolation.

The ANOVA for action binding with interval filling (filled vs. half-filled vs. sequenced) as within-subjects factor did not show any significant difference in the magnitude of action binding between the three interval fillings, $F < 1$, $BF_{01} = 10.23$ (see Figure 8A).

Effect binding. Cursor movements in all three conditions were perceived to be shifted towards the preceding action, $t_{\text{filled}}(47) = -6.21$, $p < .001$, $d_z = 0.90$, $\Delta = -124.10$ ms, $t_{\text{half}}(47) = -6.46$, $p < .001$, $d_z = 0.93$, $\Delta = -135.26$ ms, $t_{\text{sequenced}}(47) = -5.77$, $p < .001$, $d_z = 0.83$, $\Delta = -113.74$ ms. That is, cursor movements were perceived to have happened earlier when a keypress preceded this cursor movement.

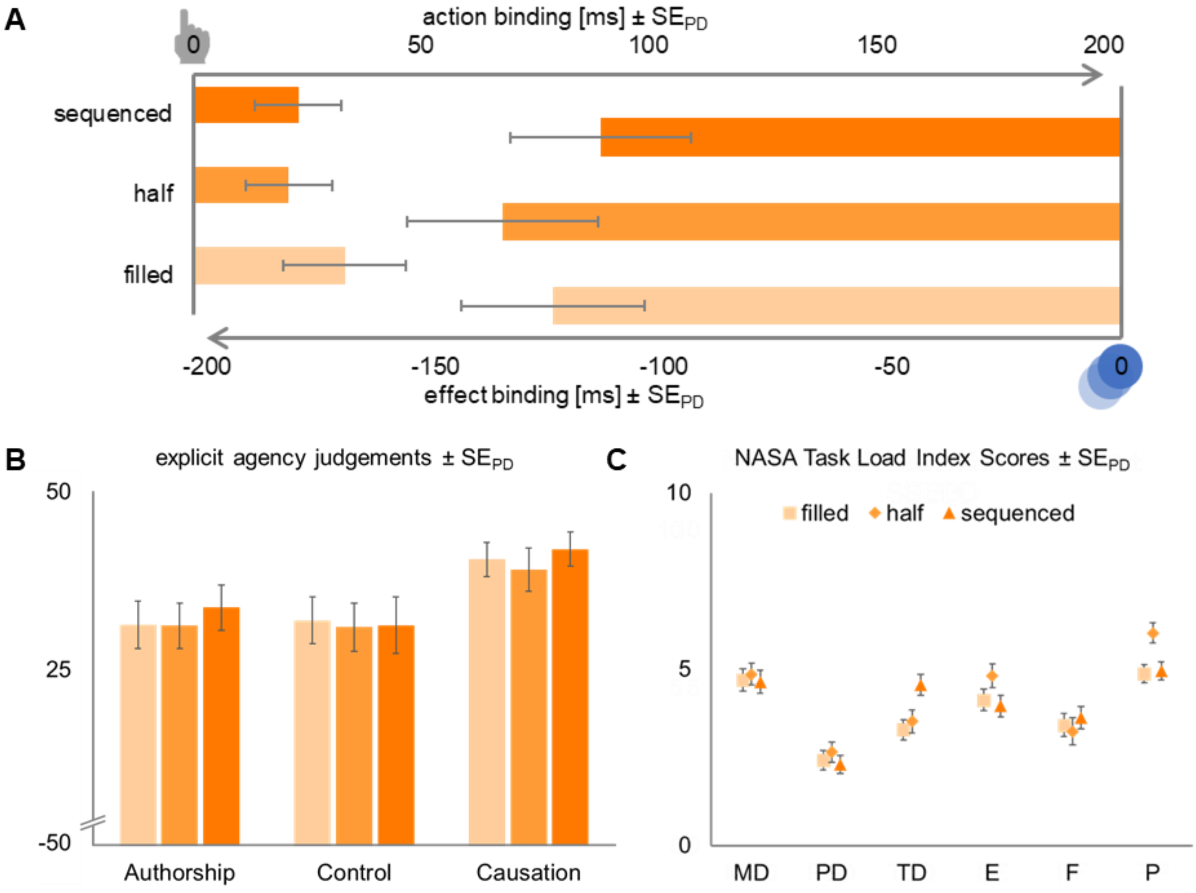
The ANOVA for effect binding with interval filling (filled vs. half-filled vs. sequenced) as within-subjects factor did not show any significant differences between the interval fillings, either, $F < 1$, $BF_{01} = 9.12$.

Explicit agency judgements. As in Experiment 1, there was no significant difference in judgements of agency between action experimental and effect experimental conditions, $F(1,47) = 2.59$, $p = .114$, $\eta_p^2 = .05$, $BF_{01} = 3.10$. Thus, explicit agency judgements were calculated across conditions. Again, agency ratings were high for all three types of judgement, authorship ($M = 28.83$, $SD = 19.14$), control ($M = 28.20$, $SD = 20.75$), and causation ($M = 36.48$, $SD = 15.15$).

Subsequently, three repeated-measures ANOVAs with Interval Filling (filled vs. half-filled vs. sequenced) as within-subjects factor were conducted. Explicit agency judgements did not differ significantly between the different interval fillings, $F_{\text{authorship}}(2,94) = 1.14$, $p = .323$, $\eta_p^2 = .02$, $BF_{01} = 5.69$, $F_{\text{control}} < 1$, $BF_{01} = 13.80$, $F_{\text{causation}}(2,94) = 1.13$, $p = .327$, $\eta_p^2 = .02$, $BF_{01} = 5.70$ (see Figure 8B).

Figure 8

Temporal Binding, explicit agency judgements, and NASA TLX scores in Experiment 2



Note. (A) Action binding and effect binding relative to the baseline condition. The y-axis intercept denotes the perceived timing of the action (top) and the perceived timing of the effect (bottom) in the respective baseline conditions. Action binding is shown as bars from left to right to indicate the perceived delay of the action. Effect binding is shown as bars from right to left to indicate the perceived advancement of the effect. Error bars depict standard errors of paired differences for the factor interval filling (Pfister & Janczyk, 2013). (B) Explicit agency judgments for authorship, control, and causation of the cursor movement. Agency judgments were made on a scale from -50 to 50 after every eighth trial in all experimental conditions. (C) Perceived task load as measured with the NASA Task Load Index (Hart & Staveland, 1988). MD: mental demand, PD: physical demand, TD: temporal demand, E: effort, F: frustration, P: performance. Squares represent participants' judgments for a sequence with filled letters, diamonds with half-filled letters, and triangles with sequenced letters. All error bars depict standard errors of paired differences for the factor interval length (Pfister & Janczyk, 2013).

NASA Task Load Index. The ANOVA for temporal demand revealed significant differences between the three interval fillings, $F(2,94) = 15.01$, $p < .001$, $\eta_p^2 = .24$. Temporal demand did not show clear evidence for or against a difference between filled and half-filled intervals, $t < 1$, $d_z = 0.08$, $BF_{01} = 2.37$. However, temporal demand increased significantly in the sequenced condition compared to the filled letters, $t(47) = -4.15$, $p < .001$, $d_z = 0.60$, and the half-filled letters, $t(47) = -4.58$, $p < .001$, $d_z = 0.66$. Interval filling did not have any significant effect on either of the items mental demand, physical demand, or effort, all Fs < 1, all $BF_{01} > 7.20$. Even though, there was a descriptive trend towards better performance in the half-filled condition than in the other two, there was neither evidence for nor against any effect of interval filling on performance, $F(2,94) = 2.50$, $p = .087$, $\eta_p^2 = .05$, $BF_{01} = 1.83$. Data did not show clear evidence for or against an effect of interval filling on frustration either, $F(2,94) = 2.21$, $p = .115$, $\eta_p^2 = .05$, $BF_{01} = 2.29$. However, there was a descriptive trend towards lower frustration in the half-filled condition than in the filled and sequenced condition (see Figure 8C).

4.3.3. Discussion

With Experiment 2, we intended to determine how intervals should be filled. Surprisingly, interval filling influenced neither participants' temporal estimations nor their task load. That is, contrary to out hypothesis, all interval fillings produced both robust action binding as well as effect binding which did not differ significantly in size. Again, effect sizes were larger for effect binding, replicating previous results on temporal binding, where effect binding was stronger than action binding (Wolpe et al., 2013).

As attending to the auditory timer is not the primary task, participants' attention was probably more focused on the visual task than on the design of the auditory timer. This attentional bias might in turn have led to reduced discrimination between the interval fillings. Considering that 250 ms suffice to discriminate the letters in our experiments, it is likely that participants simply judged whether the event in question occurred before, after, or during this letter discrimination. What is interesting is that the sequenced filling, which was designed to provide additional time cues, i.e., temporal anchors, did not influence binding sizes either.

Contrary, sequenced letters increased participants' perceived task load by leading to higher temporal demand ratings as well as higher frustration while participants judged their performance to be inferior in the sequenced condition.

As explicit agency judgements did not differ between the three types of interval filling either, we conclude that the manipulation does not have strong consequences for our experimental design. Nonetheless, with regard to the NASA TLX, participants seemed to prefer the half-filled letters. This might result from the fact that this sequence sounded most natural. When we pronounce letter sequences in our daily life, we usually make short pauses between the letters, akin to the silence in the second half of the half-filled interval. Thus, we decided to use half-filled letters for subsequent studies. It is however worth noting that researchers may adjust the filling according to their needs and stimuli without risking sabotaging their data.

4.4. Experiment 3: Manipulation of sequence predictability

Experiment 3 tested the influence of sequence predictability on temporal binding. We manipulated the order in which the spoken letters were presented on three levels: predictable, shuffled, random. Sequence predictability is of interest, as on the one hand, better predictability might lead to increased use of strategies, e.g., always pressing the key at the same letter. On the other hand, reduced predictability might increase task load and derail attention from the visual task to the auditory timer. Finally, the movement of the visual Libet Clock is typically perfectly predictable (in fact we are not aware of a study that used randomly jumping pointer positions of a visual Libet clock). Finding that predictability of time markers did impact temporal binding might thus be an observation of general interest beyond the auditory timer employed here. Therefore, we tested how sequence predictability influences temporal binding.

4.4.1. Methods

Participants. Forty-eight new participants (19 male, 8 left-handed) with a mean age of 26.10 years (*SD* = 7.30) who fulfilled the same criteria as in Experiments 1 and 2 took part in the experiment.

Apparatus and stimuli. The visual task was left unchanged from the first two experiments. For the auditory timer, participants again heard the German letters A, F, I, O, and T over headphones. In Experiment 3, we varied the predictability of the sequence in which the letters were presented on three levels (predictable, shuffled, and random) between blocks. The letter sequence in this experiment followed Experiments 1 and 2 in a way that intervals were 500 ms long and half-filled, which means they consisted of spoken letters with a length of 250 ms followed by 250 ms silence. In the predictable condition, participants repeatedly heard the letters A, F, I, O, and T, in the same order. In the shuffled condition, however, the order of the letter sequence was shuffled at the beginning of every trial. That is, participants could predict the letter sequence but only on a trial base rather than for the whole experiment. In the random condition, the order of the letter sequence was also determined at the beginning of each trial, only this time the sequence was drawn randomly from the set of five letters, with the perquisite that no letter could appear twice in a row.

Procedure. As the variable of interest in Experiment 3 was the sequence predictability, this factor was manipulated within subjects and we divided the experiment into thirds and assigned a specific sequence predictability (predictable, shuffled, and random) to each third. The order of predictability types was counterbalanced across participants. The procedure of Experiment 3 followed that of Experiment 1 with two exceptions concerning the presentation of the scale for time estimations at the end of each trial.

In the predictable condition, the scale was the same as in the previous experiments, it started with the letter A and subsequently displayed the letters F, I, O, and T before finishing with another A so that all intervals between letters were displayed. As a new letter sequence was determined at the beginning of each trial in the shuffled condition, the scale had to be adjusted accordingly. In blocks with shuffled letter sequence, participants used a scale that displayed the respective letter sequence again with the starting and finishing letter being the same. The display of the scale in conditions with a random letter sequence was again different. In these trials the scale was determined by displaying the actual timing (objectively correct judgement of the respective event) between the second and the fifth category. The surrounding letters were determined according to the sequence of the respective trial.

Design. The study used a 3 × 4 repeated-measures design with sequence predictability (predictable vs. shuffled vs. random) and condition (action experimental vs. action baseline vs. effect experimental vs. effect baseline) as within-subjects factors.

4.4.2. Results

Data analyses in Experiment 3 followed that described in Experiment 1.

Temporal binding. Erroneous trials (0.6%) and outliers exceeding 2.5 SDs of the participant's cell mean (3.3%) were excluded from the analyses.

Action binding. Separate *t*-tests revealed action binding in both the predictable condition, $t(47) = 2.57$, $p = .013$, $d_z = 0.37$, $\Delta = 43.49$ ms, and the random condition, $t(47) = 3.10$, $p < .001$, $d_z = 0.45$, $\Delta = 47.29$ ms. There was no clear evidence for or against action binding in the shuffled condition, $t(47) = 1.90$, $p = .064$, $d_z = 0.27$, $BF_{10} = 1.36$, $\Delta = 29.08$ ms. Participants judged actions in the predictable and random condition to be shifted towards the ensuing cursor movement (see Figure 9A).

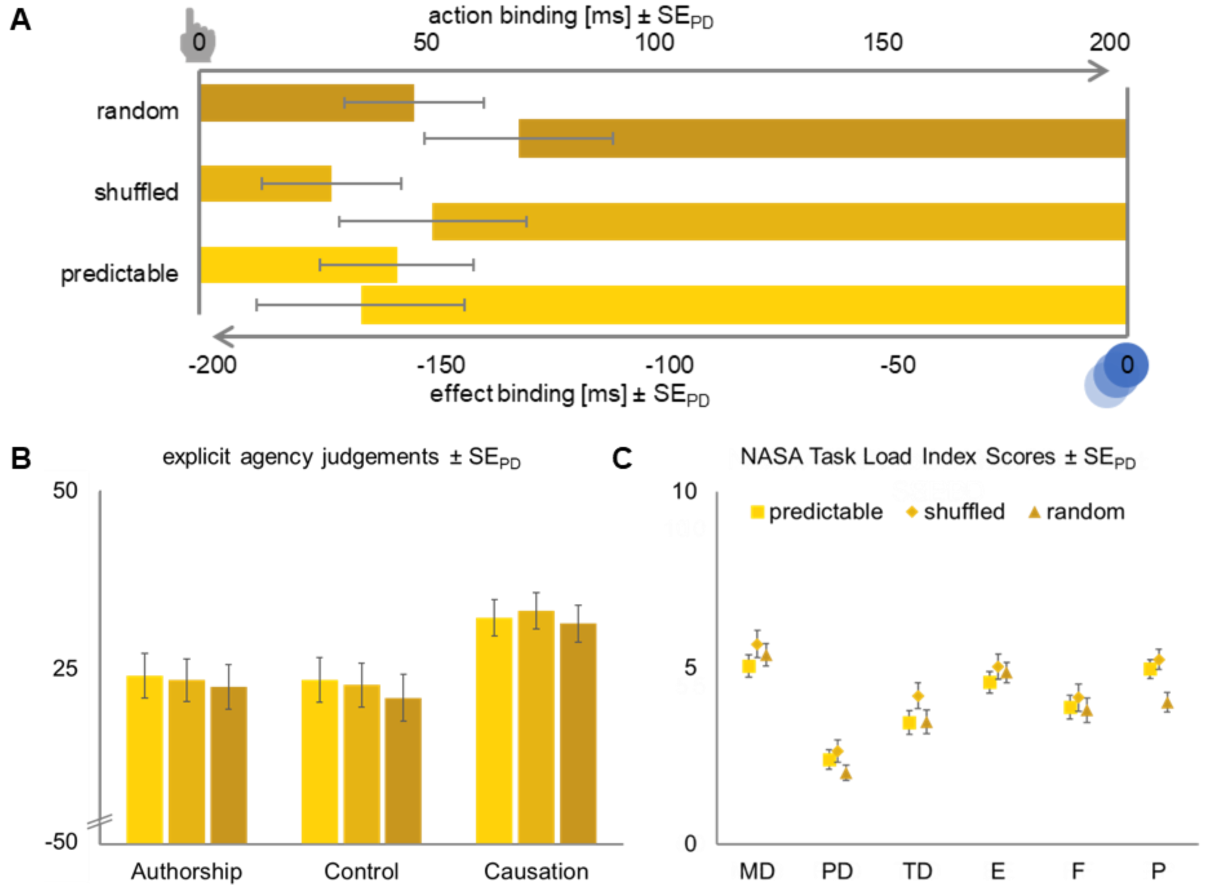
The ANOVA for action binding with sequence predictability (predictable vs. shuffled vs. random) as within-subjects factor did not show any significant difference in the magnitude of action binding, $F < 1$, $BF_{01} = 9.66$.

Effect binding. Cursor movements in all three conditions were perceived to be shifted towards the preceding action, $t_{predictable}(47) = -7.34$, $p < .001$, $d_z = 1.06$, $\Delta = -167.49$ ms, $t_{shuffled}(47) = -7.43$, $p < .001$, $d_z = 1.07$, $\Delta = -151.76$ ms, $t_{random}(47) = -6.43$, $p < .001$, $d_z = 0.93$, $\Delta = -132.88$ ms. That is, cursor movements were perceived to have happened earlier when a keypress preceded this cursor movement.

The ANOVA for effect binding with sequence predictability (predictable vs. shuffled vs. random) as within-subjects factor did not show any significant differences between the different types of predictability, $F(2,94) = 1.70$, $p = .188$, $\eta_p^2 = .04$, $BF_{01} = 3.54$.

Figure 9

Temporal Binding, explicit agency judgements, and NASA TLX scores in Experiment 3



Note. (A) Action binding and effect binding relative to the baseline condition. The y-axis intercept denotes the perceived timing of the action (top) and the perceived timing of the effect (bottom) in the respective baseline conditions. Action binding is shown as bars from left to right to indicate the perceived delay of the action. Effect binding is shown as bars from right to left to indicate the perceived advancement of the effect. (B) Explicit agency judgments for authorship, control, and causation of the cursor movement. Agency judgments were made on a scale from -50 to 50 after every eighth trial in all experimental conditions. (C) Perceived task load as measured with the NASA Task Load Index (Hart & Staveland, 1988). MD: mental demand, PD: physical demand, TD: temporal demand, E: effort, F: frustration, P: performance. Squares represent participants' judgments for perfectly predictable sequences, diamonds shuffled sequences which were determined at the beginning of each trial, and triangles for completely random sequences. All error bars depict standard errors of paired differences for the factor sequence predictability (Pfister & Janczyk, 2013).

Explicit agency judgements. As in the first two experiments, there was only anecdotal evidence for a difference in judgements of agency between action experimental and effect experimental conditions, $F < 1$, $BF_{10} = 2.44$. Thus, explicit agency judgements were calculated across conditions. Again, agency ratings were high for all three types of judgement, authorship ($M = 23.15$, $SD = 20.47$), control ($M = 22.20$, $SD = 20.74$), and causation ($M = 32.16$, $SD = 16.98$).

Subsequently, three repeated-measures ANOVAs with sequence predictability (predictable vs. shuffled vs. random) as within-subjects factor were conducted. Explicit agency judgements did not differ significantly between the different sequence predictabilities, all Fs < 1, all $BF_{01} > 7.16$ (see Figure 9B).

NASA Task Load Index. Data showed a significant effect of sequence predictability on performance, $F(2,94) = 7.51$, $p = .001$, $\eta_p^2 = .14$. There was no evidence for or against a difference between fully predictable and shuffled sequences, $t(47) = 1.10$, $p = .278$, $d_z = 0.16$, $BF_{01} = 1.74$. The random condition, however, elicited significantly lower performance ratings than both the predictable condition, $t(47) = 3.91$, $p < .001$, $d_z = 0.56$, and the shuffled condition, $t(47) = 2.66$, $p = .011$, $d_z = 0.38$. No other effects of sequence predictability were observed (see Figure 9C), $F_{MD}(2,94) = 1.09$, $p = .340$, $BF_{01} = 6.03$, $\eta_p^2 = .02$, $F_{PD}(2,94) = 1.41$, $p = .248$, $\eta_p^2 = .03$, $BF_{01} = 4.54$, $F_{TD}(2,94) = 1.28$, $p = .283$, $\eta_p^2 = .03$, $BF_{01} = 5.09$, $F_E(2,94) = 1.03$, $p = .360$, $\eta_p^2 = .02$, $BF_{01} = 6.15$, $F_F < 1$, $BF_{01} = 11.66$.

4.4.3. Discussion

Experiment 3 served to examine whether the order in which the auditory stimuli are presented influences temporal binding. Therefore, we designed an experiment with three types of predictability of the letter sequences – predictable, shuffled, and random.

Similar to Experiment 1, we found temporal binding for both actions and events. However, there was no action binding in conditions with shuffled letter sequences. A comparison between the three types of sequence predictability, however, did not reveal any significant differences in temporal binding. Therefore, both the predictable sequence and the random sequence appear to be suitable for measuring temporal binding with our setup.

It is, however, worth noting that the presentation of the scales, which participants used to make their time judgements, differed between the conditions. This is a result of the study design, as participants always made their temporal judgements on a scale of 5 + 1 letters. While the scale in the predictable condition was always the same (AFIOTA) it changed in the other two conditions. For shuffled letter sequences, participants also saw a scale, which had the same letter at the beginning and the end but was shuffled in-between according to the sequence. Hence, participants not only had to adjust to a new letter sequence every trials but also a newly arranged scale. Similar flexibility was demanded in the random condition, only this time, participants saw only a snippet of the entire letter sequence which contained the objectively “correct” letters as well as at least one more element to the left and the right. Thus, scale presentation might have influenced participants’ performance and judgements in these conditions.

Surprisingly, sequence predictability did not notably influence participants’ task load. They rated their task load to be about equally high in all three conditions. The only item that was influenced by sequence predictability was participants’ perceived performance. Participants rated their task completion to be better in the predictable and shuffled condition compared to the random condition.

To sum it up, implicit temporal binding measures suggest that either predictable or random letter sequences are suitable measures for temporal binding. Considering participants’ subjective ratings on performance, which tend to be lower for random sequences, give an indication to using predictable or shuffled letter sequences. Therefore, we decided to stick with a predictable sequence for future studies.

4.5. Experiment 4: Manipulation of sequence length

In Experiment 4, we systematically varied the sequence length, that is, how many different letters constitute the auditory timer. There were three different sequence lengths: 5 items, 10 items, and 15 items (for more details see Apparatus and stimuli.). As longer sequences should result in weaker retention of the sequence in working memory (cf. G. A. Miller, 1956), we expected both action binding and effect binding to decrease with increasing length of the letter sequence.

4.5.1. Methods

Participants. Forty-eight new participants (12 male, 2 left-handed) with a mean age of 24.77 years (*SD* = 5.42) who fulfilled the same criteria as in the other three experiments were recruited.

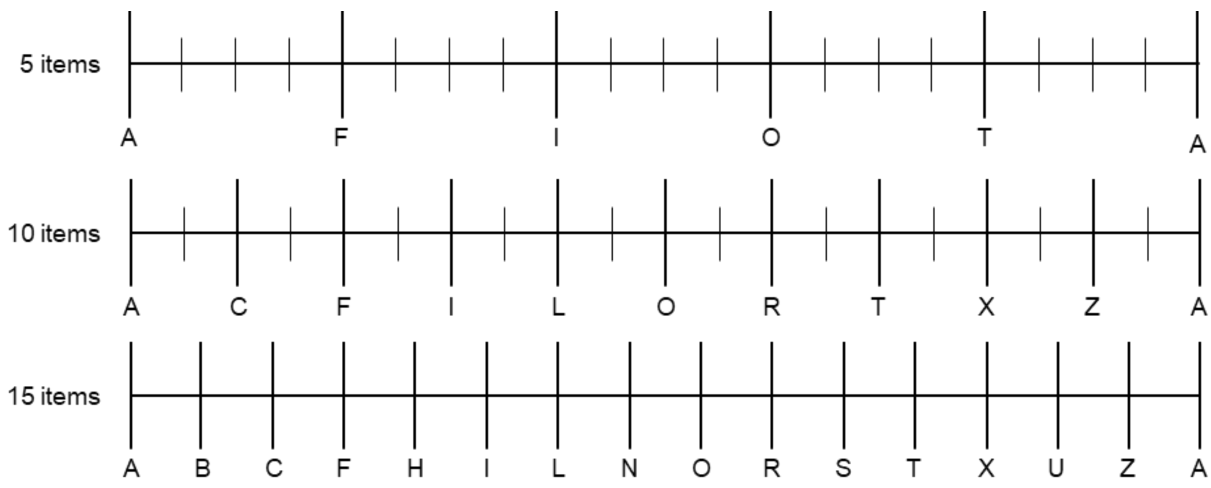
Apparatus and stimuli. The visual task was left unchanged from the other experiments. For the auditory timer, participants again heard the German letters A, F, I, O, and T over headphones. Now, we varied the length of the letter sequence, that is the number of letters in the sequence presented on three levels (5, 10, and 15) between blocks. These three levels were chosen, as the smallest amount of items should easily be remembered as healthy human can store at least 7 ± 2 items in their working memory (G. A. Miller, 1956). However, hearing the same five letters repeatedly might lead to frustration and boredom in the participants. Thus, we decided to present 10 letters as intermediate level. These 10 letters were A, C, F, I, L, O, R, T, X, and Z. After a few trials, participants should be able to remember the presented letters without too much effort. Contrary, 15 letters should appear to be a random sequence to participants, as they will probably never hear the entire sequence during the trials. The 15-letter sequence consisted of the following letters: A, B, C, F, H, I, L, N, O, R, S, T, U, X, and Z.

As alluded to in the discussion of Experiment 3, an altered letter sequence carries the effect of a changing scale for temporal estimations as well. In the previous experiment, we addressed this issue by always presenting the same scale resolution while changing the anchors, i.e., letters on the scale. This time, we decided to display the entire letter sequence at the end of every trials. Hence, in addition to the different levels of difficulty participants should have in remembering or getting attuned to the sequence, the resolution of the scale for temporal estimations decreased with increasing sequence length. The sequence lengths were set to 5, 10, and 15, so the scale for participants' estimations would visually remain the same while more letters were added for the longer sequence lengths. While the visual appearance of the five items scale was a scale with 6 anchors (AFIOTA) and three subdivisions each, the scale for 10 items displayed the respective 10 items plus the starting letter at the end. Each of these categories had one subdivision. The 15-item scale had no subdivisions and only displayed the 15 + 1 letters in sequence (see Figure 10). These adjustments of the scale resulted in the following resolutions: one pixel on the 5-item scale equaled 2.5 ms, while one pixel on the 10-item scale was equal to 5 ms and one pixel on the 15-item scale equal to 7.5 ms. Bottom line, during this experiment, participants always saw the entire sequence of letters when they gave their temporal estimation (see Figure 10).

Procedure. As the variable of interest in Experiment 4 was the length of the letter sequence, this factor was manipulated within subjects and we divided the experiment into thirds and assigned a specific sequence length (5, 10, and 15 letters) to each third. The order of sequence lengths was counterbalanced across participants. However, the manipulation of sequence length in this experiment involved changing the scale for time estimations as well. Apart from that, the procedure of Experiment 4 followed that described in Experiment 1.

Figure 10

Scale presentation for temporal estimations in Experiment 4



Note. The first row shows the scale presented when the auditory timer consisted of 5 items (1 px = 2.5 ms). In the middle the 10-items scale is presented (1 px = 5 ms), and at the bottom, the scale consisting of 15 letters (1 px = 7.5 ms).

Design. The study used a 3 × 4 repeated-measures design with sequence length (5 items vs. 10 items vs. 15 items) and condition (action experimental vs. action baseline vs. effect experimental vs. effect baseline) as within-subjects factors.

4.5.2. Results

Data analyses in Experiment 4 followed that described in Experiment 1.

Temporal binding. Erroneous trials (0.4%) and outliers, trials in which temporal binding exceeded 2.5 SDs of the participant's cell mean (3.1%), were excluded from the analyses.

Action binding. Participants only judged their action to be shifted towards the effect in blocks where the sequence consisted of 5 items (see Figure 11A). That is actions were perceived to have happened later in the 5 items condition, $t(47) = 3.02$, $p = .004$, $d_z = 0.44$, $\Delta = 34.56$ ms, but not when the letter sequence consisted of 10 items, $t(47) = 1.34$, $p = .186$, $d_z = 0.19$, $BF_{10} = 0.71$, $\Delta = 9.21$ ms, or 15 items, $t(47) = 1.33$, $p = .191$, $d_z = 0.19$, $BF_{10} = 0.70$, $\Delta = 6.55$ ms (see Figure 11A).

The ANOVA for action binding with sequence length (5 items vs. 10 items vs. 15 items) as within-subjects factor revealed a significant difference in the magnitude of action binding between the three sequence lengths, $F(2,94) = 3.75$, $p = .027$, $\eta_p^2 = .07$. That is, action binding was significantly larger in the 5 items condition compared to the 10 items condition, $t(47) = 2.05$, $p = .046$, $d_z = 0.30$. However, there was no clear evidence for or against a difference between the 10 and the 15 items conditions, $t < 1$, $BF_{01} = 2.59$.

Effect binding. Cursor movements in all three conditions were perceived to be shifted towards the preceding action, $t_{5items}(47) = -4.84$, $p < .001$, $d_z = 0.70$, $\Delta = -116.47$ ms, $t_{10items}(47) = -4.48$, $p < .001$, $d_z = 0.65$, $\Delta = -47.75$ ms, $t_{15items}(47) = -4.15$, $p < .001$, $d_z = 0.60$, $\Delta = -36.74$ ms. That is, cursor movements were perceived to have happened earlier when a keypress preceded this cursor movement.

The ANOVA for effect binding with sequence length (5 items vs. 10 items vs. 15 items) as within-subjects factor showed a significant difference between the sequence lengths,

$F(2,94) = 13.32$, $p < .001$, $\eta_p^2 = .22$. The temporal shift in perception was significantly larger in the 5 items condition than in the 10 items condition, $t(47) = -3.61$, $p < .001$, $d_z = 0.52$, while there was no clear evidence for or against a difference of effect binding in the 10 and 15 items conditions, $t(47) = -1.18$, $p = .244$, $d_z = 0.17$, $BF_{01} = 1.63$.

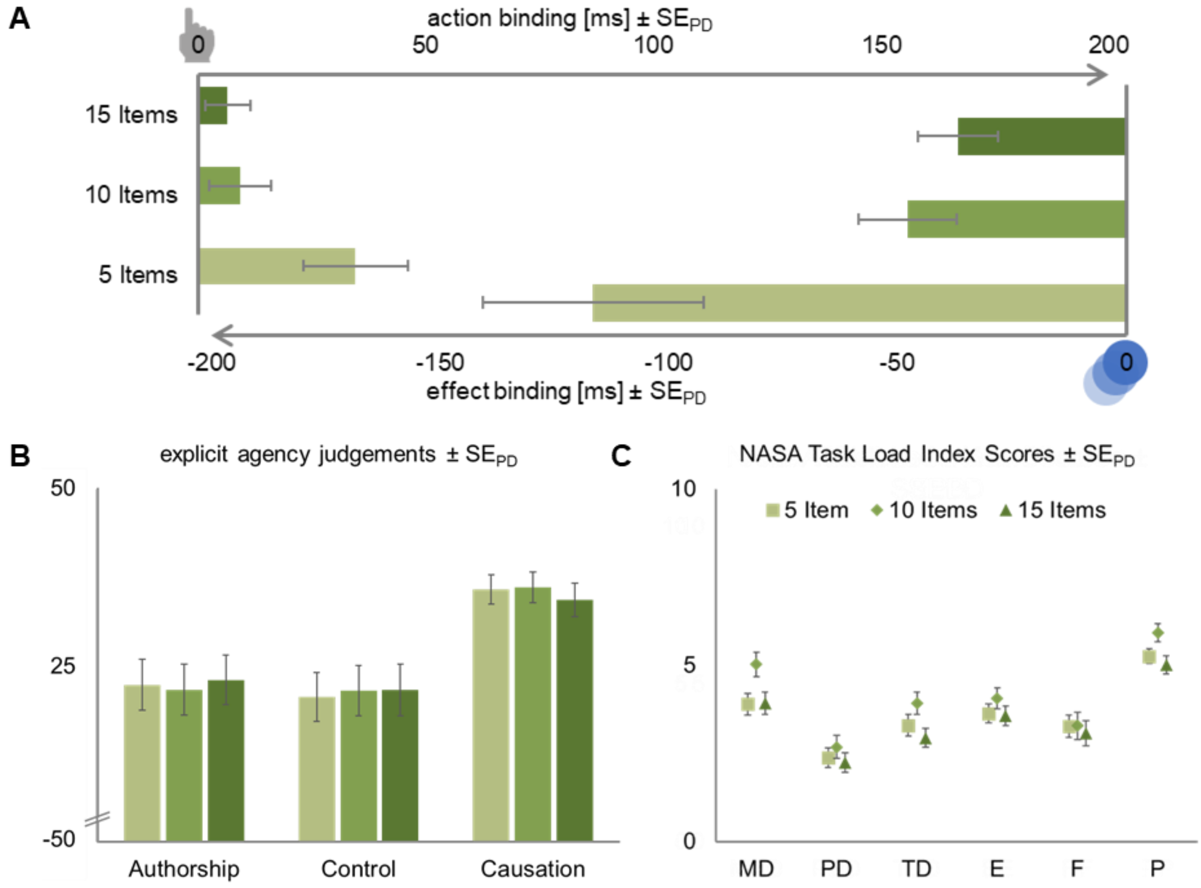
Explicit agency judgements. As in the other experiments, there was no significant difference in judgements of agency between action experimental and effect experimental conditions, $F < 1$, $BF_{01} = 12.45$. Thus, explicit agency judgements were calculated across conditions. Again, agency ratings were high for all three types of judgement, authorship ($M = 22.20$, $SD = 23.78$), control ($M = 21.13$, $SD = 23.07$), and causation ($M = 35.37$, $SD = 13.96$).

Subsequently, three repeated-measures ANOVAs with sequence length (5 items vs. 10 items vs. 15 items) as within-subjects factor were conducted. Explicit agency judgements did not differ significantly between the different sequence lengths, all Fs < 1, all $BF_{01} > 7.96$ (see Figure 11B).

NASA Task Load Index. Data showed a significant effect of sequence length on temporal demand, $F(2,94) = 3.26$, $p = .043$, $\eta_p^2 = .07$. Temporal demand gradually increased with sequence length. However, it only differed significantly between the 5items condition and the 15 items condition, $t(47) = -2.66$, $p = .011$, $d_z = 0.38$ (see Figure 11C). Data did not provide clear evidence for or against a difference in mental demand between the three sequence lengths, $F_{MD}(2,94) = 2.91$, $p = .060$, $\eta_p^2 = .06$, $BF_{01} = 1.34$. No other effects of sequence length were observed, all other $Fs < 1$, $BF_{01} > 7.28$.

Figure 11

Temporal Binding, explicit agency judgements, and NASA TLX scores in Experiment 3



Note. (A) Action binding and effect binding relative to the baseline condition. The y-axis intercept denotes the perceived timing of the action (top) and the perceived timing of the effect (bottom) in the respective baseline conditions. Action binding is shown as bars from left to right to indicate the perceived delay of the action. Effect binding is shown as bars from right to left to indicate the perceived advancement of the effect. Error bars depict standard errors of paired differences for the factor sequence length (Pfister & Janczyk, 2013). (B) Explicit agency judgments for authorship, control, and causation of the cursor movement. Agency judgments were made on a scale from -50 to 50 after every eighth trial in all experimental conditions. (C) Perceived task load as measured with the NASA Task Load Index (Hart & Staveland, 1988). MD: mental demand, PD: physical demand, TD: temporal demand, E: effort, F: frustration, P: performance. Squares represent participants' judgments for sequences of 5 letters, diamonds 10 letters, and triangles for sequences of 15 letters. All error bars depict standard errors of paired differences for the factor sequence length (Pfister & Janczyk, 2013).

4.5.3. Discussion

In Experiment 4, we tested whether the sequence length of the auditory stimuli influences temporal binding. Therefore, we designed an experiment with three lengths of the letter sequences, 5 items, 10 items, and 15 items. Sequence length had a notable effect on both action binding as well as effect binding. Contrary to our hypothesis, participants only showed action binding in the 5 items condition while effect binding, even though present in all three conditions, was drastically reduced for medium and long sequences. The implemented variation of sequence length carried the effect of an altered scale and scale resolution as well. Therefore, reduced temporal binding in the two longer sequence conditions could have resulted from the different scale presentation or participants' estimation strategies. The absolute length of all three scales was equal, however, the 5 and 10 items scales had additional visual markers as subdivisions on the scale (see Figure 10) resulting in the possibility to give more fine-grained estimations. While it was easy to predict the entire sequence in the 5 items condition, it was a lot harder for sequences consisting of 10 items and almost impossible for the longest sequence of 15 items. We therefore suggest that participants might have tried to locate both the actions and effects with respect to both the preceding and ensuing letter in the 5 items condition. Contrary, in the two longer sequence conditions, participants probably only used the previously heard letter as anchor for their estimation. Additionally, we presume that it is a lot easier to refer to experienced events rather than anticipated events, therefore, it is not surprising that estimations in the 10 and 15 items conditions show less variability than in the 5 items condition. Taken together, this accentuates the importance of using a scale which allows participants to give more fine-grained estimations.

4.6. General discussion

With the present line of experiments, we investigated different design factors to establish an auditory measure for temporal binding. Specifically, we systematically manipulated four factors of the timed auditory letter sequence that served as auditory timer. These were interval length (250 ms, 500 ms, 750 ms), interval filling (filled, half-filled, sequenced), sequence predictability (predictable, shuffled, random), and sequence length (5 items, 10 items, 15 items). Overall, the setup that we used produced robust temporal binding for both actions and effects, which is a crucial for the development of an alternative measure. Based on previous studies using a visual Libet Clock to measure temporal binding, both the absolute temporal binding as well as the standardized effect sizes we discovered were to be expected (e.g. Moore & Obhi, 2012; Ruess, Thomaschke, Haering, et al., 2018; Schwarz, Weller, Pfister, & Kunde, 2019). If anything, effect binding seemed to be slightly larger than in previous studies, but it was consistent across all four experiments (N = 192) reported here. These observations make the auditory timer a potent means for measuring temporal binding, as it is possible to record participants' perception of events timed to the millisecond. Recently, a new way of interpreting temporal binding in terms of multisensory cue integration has arisen (Kawabe et al., 2013; Legaspi & Toyoizumi, 2019; Lush et al., 2019). According to the authors, temporal binding can be explained by integrating and weighting information about planned actions and perceived sensory events. To make inferences about participants' judgements, the method used for measuring temporal binding has to be precise with a high resolution. In line with this, we found that temporal binding was mostly influenced by the characteristics of the interval and the sequence length but not so much by the presentation order of the letters. Consequently, the characteristics of the auditory timer should be adapted according to the research purpose. Single letters should be easy to discriminate and the letter sequence should be of a length which can be displayed with a good spatial resolution on the screen, i.e., 1 px should account for only few milliseconds of the auditory sequence.

Our attempt to use a previously employed auditory timer with an interval length of 250 ms (Cornelio Martinez et al., 2018) revealed higher task load and frustration compared to an interval length of 500 ms, which appeared to be a good interval length for letter discrimination. Additionally, this constitutes a cycle length of 2500 ms, which makes the auditory timer more comparable to the visual timer used in standard Libet Clock experiments (Schwarz et al., 2018; Schwarz, Weller, Klaffehn, & Pfister, 2019; Weller et al., 2017).

Another factor to be considered is whether the task configuration, i.e., a set goal and the lack of freedom to choose an action influenced participants' temporal estimations. Previous research concentrating on the influence of goal attainment on explicit judgements of agency found that goal attainment increased judgements of agency even if participants did not actually achieve the goal by themselves (Dewey et al., 2010). In addition, Barlas et al. (2017) conducted a study in which participants either had to press a certain button or could freely choose from up to four different buttons. Results showed that freedom of choice increased both temporal binding as well as explicit agency judgements (see also Barlas & Obhi, 2013). In that light, our forced choice setup may would have reduced temporal binding, supporting the robustness of the present findings. Thus, the influence of the task setup is an interesting factor for future research.

Throughout all experiments, participants explicitly rated their sense of agency to be high. Such high agency judgements might be due to the simplicity of the task, the cursor movement always followed participants' keypresses and the very low error rates show that participants had no difficulty in completing it. These observations are in line with previous research indicating that participants take credit even for successful events which they are not entirely responsible for (Dewey et al., 2010). These ratings did not differ between actions and effects. Schwarz, Weller, Klaffehn, and Pfister (2019) suggest that participants' ratings for causation over outcomes should in general be higher than ratings over the responsibility for a distinct action. However, in their study, the questions that participants had to answer in blocks where the timing of the action had to be estimated were different from those in blocks in which the timing of the effect had to be estimated, while the questions in our experiments were the same in all blocks. Nevertheless, the importance of causality is supported by our observation that participants generally rate their causation higher than authorship and control. This is possibly due to the fact that from childhood on, healthy individuals make a lot of assumptions about their causality on a daily basis as the decision whether it was me or not comes fairly natural (Blakey et al., 2019; Wegner, 2003). Contrary, we do not always reflect on our authorship and control over events when they happen as expected. Additionally, agency judgements in the present study reflect a general judgement of agency generated over eight trials, whereas implicit feelings of agency were recorded after each trial. The lack of variation in the explicit agency measures might also be explained by the idea that the implicit and explicit measures for sense of agency, i.e., temporal binding and agency judgements, probably rely on different mechanisms and therefore do not necessarily have to correlate (Dewey & Knoblich, 2014).

Note that all recommendations for the design of an auditory timer to measure temporal binding are based on the particular task presented in this study, that is moving a cursor through a 3 × 3 grid, and this was tested on an iPad only. Further research is necessary to investigate whether or not our conclusions generalize to other tasks and input devices. Up until then, we suggest that the presented recommendations can be used to make informed design choices that are affecting the detection of any given effects to different extents. Therefore, every parameter should be selected carefully. Please do also note that the recommendations given above are not to be taken as “gold standard” for designing any auditory timer, rather we grant that different methods are suitable for different research questions.

The paradigm that we used to elicit temporal binding is a rather basic task with a simple action and visual effect. It still needs to be further evaluated whether the setup is also suitable for even more visually demanding tasks. Additionally, in the current study we only varied one factor at a time (except for Experiment 4 in which the manipulation of the sequence length was confounded with the resolution of the estimation scale), neglecting any possible interactions that might accompany certain design choices. We have shortly alluded to some of these possible interactions in the discussions of the respective experiments, e.g., how the length of the letter sequence influences scale presentation. Therefore, our design recommendations are specific for each design factor. However, combinations of other manipulations might come with additional benefits or pitfalls.

To conclude, we found that most of the tested design choices were in principal able to detect temporal binding. Thus, the proposed auditory timer appears to be quite robust to variations within certain ranges and can widely be employed to study temporal binding for visual demanding tasks.

III. Empirical synopsis: Choosing how

5. Temporal binding in multi-step action-event sequences is driven by altered effect perception

The perceived compression of the interval between a voluntary action and a subsequent consequence is termed temporal binding and serves as an implicit measure for sense of agency. In everyday life, oftentimes multiple actions are required for goal attainment, i.e., a multi-step sequence of actions has to be performed to evoke the desired effect. However, present-day research mainly assesses the sense of agency for single actions and effects. Preliminary research on the sense of agency in longer action-event sequences is inconclusive. To fill this gap, we studied temporal binding in multi-step action-event sequences. In two experiments (free and forced choice), we employed a temporal binding paradigm in which participants had to press two keys to evoke the corresponding effects. Overall compression of the interval between actions and effects was driven by strong effect binding for both effects, while there was no significant action binding in either of the experiments.

5.1. Introduction

When humans act to produce perceptual changes in their environment, they experience agency. The sense of agency is thought to comprise of two components, judgements of agency and feelings of agency (Synofzik & Vosgerau, 2008). While the former are conceptualized as explicit statements about a person's agency, the latter reflect a non-conceptual implicit sense of agency. The most common measure for explicit judgements of agency is to ask participants whether they caused a sensory event or not (e.g. Sidarus et al., 2013; Weller et al., 2017). These judgements of agency are thought to rely on retrospective evaluations of a person's causal beliefs. However, such judgements are highly susceptible to various biases and can be influenced by manipulating, for example, the contingency between an action and its consequence (Daprati et al., 1997; Moore, 2016).

Feelings of agency are more difficult to assess, as they are thought to reflect prospective aspects of the sense of agency. Haggard and Tsakiris (2009) proposed temporal binding as an implicit measure of sense of agency (see also G. Hughes et al., 2013). Temporal binding describes the phenomenon that the interval between causally linked sensory events is perceived as shortened (e.g. G. R. Humphreys & Buehner, 2009; Tsakiris & Haggard, 2003). Agency research draws on this observation as the perceived time points of voluntary actions and subsequent effects are believed to shift towards each other, which cannot occur when either of the two entities (action or event) happens in isolation. Specifically, the perception of the action is shifted towards the effect (= action binding) while the perceived timing of the effect is shifted towards the action (= effect binding). These perceptual shifts were initially thought to rely on a successful comparison of predicted and actual consequences as proposed by the comparator model (Blakemore et al., 1999). However, more recent research has shown that the intention to produce an event is no prerequisite for temporal binding, which questions its validity as a pure index of sense of agency, and points instead to the importance of causal relations and multisensory cues for this phenomenon to occur, (e.g. Buehner, 2012; Kirsch et al., 2019). Hoerl et al. (2020), for example, propose a framework for the sense of agency based on causal links that exist both with and apart from voluntary actions. This framework also considers causality beliefs as well as multisensory cues.

Research on multisensory integration explains temporal binding based on the perceptual certainty of the events to be judged, i.e., actions and effects (Ernst, 2006). Action shifts are typically substantially smaller than effect shifts, which in many cases mirrors the difference in perceptual certainty between action and effect (Cao et al., 2020; Ernst & Banks, 2002; Kirsch et al., 2019). While there are various cues such as the planning of a body movement or the proprioceptive feedback of that movement which provide actors with a fairly certain estimate of the actual timing of the action, there are far fewer temporal cues for the effect. Thus, the less certain event is shifted towards the more certain event (cf. Moore & Fletcher, 2012). This finding has been replicated in numerous studies using different task setups, stimuli, effects, and action-effect delays (for an overview see Moore & Obhi, 2012). All of these studies relied on a similar task setup using the Libet clock (Haggard et al., 2002; Libet et al., 1983). Participants watch a clock with a rotating clock hand on a screen while performing keypresses at leisure. In an operant block, these keypresses are followed by an effect, usually a tone. At the end of a trial, participants are asked to report the position of the clock hand at either the time of their action or the effect. In addition to the operant blocks, there are two baseline blocks in which participants either only press a key without eliciting an effect, or they only encounter the effect without prior action. Temporal binding is calculated by comparing the estimation accuracy in the baseline condition with that in the operant condition for action and effect separately.

While agency has been studied extensively for single action-event episodes, much less is known about agency in multi-step actions. However, such action sequences resulting in goal attainment are rather common in our daily life. Oftentimes, we do not expect our actions to have immediate results, or we only find out whether an action sequence was successful when the final goal state is reached. In multi-step actions, several sequential activities are required to produce a certain intended effect. For example, opening a safe requires several turns of the knob before the bolt snaps back and the door eventually opens. Such sequential activities thus include multiple actions and multiple action effects, which become apparent after the final action has been carried out. Goals in multi-step action sequences may be represented at different levels of abstraction, very broadly as simply attaining a certain effect, e.g., reaching the goal, or very narrow as motor commands, e.g., moving a limb. This is what Pacherie (2008) defined as D-, P-, and M-intentions. Distal intentions are directed at overall goals such as opening the safe while proximal intentions and motor intentions break these D-intentions down into sub-goals and finally immediate motor intentions required to specify the respective motor commands for turning the knob (Pacherie, 2008). While it appears easy and straightforward to assess explicit judgements of agency over more complex action-effect sequences, it is not trivial to study implicit feelings of agency for the same sequences as it is not yet clear which mechanisms underlie, for example, temporal binding and how stable the effects are over time. The question we addressed here is how much agency agents experience for individual actions and action effects in such multi-step action-event sequences.

Preliminary studies approaching this question from different perspectives and by relying on different measures yielded somewhat diverging results. For example, Ruess, Thomaschke, Haering, et al. (2018) studied sequences of one action and two effect tones. They found that even unintended effects occurring shortly after an intended effect are bound to the preceding action. In their experiments, the interval between the action and an intended tone was always fixed, whereas a second tone could follow at a random or fixed interval. In line with previous studies, temporal binding was strongest for fixed intervals, but random delays of 200-800 ms between the two tones also produced perceptual shifts of both effects towards the action. However, effect binding for the second tone was always smaller than for the first tone. The authors attribute this observation to the prolonged overall delay between the action and the second tone which in general diminishes temporal binding (Ruess, Thomaschke, Haering, et al., 2018).

Another study investigating the sense of agency for multiple actions rather than multiple action effects measured sensory attenuation (Garrido-Vásquez & Rock, 2020). Participants performed one single or multiple keypresses to elicit a tone. The authors found sensory attenuation, as another implicit measure of sense of agency, to increase with the number of keypresses required to elicit an effect. However, explicit agency ratings were higher for single actions than for multiple actions.

In yet another preliminary study, Yabe et al. (2017) reported a decomposition of temporal binding in action-event sequences. Participants experienced either action-sound-action triplets or sound-action-sound triplets. In the action-sound-action condition there was neither temporal binding of the sound to the preceding nor the ensuing action, whereas in the sound-action-sound condition both sounds were perceptually shifted towards the action. Yabe et al. (2017) interpreted this observation as a perceptual decomposition of the triplet into two dyads, each necessarily including an action as an anchor. Consequently, both sounds were attracted by the intermediate action in the sound-action-sound condition, whereas an intermediate sound was equally attracted towards both flanking actions in the action-sound-action condition.

Imaizumi et al. (2019) found opposing results in longer sequences of alternations between actions and events. Participants actively or passively pressed a key which produced a tone, and then pressed a key again. One alternation sequence consisted of five keypresses interlaced with four tones. The study employed the interval estimation method, rather than time perception of individual events, as measure for temporal binding. Thereby, the perceived intervals between any two occurrences in the action-event sequence could be assessed. The results suggest different time perception biases on a local (dyadwise) and global (sequencewise) level. When comparing active and passive keypress conditions, there was no temporal binding in individual action-effect dyads within the action-event sequence, whereas there was a perceived shortening of the sequence in total, i.e., temporal shrinkage for the whole sequence. Thus, the active or passive movement manipulation did not seem to have an influence on the perceived duration of the individual intervals, whereas the overall length of the full sequence was perceived to be significantly shortened. These results however raise the question for underlying mechanisms that cause these observations. The paradigm employed by Imaizumi et al. (2019) seems ill-suited for further analyses as the interval estimation method does not allow to disentangle the perceived time point of individual events.

To sum up, in slightly different multi-step action setups different observations have been made. First, when one action produces two effects, effect binding for the second of two effects decreases with the absolute length of the interval between action and the second of two effects

(Ruess, Thomaschke, Haering, et al., 2018). Second, explicit agency for an effect drops when it is produced by multiple rather than one action (Garrido-Vásquez & Rock, 2020). Third, effect binding occurs only when that effect is not followed by another action (Yabe et al., 2017). Fourth, in longer action-effect-action-effect sequences, there is no local temporal binding (consistent with Yabe et al., 2017), but possibly a shortening of perceived overall sequence duration (Imaizumi et al., 2019).

In the present paper, we explored the perceived timing of actions and effects in a situation in which multiple actions produce corresponding individual effects following the final action. This is a quite common scenario in many social and technical settings. For example, it is common in many imitation situations, where a model demonstrates certain multi-step actions, which are then carried out by an observer, and which can thus be construed as an effect of the model's action (e.g. Pfister et al., 2013). Uttering individual words (e.g., "I solemnly swear…") which are then repeated by a counterpart at an inauguration is an example of verbal imitation. An example in technical environments might be turning the dial on a safe to first unlock and then open it. All in all, these interactions, social or non-social, are characterized by the peculiarity that we do not expect effects from single motor actions but rather from a sequence of multiple actions, e.g., words or gestures. The question we asked here is how sense of agency shapes the perceived time points of actions and effects in such scenarios.

In two experiments, we examined the perceived timing of two actions evoking corresponding effects at the end of the action sequence. We manipulated the level of choice between the two experiments to keep the task as similar as possible but still change the way the action sequence was represented – as one predetermined sequence or as individual freely chosen actions leading to a defined goal. Representing actions as individual freely chosen keypresses should render both the actions as well as the effects more separate as compared to having one sequence where the actions are less distinguishable. In the present study participants moved a cursor through a grid by pressing arrow keys (cf. Figure 12). Each trial consisted of two keypresses that were afterwards followed by the two respective effects (action-action-effect-effect). Previous research on admittedly somewhat different situations suggests the following: If duration perception dilates on a local scale but compresses on a global scale (Imaizumi et al., 2019), we should find that the perceived timing of the first action moves to a later point in time, and the perceived timing of the last effect moves to an earlier point in time. The perceived timing of intermittent events should be left unaltered. However, if temporal binding in sequences is just a concatenation of binding dyads, temporal binding should occur between the second action and the first effect (Yabe et al., 2017) but not necessarily between the first action and the second effect. If temporal binding is, however, merely influenced by the absolute interval length between an action and any subsequent effect, the perceived timing of the first effect should be more biased towards the action than the perceived timing of the second effect just as the second action should show stronger action binding than the first one (Ruess, Thomaschke, Haering, et al., 2018).

5.2. Experiment 1: Forced choice

Experiment 1 tested for temporal binding in multi-step action-event sequences when participants had to follow a defined path in a grid with cursor movements. That is, participants had to perform two keypresses to move a cursor from a start area to a goal area. Only if these two keypresses were executed correctly, the cursor moved to the targeted locations, otherwise, an error message was displayed. In agreement with the referenced literature on binding in multi-step sequences, we expected to find a global compression of the entire action-event sequence, i.e., effect binding at least for the second effect and action binding for the first action. However, the different studies lead to competing predictions regarding the intermittent action and effect, thus, we were especially interested in analyzing the perceived timing of these. Therefore, we chose a paradigm, which allowed to assess the perceived timing of each individual event in the sequence.

All experiments were preregistered on the Open Science Framework (OSF) and were approved by the ethics committee of the psychology department of the Julius-Maximilians-University of Würzburg (GZ 2019-09).

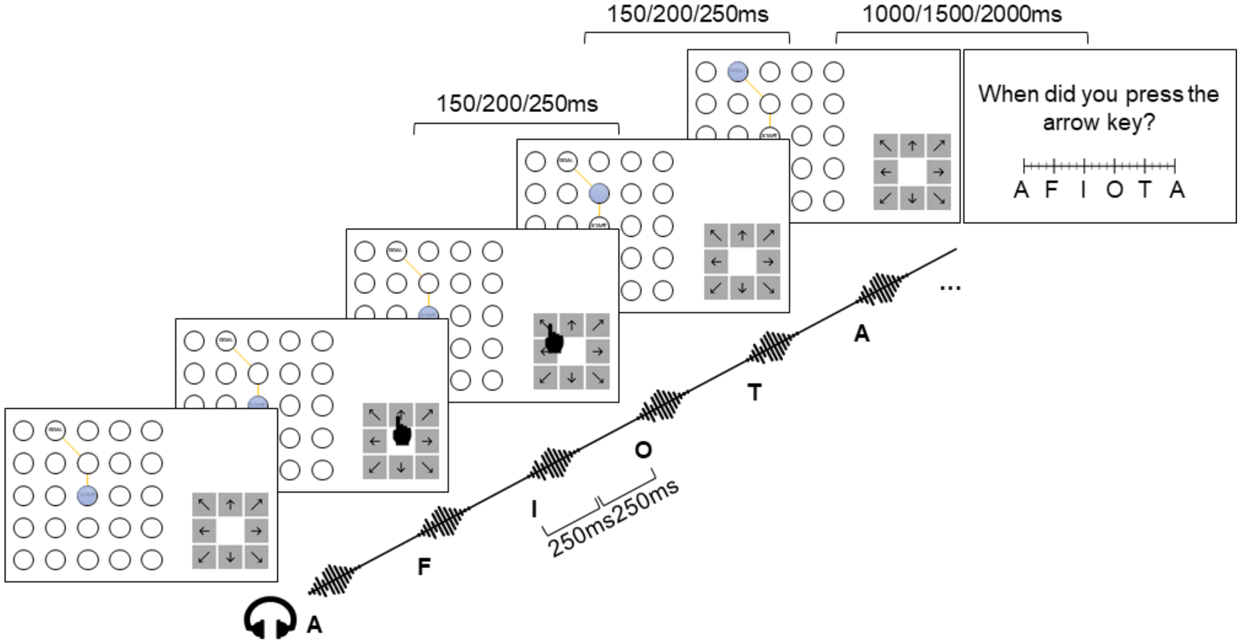
5.2.1. Methods

Participants. Forty-eight participants (16 male, 4 left handed, mean age = 23.9, *SD* = 4.0) recruited over the university's participant pool (SONA) took part in the experiment and received monetary compensation for their voluntary participation. Prior to data collection we conducted a power analysis for paired sample *t*-tests using G*Power 3.1 (Faul et al., 2009). Expecting medium effect sizes (e.g., Ruess et al., 2017), the power analysis was performed with $d = 0.4$ and $\alpha = .05$. Consequently, 41 participants would have been required to ensure high power (.80). However, for counterbalancing of the conditions, we recruited 48 participants. Prior to the experiment, they signed an informed consent form. All participants were naïve to the purpose of the study and were debriefed afterwards.

Apparatus and stimuli. *Navigation task* | Participants were asked to navigate a cursor through a 5 × 5 navigation grid by pressing arrow keys next to the grid (see Figure 12). In Experiment 1 the cursor had to follow a predefined route that was indicated by orange lines connecting the dots in the grid, thus it was a forced choice task with visual effects, i.e., the cursor movements. Participants completed the task on an iPad Pro using only the index finger of their right hand. The iPad's LED screen, with a 12.9'' diagonal and a resolution of 2732 × 2048 px, was used in landscape mode. We chose the iPad for task execution to eliminate undesired consequences such as the proprioceptive feedback as well as possible auditory effects from pressing and releasing keyboard keys. As shown in Figure 12, during task execution the screen showed a 5 × 5 grid of circles with a diameter of 100 px on the left part of the screen while a keypad with 8 arrows was displayed to its right. The keys were located slightly to the bottom of the display for ergonomic reasons. At the beginning of each trial, the center circle displayed the German word for start ("Start") and was filled in blue to illustrate a moveable cursor. Additionally, one of the outer 16 circles (except for the circles in the four corners) in the grid displayed the German word for goal ("Ziel"). The two areas were connected with straight orange lines indicating the keypresses that participants had to perform in the respective trial.

Figure 12

Trial procedure in the two experiments



Note. Participants navigated a cursor (blue circle) through the grid by pressing the arrow keys located next to it. During task execution, participants heard letters over headphones that served to report the timing of either one of the keypresses or one of the cursor movements.

Auditory timer **|** Over the course of the trial, participants heard a loop of five timed letters over headphones. These were the German letters A, F, I, O, and T, which served as a timer to reference the perceived timing of the keypresses and cursor movements. This was done by indicating the exact position on a visual scale displaying all letters at the end of each trial – as shown in the last frame in Figure 12. Participants were explicitly instructed to use the entire scale including the spaces between the letters to make their judgement as accurate as possible. Each of the letters was presented for 250 ms followed by 250 ms silence resulting in a loop length of 2500 ms[5]. This auditory timer presentation resulted in a temporal resolution where one pixel of the visual scale was equal to 2 ms. A representative example of the auditory timer as well as the exact instructions are available at this project's OSF page (https://osf.io/6xv29/; see also Appendix I).

[5] For more details on the method, see Muth et al. 2021.

Figure 13

Schematic overview of the experimental conditions in the experiment

Note. Participants experienced 8 different conditions throughout the experiment. Four conditions in which they were asked to judge the timing of their action (1–4) and four conditions in which we assessed the perceived timing of the effects (5–8). Blue icons indicate the event to be reported in the respective condition.

Procedure. The experiment consisted of one session lasting approximately 1.5 hours. We used a procedure similar to the so-called Libet clock procedure (Haggard et al., 2002; Libet et al., 1983). However, instead of presenting a visual clock for the time estimations, we used an auditory timer, which allows to examine temporal binding for visually demanding tasks (Muth et al., 2021).

Throughout the experiment, participants encountered eight different conditions (see Figure 13): (1) Action1 operant: Cursor movements followed participants' keypresses and the perceived timing of the first keypress was assessed. (2) Action1 baseline: Participants executed the required keypresses. However, they were not followed by any cursor movement. The perceived timing of the first keypress was assessed. (3) Action2 operant and (4) Action2 baseline followed the same logic as the first two conditions but in these conditions, the perceived timing of the second keypress was assessed. (5) Effect1 operant: Cursor movements followed participants' keypresses and the perceived timing of the first cursor movement was assessed. (6) Effect1 baseline: After a random interval of 1500-2500 ms the cursor movement along the predefined path occurred without participants' keypresses and the perceived timing of the first cursor movement was assessed. (7) Effect2 operant, and (8) Effect2 baseline followed the same logic as the two Effect1 conditions; however, in the latter, the perceived timing of the second cursor movement was assessed. The order of conditions was counterbalanced across participants.

Trials started with the presentation of the navigation grid on the left, the keypad on the right side of the screen and the letter sequence over headphones. The sequence's starting letter was selected randomly. Participants saw the start and goal area and the path to follow. Paths included all possible paths to any of the outer circles excluding those that required pressing any arrow key twice. Consequently, there were 24 different trials with two repetitions per block, i.e., 48 trials per block. At the beginning of the first block, participants performed eight practice trials which were not presented during the following experimental blocks to familiarize with the task. These practice trials were all response repetitions, i.e., trials in which they had to press the same key twice. Additionally, the first two trials of the experimental blocks were also response repetitions serving the purpose of letting participants adjust to the new condition. We selected these paths to ensure that participants saw each path in the experimental blocks equally often. Practice trials were not included in the final analysis.

In the operant blocks and action baseline blocks, participants were asked to wait at least three letters and then press the two corresponding arrow keys at their leisure. Once both keypresses were executed in the operant conditions, the cursor moved first to the location of the first keypress and then to that of the second keypress. The cursor movement followed the second action with a random delay of 150, 200, or 250 ms and was displayed until the onset of the second cursor movement. The second cursor movement followed the first one with the same delay and was also displayed for either 150, 200, or 250 ms. In the action baseline conditions, the auditory timer continued for a random interval of 1500, 2000, or 2500 ms after the second keypress. In the effect baseline conditions, participants were to wait for the cursor movement without performing any action. Here cursor movements started 1500-2500 ms, i.e., 3 to 5 letters, after the auditory timer started with a random delay of 150, 200 or 250 ms between the cursor movements. If participants did not succeed in pressing the two correct keys, an error message reading the German word for error (“Fehler”) in red was displayed in front of the grid for 750 ms and the trial aborted. Trials concluded with presentation of the visual scale of 1250 px width with 6 large tick marks for each letter of the auditory sequence as shown in Figure 12. Note that the “A” was displayed twice so participants could make judgements both before and after the “A”. Participants were asked to report the timing of either one of the two actions or one of the two effects by selecting the corresponding position on the scale. Temporal estimations were used to calculate temporal binding for both actions and both effects (see Data analysis. for more detail). Temporal binding was calculated as difference in estimation error between baseline and operant conditions.

Additionally in operant blocks, participants answered three questions on a visual scale from -50 to 50 rating their authorship, control, and causation over the cursor movements every eight trials. These judgements served as manipulation check, to confirm participants’ perceived causality and control over the events. As mentioned above, such explicit agency ratings are susceptible to demand effects and are thus not the focus of our research interest.

Data analysis. Firstly, we calculated estimation errors as the difference between temporal estimation and actual timing of the respective event. We discarded erroneous trials, that is, trials in which participants did not succeed in pressing both correct keys. Additionally, trials in which estimation errors exceeded 2.5 SDs of the participant’s cell mean in the respective condition were also dropped from the analyses. Secondly, we calculated means for each estimation condition individually. Subsequently, we subtracted each participant’s mean estimation error in the baseline condition from that in the respective operant condition to obtain temporal binding. Positive values can be interpreted as perceived shift to a later point in time, while negative values indicate that an occurrence was perceived to have happened earlier in the operant than in the baseline condition. Two-tailed paired *t*-tests were calculated for each action and effect separately. Effect sizes for all *t*-tests were computed as $d_z = \frac{t}{\sqrt{n}}$.

Additionally, post-hoc Bayes analysis were used to inspect the evidence for and against the null hypothesis. Bayes factors with a scale parameter of 0.707 were calculated using JASP computer software (JASP Team, 2018). As per convention, a Bayes factor of $BF_{10} < \frac{1}{3}$ can be interpreted as evidence in favor of the null hypothesis, while Bayes factors greater than 3 yield at least moderate evidence for the alternative hypothesis (Dienes, 2014).

5.2.2. Results

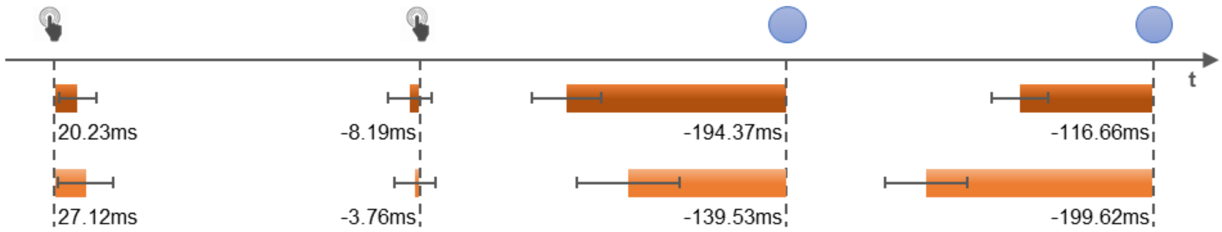
Errors (3.1%) and outliers (2.9%) were very rare. Consequently, error rates will not be further analyzed here (see P. Dixon, 2008 for comments regarding floor and ceiling effects in the analysis of error data). Descriptively, errors occurred mainly in the action baseline conditions and happened more often for the second than for the first action. This is not very surprising, as participants had to perform both actions while the cursor remained at the starting position. Thus, on some trials, they pressed the first key again, indicating that they might not have been sure whether the first keypress was recorded. Additionally, they had to anticipate the first cursor movement to execute the second keypress correctly. The manipulation check was successful. Explicit agency judgements were high across almost all participants, $M_{\text{authorship}}$ = 24.9 (20.9), M_{control} = 24.8 (21.2), $M_{\text{causation}}$ = 31.9 (19.5), indicating that participants indeed felt authorship and control over the cursor movements.

Action binding. There was no significant difference in mean estimation errors between the operant and the baseline conditions neither for the first action, $t(47) = 1.19$, $p = .237$, $d_z = 0.17$, $\Delta = 20$ ms, $BF_{10} = 0.31$, nor for the second action, $t(47) < 1$, $\Delta = -8$ ms, $BF_{10} = 0.17$. Follow up Bayes analyses indicated at least moderate evidence for the null hypothesis in both cases. Additionally, the observed results did not differ between the two actions, $t(47) = 1.15$, $p = .166$, $d_z = 0.17$, $\Delta = 28$ ms, $BF_{10} = 0.29$, indicating moderate evidence for the null hypothesis. Consequently, we did not observe a perceived shift of either action towards the ensuing effects.

Effect binding. Data showed significantly larger constant estimation errors for operant conditions compared to baseline conditions for both effects. As shown in Figure 14, the first effect was perceived to have happened earlier in blocks where participants had to press keys to move the cursor compared to conditions in which participants simply observed the cursor movements, $t(47) = -6.34$, $p < .001$, $d_z = -0.92$, $\Delta = -194$ ms, $BF_{10} = 1.74e^5$. The same held true for the second cursor movement. There was a perceptual shift of the second cursor movement towards an earlier point in time when the cursor movements were preceded by participants' keypresses, $t(47) = -4.70$, $p < .001$, $d_z = -0.68$, $\Delta = -117$ ms, $BF_{10} = 903.14$. However, the magnitude of these two shifts differed significantly, i.e., the forward shift of the first cursor movement was larger than that of the second cursor movement, $t(47) = -2.68$, $p = .010$, $d_z = -0.39$, $\Delta = -78$ ms, $BF_{10} = 3.79$.

Figure 14

Temporal binding in Experiments 1 & 2



Note. Temporal Binding in Experiment 1 (top) and Experiment 2 (bottom). Action binding and effect binding relative to the respective baseline conditions. Dotted lines mark the perceived timing of the occurrence in the baseline conditions. Bars going from left to right indicate a perceived delay of the occurrence, while bars going from right to left indicate a perceived shift to an earlier point in time. Error bars depict standard errors of paired differences (Pfister & Janczyk, 2013).

5.2.3. Discussion

In Experiment 1 we aimed at studying temporal binding in sequences of two actions and two effects. We did not observe a shift of the perceived timing of actions, neither of the first nor of the second one. Contrary, we did observe effect binding for both effects, which was stronger for the first than for the second effect.

Interpreting the non-significant action binding in the light of multisensory integration would suggest smaller variability of estimation errors in the Action2 operant condition compared to the Action2 baseline condition as in the former agents should have more perceptual certainty due to additional temporal cues in form of the preceding action and the ensuing effect. A post-hoc analysis[6] contradicted this notion, variances were larger in the operant compared to the baseline condition. However, estimation errors for the first action did indeed show a numeric trend in the predicted direction. That is, the first action in the operant condition was descriptively shifted to a later point in time. However, this shift was merely marginal.

To shed further light on this, a second experiment was conducted in a free choice instead of forced choice design. Previous studies have found temporal binding to be stronger in an interval estimation task when participants could freely choose one of four actions to execute compared to situations in which the action was forced upon them (Barlas et al., 2018). This may increase the chances to observe specifically action binding for which Experiment 1 may have been too insensitive to detect (but see Schwarz, Weller, Klaffehn, & Pfister, 2019 for a lack of influence of free vs. forced choice).

5.3. Experiment 2: Free choice

Experiment 2 was designed to test for temporal binding in sequences of multiple actions and subsequent effects when participants freely chose how to get from a start to goal area. To this end, participants could perform any two keypresses which would result in the desired end position of the cursor. If this was the case, the cursor moved along the path chosen by the

[6] Analysis and reports can be retrieved from the project's OSF page: https://osf.io/2vey8/

participant, otherwise, an error message was shown. In contrast to Experiment 1, where participants might have represented the two keypresses as one predefined action chunk, we expected participants to maintain the final goal of reaching a certain target position of the cursor, while relying on more distinguished representations of the two actions. As free choice has been suggested to increase temporal binding, we expected the same to occur for this now (partly) free choice design as compared to Experiment 1.

5.3.1. Methods

Participants. A new set of forty-eight participants (14 male, 6 left-handed) with a mean age of 24.6 years (*SD* = 7.5) who fulfilled the same criteria as in Experiment 1 were recruited.

Apparatus and stimuli. The task was the same as in Experiment 1, only this time, there was no connecting line from the start to the goal area and participants could thus freely choose which two keys to press to reach the goal area.

Procedure. In principle, the procedure of Experiment 2 followed that of Experiment 1 with the difference that this time the path from start to goal area was not defined. While start and goal location were set, participants could choose freely how to navigate from one to the other. As in Experiment 1, the goal location could be any of the outer 16 circles sparing the four corners. Participants were asked not to press arrow keys twice so that each of the resultant 12 goal positions could be reached in two ways as in the first experiment. Each block consisted of 48 trials plus two practice trials at the beginning of the block to familiarize with the new condition. Additionally, this time we included a practice block of 16 trials at the beginning of the experiment which was the same for all participants, it was an Action1 operant block and goal locations were the four circles located right above, to the left and the right, and below the start area. Practice trials were not included in the analysis.

During trials, participants were asked to wait at least three letters before performing two distinct key presses (no repetitions) which would move the cursor to the goal position. Timing, cursor presentation, and the scale for temporal judgements were the same as in Experiment 1.

Data analysis. We analyzed the data in Experiment 2 according to the analysis plan described in Experiment 1.

5.3.2. Results

Errors (3.3%) and outliers (2.7%) again were rare and thus not further analyzed. Errors occurred mainly in action baseline blocks for the second action. However, it is noteworthy, that in this experiment, there were two first actions that were considered correct, as there were always two ways to get from start to goal area. Again, explicit agency judgements were high across almost all participants, $M_{\text{authorship}}$ = 32.7 (19.5)M_{control} = 29.7 (24.1), $M_{\text{causation}}$ = 36.6 (17.3), suggesting that participants felt authorship and control over the cursor movements.

Action binding. Figure 14 shows the estimation errors in the operant conditions relative to the baseline conditions. There was no significant difference in the size of estimation errors between baseline and operant conditions neither for the first action, $t(47) = 1.09$, $p = .282$, $d_z = 0.16$, $\Delta = 27$ ms, $BF_{10} = 0.27$, nor for the second action, $t(47) < 1$, $\Delta = -4$ ms, $BF_{10} = 0.16$. Bayes factors indicated moderate to strong evidence for the null hypothesis. A comparison between the perceived shifts of both actions did not show any significant difference, $t(47) = 1.05$, $p = .297$, $d_z = 0.15$, $\Delta = 31$ ms, $BF_{10} = 0.26$.

Effect binding. Estimation errors in the operant condition were significantly larger than in the baseline condition for both cursor movements. Hence, the perception of the first effect was shifted towards the preceding actions, $t(47) = -3.05$, $p = .004$, $d_z = -0.44$, $\Delta = -140$ ms, $BF_{10} = 9.03$, likewise, the perception of the second cursor movement was also shifted to an earlier point, $t(47) = -5.52$, $p < .001$, $d_z = -0.80$, $\Delta = -200$ ms, $BF_{10} = 1.17e^4$. A comparison between the effect binding for the first and second effect did not reach significance, $t(47) = 1.31$, $p = .198$, $d_z = 0.19$, $\Delta = 60$ ms. However, the post-hoc Bayes analysis did not find strong evidence for or against the null hypothesis, $BF_{10} = 0.35$.

Between experiment analysis. Following the two experiments, we conducted a between-experiment analysis to compare action binding as well as effect binding and see whether choice (free vs. forced) had an influence on the reported temporal binding. We expected choice to increase temporal binding.

To test this, we conducted a 2×2×2 mixed ANOVA with experiment as between-subjects factor and event (action vs. effect) and position (1st vs. 2nd occurrence) as within-subjects factors. Whenever there was a violation of the sphericity assumption, we applied Greenhouse-Geisser correction.

Results only partly supported our hypothesis. Letting participants freely choose which path to take did indeed influence temporal binding. However, it did not lead to a general increase of binding. Not surprisingly, there was a significant main effect of event (action vs. effect), $F(1,94) = 57.61$, $p < .001$, $\eta_p^2 = 0.38$, which did however not interact with the factor experiment, $F(1,94) < 1$. In contrast to this, there was no significant main effect for position (first vs. second), $F(1,94) < 1$. However, the interaction of position with experiment was significant, $F(1,94) = 4.35$, $p < .040$, $\eta_p^2 = 0.044$. This interaction was driven by the reversal of the pattern for effect binding. Thus, even though freedom of choice had an influence on the reported estimation errors, it did not increase temporal binding in general.

5.3.3. Discussion

Experiment 2 was designed to facilitate temporal binding and elucidate the results found in Experiment 1 by introducing a free choice component to result in more separated action and effect representations while maintaining the same goal. We replicated most of the findings from Experiment 1. We again observed effect binding for both the first and the second effect. That is, the perceived timing of both these cursor movements was shifted towards the preceding actions. However, this time, there was no difference in the magnitude of the perceived shift between the first and second cursor movement. If anything, the perceived time point of the second effect moved more towards the preceding actions than that of the first effect. Thus, while the use of free choices shaped the degree of effect binding of the two effects, it had no influence on action binding. However, it is worth noting that the free choice manipulation we introduced might not have been as strong as in previous studies. To keep the experiments as similar as possible, we limited the number of possible choices to two by displaying a final goal to be reached. Hereby, participants could indeed select from two options for their first keypress, however, this choice restricted the second keypress to a single option.

5.4. General discussion

The present study examined temporal binding in action-effect sequences consisting of two actions followed by the same number of effects. Participants navigated a cursor from a start to a goal area by clicking on arrow keys on a touchscreen. They then judged the timing of either a key press or a cursor movement, either when they occurred in isolation or as part of the action-event sequence. Between the two experiments, we manipulated whether the action sequence was predetermined, i.e., forced choice, or whether participants could freely choose which keys to press to get to the goal area.

Previous research has indicated that temporal binding does not only occur for single actions and subsequent effects, but also for single actions followed by multiple effects and sequences of alternating actions and effects. In line with these reports, we observed that the perceived timing of the cursor movements shifted towards the actions, if the actions preceded the cursor movements. However, we did not find a perceived action shift towards the ensuing effects. In addition, freedom of choice did not increase the overall temporal binding, rather it interacted with the serial order of the two effects. That is, with forced choice the first effect showed a greater temporal shift than the second effect while this pattern was non-significantly reversed with free choices.

Consequently, we could replicate results of previous studies showing that temporal binding does occur in multi-step sequences of actions and events. However, the architecture of these binding episodes appears to be more complex than previously suggested. According with the idea of global shrinking of the perceived time of an entire multi-step sequence (Imaizumi et al., 2019) we did observe that the final effect moves perceptually towards preceding actions. Yet, we barely found any action binding for the first action, while we found strong perceptual shifts of the first effect, which is inconsistent with idea of local dilation of perceived time between adjacent events. Moreover, we did observe that the first effect of a multi-step sequence is perceived as occurring earlier, which accords with the idea that multi-step actions are a concatenation of individual dyads (Yabe et al., 2017). Yet, we also observed large temporal binding for the second effect (numerically even larger than that for the first effect in Experiment 2), which is inconsistent with the construction of dyads between adjacent actions and effects. Finally, we did observe stronger binding of the first effect, consistent with the idea that the second of two effects of a preceding action shrinks because it is necessarily more separated in time to the preceding action(s) (Ruess, Thomaschke, Haering, et al., 2018). Yet, we did so only in forced choice conditions (Experiment 1) but not in free choice conditions (Experiment 2).

The specific use of temporal binding allows us to dig deeper into how agents implicitly reconstruct the causality of events in multi-step sequences. We consider two such reconstructions plausible. First, both effects might be construed to be caused by the second action (or an action-sequence consisting of nominally distinct but grouped actions – A1 and A2; cf. Verwey et al., 2015). In that case, temporal binding for the first effect should be significantly larger than for the second effect as proposed by Ruess, Thomaschke, Haering, et al. (2018). Second, the first action (A1) might be construed as causal for the first effect (E1) and the second action (A2) might be construed as causal for the second effect (E2). In that case, A1E1-binding should be smaller than A2E2-binding, as the absolute interval length between the first action and first effect was on average significantly longer than the absolute interval length between second action and second effect (592 ms and 400 ms). Results found in Experiment 1 are more consistent with the first model proposing that A2 was perceived as causal for both E1 and E2. Yet, although objective timing of effects was held constant in both experiments, causality was construed differently in Experiment 2. Here results were more in line with the second model, proposing that E1 was construed a consequence of A1, and E2 was construed a consequence of A2.

Why did this change in causal reconstruction occur? While in the first experiment, already the first cursor movement perfectly reliably indicated that and how the final goal area would be reached, the effect sequence was probably represented differently in the second experiment, i.e., as two individual effects of the chosen actions. During trials in the second experiment, participants had to make choices and probably form intentions to move the cursor along a path of their choice. Here, participants had to monitor both cursor movements more closely to ensure that they followed the freely chosen keypresses. Therefore, there was less reason to interpret the two cursor movements as one movement in the second experiment and more reason to follow the cursor movements more closely in the second experiment. Additionally, the causal link between first and second cursor movement might become more apparent in the second experiment, as in Experiment 1 participants knew how the cursor would move even before forming an intention or having pressed any key, whereas in Experiment 2, the first cursor movement determined participants' path to success. Thus, we assume that the causal link between the two cursor movements was stronger in Experiment 2 as compared to Experiment 1 (Hoerl et al., 2020).

While the sequence of cursor movements was predetermined in Experiment 1, it was not in Experiment 2, probably prompting cursor movements to grab more attention in Experiment 2. This would be in line with the results of Ruess, Thomaschke, Haering, et al. (2018) who found less binding for a second, irrelevant, effect of a single action. Apart from attention, the effects might also have acquired positive valence, which has shown to increase temporal binding (e.g., J. F. Christensen et al., 2016; Takahata et al., 2012; Yoshie & Haggard, 2013, but see also Moreton et al., 2017). E2 was more indicative of goal achievement (which likely comes with positive affect) in Experiment 2, whereas E1 was more indicative goal achievement in Experiment 1.

It is noteworthy that we did not replicate previous observations of a general increase of temporal binding when either the identity or timing of the actions is freely chosen rather than forced (Barlas et al., 2017, 2018). Note though, that recent studies have challenged this impact of free vs forced choice, suggesting that it is by far no ubiquitous observation (Antusch et al., 2021; Schwarz, Weller, Klaffehn, & Pfister, 2019).

5.4.1. Limitations

The presented study design comes with limitations. Most notably, we chose an action sequence as baseline condition, i.e., a situation in which participants had to perform two key presses that were not followed by a visual effect. It is possible that temporal binding might already occur between these two events in the baseline condition. Humans struggle to give the exact time point even of voluntary movements, especially when sensory certainty is very low (Wolpe et al., 2013). Any additional temporal cue e.g., internal cues, such as the plan to execute the movement or motor commands, and external cues, such as proprioceptive feedback of the tablet surface or subsequent events, influence judgements on these time points (Farrer et al., 2013). Therefore, it seems plausible that some kind of temporal integration occurred between the two actions even in the baseline conditions. Consequently, any binding towards subsequent effects that could possibly have occurred in the operant condition might have been overshadowed.

5.5. Conclusion

The present line of research investigated temporal binding in multi-step action-event sequences in a free choice and a forced choice paradigm. Results revealed temporal binding in both experiments. For the first time we could show that events at the end of multi-step motor sequences even with temporal delays are perceived as self-generated. This perceived compression of the interval between participants' actions and subsequent effects was due to the perception shift of the events, i.e., the effect binding. There was no action binding for either action in either experiment. This calls for further research on the mechanisms behind and the cognitive architecture of temporal binding per se and especially its extension to multi-step sequences.

IV. Empirical synopsis: Knowing where

6. Influence of spatially ambiguous feedback in multi-step sequences

Many of our daily actions require upfront planning and execution of multiple actions before their outcomes can be experienced. Sometimes action outcomes are ambiguous, for example due to obstruction or unintended side effects. The present study sheds light on the construal of the sense of agency in such ambiguous multi-step action-event sequences. Participants performed two keypresses to move a cursor from a start to a goal area. The subsequent feedback could be either in line with their actions or spatially ambiguous, i.e., accompanied by a second cursor movement, on either the first or second step towards goal attainment. As implicit measure for the sense of agency, i.e., the conception of the self as controlling agent, we measured temporal binding. Additionally, explicit agency ratings were collected. While feedback ambiguity only influenced temporal binding when the first cursor movement was spatially ambiguous, it had a systematic and differential effect on explicit agency ratings. Authorship and control ratings were drastically reduced with any kind of ambiguous feedback, whereas its influence on causation ratings was less pronounced.

6.1. Introduction

Many of our daily actions require accurate planning and execution of multiple motor commands before we receive feedback on whether our actions were successful or not. Nonetheless, we readily take responsibility for our actions and the ensuing event-sequences, i.e., experience a sense of agency (Haggard, 2017). Imagine preparing a cup of cappuccino for example. Selecting the cappuccino program on a fully automated coffee machine requires multiple keypresses, which trigger a sequence of events such as the espresso drizzling into the cup followed by the light oat milk froth. In this example, two distinct actions trigger an event-sequence that is comprised of two effects. But what happens if ambiguity is introduced at either of the steps in the sequence? What if we cannot be sure how or even whether a goal was indeed attained?

In this study, we aimed at scrutinizing whether and how the path to goal attainment influences implicit and explicit measures of the sense of agency. Previous studies have shown temporal binding for two effects which follow either a single action (Ruess, Thomaschke, Haering, et al., 2018) or two actions (Muth et al., 2022). However, the nature of these perceptual shifts is yet unclear. How is causality construed in such sequences? And what happens if the action causes two simultaneous effects, one of which is intended and the other unintended?

To test this, we designed a study in which participants received either unambiguous feedback in line with their prior actions or spatially ambiguous feedback where additional visual action-effects were presented along with the intended action-effects. Participants performed a navigation task on an iPad with the goal to move a cursor from a start to a defined goal area by pressing two keys. The cursor movement only started once two keypresses had been made.

In such multi-step action-event episodes Delevoye-Turrell et al. (2007) distinguish two kinds of sequences: triggered sequences and planned sequences. Triggered sequences are characterized by a succession of actions and events where feedback signals the end of an element in the sequence. The sequences used in the present study do not provide feedback after each action and thus rather correspond to planned sequences where one should be prepared to preplan the entire action sequence before its initiation. Consequently, it seems advantageous for the system to have a precise prediction about the motor processes involved in the action sequence (Delevoye-Turrell et al., 2007). If action sequences can be prepared in advance, this could lead to better performance as there is less interference between the currently executed action and the planning of the action to come (Nazari et al., 2017). Moreover, the subjective passage of time seems to be slowed down during action preparation (Hagura et al., 2012). With regard to the present study, we cannot be certain how and when participants plan the two to be executed keypresses and thus cannot control for perceptual biases due to other cognitive processes taking place simultaneously. Reaction time data from Muth et al. (2022) using the same task setup suggests that participants group their keypresses. Consequently, it is warranted to assume that actions would probably be executed as a chunk in this study, too, making their temporal resolution harder to disentangle. Another argument why the first and second keypress in such action sequences cannot be regarded as equal is provided by Neszmélyi and Horváth (2017). The authors have shown that actions producing an outcome are different from those not producing an outcome. When participants had to pinch a force sensor to either elicit a tone (motor-auditory condition) or not (motor condition) they applied more force when no auditory feedback followed their action (Neszmélyi & Horváth, 2017). In the present study participants had to press two keys to evoke the corresponding cursor movements after the second keypress making it one motor-only and one motor-visual keypress. Finally, several studies suggest that temporal binding for actions and effects is unrelated (Siebertz & Jansen, 2022; Tonn et al., 2021). Therefore, we decided to concentrate solely on perceived temporal shifts of the outcomes. We were particularly interested to see how causality is construed in such sequences and how this reflects in implicit and explicit measures of the sense of agency.

6.1.1. Predictions

In the main analyses, we pit two possible temporal binding patterns against each other. Depending on the assumed underlying mechanisms of temporal binding, two separate sets of hypotheses concerning the spatially ambiguous cursor movement can be postulated:

Forward model. If temporal binding is based on forward models (e.g., Wolpert et al., 1995; for a review see Waszak et al., 2012), effect binding in the spatially ambiguous condition should mirror effect binding in the spatially unambiguous condition. This is because the experienced sensory consequences, i.e., the movements of the cursor, match the predicted cursor movements. In this case, the additional cursor should not have a major influence on effect binding.

Multisensory integration. If multisensory integration is the underlying mechanism, temporal binding in the spatially ambiguous condition should be different to that in the spatially unambiguous condition. In this case, we expect effect binding for spatially ambiguous outcomes to be less pronounced than for unambiguous outcomes. The additional sensory information makes the ambiguous cursor movement more salient and therefore increases its perceptual certainty. Consequently, the perceived time point of the spatially ambiguous outcome should be less biased towards other events in the sequence. This will also be reflected by lower variances for the perceived time point of the ambiguous outcome compared to the unambiguous ones.

As regards explicit agency ratings, we expect participants to report lower levels of sense of agency for spatially ambiguous compared to spatially unambiguous outcomes as unpredictable or unintended action-effects typically result in lower agency ratings (e.g., Haering & Kiesel, 2015).

6.2. Experiment 1

In Experiment 1, we tested how spatial ambiguity of intermittent feedback influences the sense of agency in action-event sequences. Therefore, we manipulated whether participants saw only the action-effects that were perfectly in line with their prior actions or additional action-effects that were plausible but not identical to the intended ones. On each trial, participants had to press two arrow keys to move a cursor from a start to a goal area. Once the correct keys had been pressed, the cursor moved from the start to the goal area.

Depending on the mechanisms assumed underlying temporal binding two distinct hypotheses can be formed that are further detailed above. In addition, we expected participants to give lower agency ratings in blocks with spatially ambiguous feedback compared to blocks with spatially unambiguous feedback.

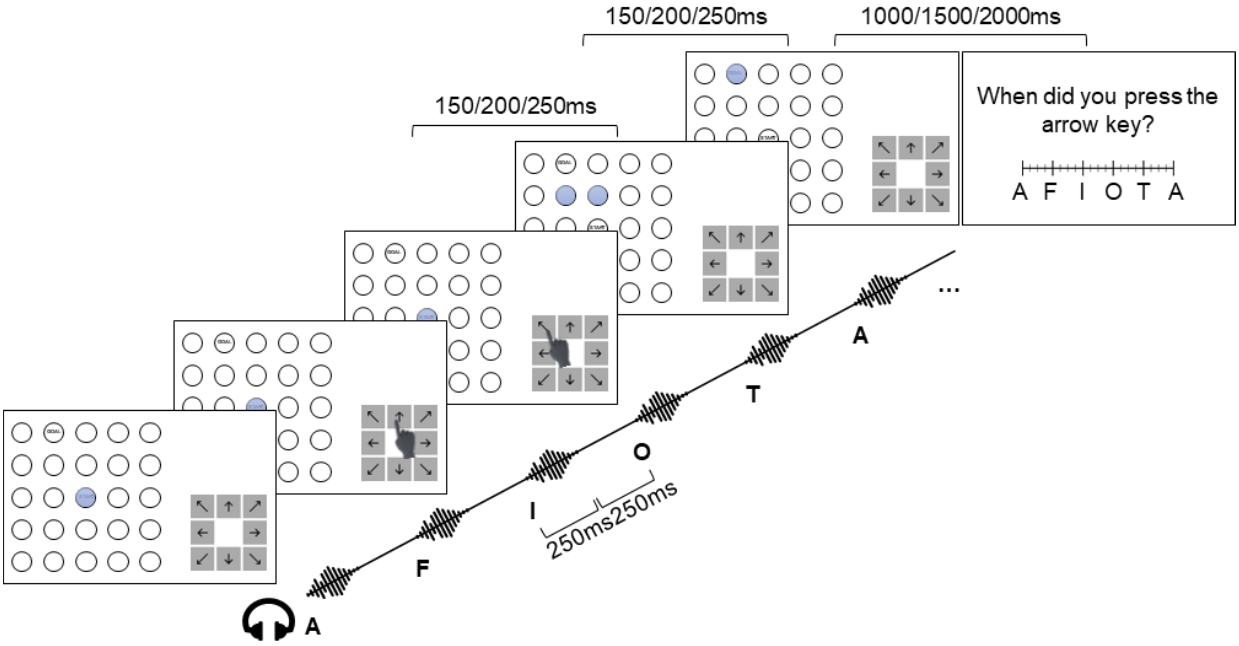
6.2.1. Methods

Participants. We recruited 48 participants between 19 and 58 years of age (*M* = 26.7, *SD* = 7.8, 34 females, 4 left-handed) over the university's participant pool (SONA) who received monetary compensation for their voluntary participation in the study. Prior to data collection, participants signed an informed consent form. All of them were naïve to the purpose of the study and debriefed afterwards. To determine the sample size, we conducted a power analysis for a repeated-measures ANOVA using G*Power 3.1 with an expected $f = 0.20$, $\alpha = .05$ and a power of .85 (Faul et al., 2009). This was based on effect sizes found in a previous study on temporal binding in action-event sequences (Muth et al., 2022; position-experiment interaction $\eta_p^2 = 0.04$). The proposed sample size of 39 should ensure high power ($1-\beta > .85$). For counterbalancing of the conditions, we added 9 participants to the final sample.

Apparatus and stimuli. *Task* | Participants completed a free choice task with visual action-effects on an iPad Pro. By pressing arrow keys, they moved a cursor (blue circle) from the start area in the middle of the grid to a set goal area on the outer nodes of the grid (see Figure 15). The task was completed using only the right index finger on an iPad Pro in landscape mode (12.9'' diagonal; 2732 × 2048 px resolution). By using the iPad, we minimized undesired consequences such as the proprioceptive feedback and possible auditory signals from pressing and releasing keyboard keys.

Figure 15

Trial procedure for a spatially ambiguous trial in Experiment 1



Note. Participants moved a cursor (blue circle) from a start to a goal area in a 5 × 5 grid by pressing arrow keys. Subsequently, the cursor moved according to the keypresses and cursor movements were either unambiguous or ambiguous (cursor appeared in two locations on first move). At the end of the trial, participants judged the time point of one of the cursor movements. Trial simulations are available on the project's OSF page (https://osf.io/f3swh/).

During task execution, the screen displayed a 5 × 5 grid of circles with 100 px diameter on the left side of the screen and a keypad with 8 arrow keys on the right. For ergonomic reasons, the keypad was located slightly to the bottom of the display. Trials started with the circle in the center displaying the German word for start ("Start") and one of the outer circles displaying the word goal ("Ziel"). The circle indicating the current cursor position was filled in blue to illustrate a moveable cursor. The goal area could be any of the outer 16 circles except for the four circles in the corners. Participants were asked to press two arrow keys to move the cursor from the start to the goal area and avoid pressing a key twice to reach the goal. Participants could start their keypresses whenever they chose to do so, however, they were instructed to wait at least three letters of the auditory timer, to get accustomed to it, before starting the keypresses.

Auditory timer | During task execution, participants heard a loop of five timed letters over the iPad's speakers serving as a timer to reference the perceived time point of the observed cursor movements (for details on the method see Muth et al., 2021). The letter sequence was played in German and comprised the letters A, F, I, O, and T. At the end of each trial, participants indicated the exact position of the cursor movement on a visual scale of 1250 px width with 6 large tick marks for each letter of the auditory sequence (see last frame of Figure 15). Each letter in the loop was presented for 250 ms followed by 250 ms silence resulting in a loop length of 2500 ms. The auditory timer had a temporal resolution where one pixel of the visual scale was equal to 2 ms. A representative example of the auditory timer is available at this project's OSF page (https://osf.io/4wkzu/).

Procedure. Trials started with the presentation of the cursor at the start position, a randomly selected goal area and the onset of the auditory timer. Throughout the experiment, participants encountered four operant and four baseline blocks. In the operant blocks, participants performed two keypresses of their choice that were then followed by the corresponding cursor movements. That is, once participants had pressed the two arrow keys, the cursor moved to the first corresponding location after a random delay of 150, 200, or 250 ms and continued to the final position after the same delay. If participants performed keypresses that would not result in goal attainment, they received an error message. The German word for error ("Fehler") was displayed in red letters on the screen for 750 ms.

In baseline blocks, participants were asked not to press any key but simply observe the cursor movement. Here, trials started with the presentation of the cursor at the start position, a randomly selected goal area and the onset of the auditory timer. However, this time the cursor movement to the first node started with a random delay of 1500-2000 ms and was followed by the second cursor movement with a random delay of 150, 200, or 250 ms. When participants pressed any arrow key in the baseline conditions, they received the same error message as in the operant blocks. At the end of each trial, participants were asked to report the exact time point of either the first or the second cursor movement by selecting the corresponding position on a visual scale displaying all letters (see Figure 15). Note that the "A" was displayed twice so participants could make judgements both before and after the "A".

The critical manipulation was the spatial ambiguity of the observed cursor movement. Here, we manipulated whether participants saw the cursor movement corresponding exactly to their keypresses or an ambiguous cursor movement where the corresponding node plus an additional adjacent node were filled in blue on the first step. On the second cursor movement, only the corresponding node was filled in blue.

The order of the blocks was counterbalanced across participants. However, all ambiguous blocks were presented in sequence and all unambiguous blocks were presented in sequence. The experiment consisted of one session with a duration of approximately one hour.

Data analysis. As a first step, we calculated estimation errors as the difference between participants' judged time points and the actual timing of the respective event in each condition. Error trials in which participants failed to press arrow keys that would eventually result in goal attainment or baseline trials in which participants pressed a key were excluded (2.4%). Additionally, we excluded trials in which the estimation error exceeded 2.5 SDs of the participant's cell mean for each condition individually (2.4%). Then, we calculated two-tailed paired t-tests between baseline and operant conditions for each position of the outcome (1^{st}, 2^{nd}) and spatial ambiguity separately. Effect sizes for all t-tests were computed as $d_z = \frac{t}{\sqrt{n}}$.

Second, to calculate temporal binding, estimation errors in the baseline condition were subtracted from those in the respective operant condition. To test the two hypotheses regarding temporal binding, we conducted a 2 × 2 repeated-measures ANOVA with effect position (1^{st} vs. 2^{nd}) and spatial ambiguity (ambiguous vs. unambiguous) as within-subjects factors and follow-up paired t-tests. Finally, to analyze whether the variances estimation errors differed between the conditions, we conducted a 2 × 2 × 2 repeated-measures ANOVA with spatial ambiguity, position, and block (baseline vs. operant) as within-subjects factors.

The explicit judgements of agency asking for control, causation and authorship over the cursor movement were formulated as a general judgement and not specific to a certain part of the cursor movement. Thus, we collapsed the ratings across blocks of the same spatial ambiguity to attain one explicit rating each for ambiguous and one for unambiguous feedback per participant. Finally, we conducted a 2 × 3 repeated-measures ANOVA with spatial ambiguity and agency judgement (control vs. causation vs. authorship) as within-subjects factors. Whenever the sphericity assumption was violated, we report Greenhouse-Geisser corrected statistics. Follow-up paired *t*-tests were conducted to examine individual differences.

6.2.2. Results

Temporal binding. Both spatially unambiguous cursor movements in the operant condition were perceived to have happened earlier than in the baseline condition, $t_{\text{first}}(47) = -3.47$, $p = .001$, $d_z = 0.50$, $\Delta = -78$ ms, $t_{\text{second}}(47) = -2.31$, $p = .025$, $d_z = 0.33$, $\Delta = -52$ ms. While the perceived time points of the cursor movements in the spatially ambiguous condition did not differ significantly between baseline and operant conditions, $t_{\text{first}}(47) = -1.71$, $p = .094$, $d_z = 0.25$, $\Delta = -45$ ms, $t_{\text{second}}(47) = 0.54$, $p = .591$, $d_z = 0.08$, $\Delta = 13$ ms.

The ANOVA revealed a main effect of position, indicating that temporal binding for the first cursor movement was stronger than for the second cursor movement, $F(1,47) = 4.25$, $p = .045$, $\eta_p^2 = .08$ (for absolute estimation errors see Table 1). Additionally, effect binding was larger in the unambiguous condition compared to the ambiguous condition, $F(1,47) = 5.08$, $p = .029$, $\eta_p^2 = .10$. The interaction between effect position and spatial ambiguity was not significant, $F(1,47) = 1.56$, $p = .218$, $\eta_p^2 = .03$. Even though temporal binding for the first effect was numerically smaller in the spatially ambiguous condition, the comparison between the two conditions did not reach significance, $t(47) = 1.42$, $p = .161$, $d_z = 0.21$, $\Delta = 32$ ms. In contrast, temporal binding for the second cursor movement was larger for the unambiguous compared to the ambiguous cursor movements, $t(47) = 2.37$, $p = .022$, $d_z = 0.34$, $\Delta = 65$ ms (see Figure 16A).

In the 2 × 2 × 2 repeated-measures ANOVA for analyzing estimation error variances with spatial ambiguity, position, and condition as within-subject factors, all main effects but no interaction was significant. Estimation error variances were significantly larger in the ambiguous condition compared to the unambiguous condition, $F(1,47) = 18.01$, $p < .001$, $\eta_p^2 = .28$.

Additionally, estimation error variances for the first cursor movement were larger than for the second, $F(1,47) = 11.80$, $p = .001$, $\eta_p^2 = .20$ and they were also larger in the operant compared to the baseline condition, $F(1,47) = 21.65$, $p < .001$, $\eta_p^2 = .32$.

Agency ratings. The 2 × 3 ANOVA with spatial ambiguity and judgement type yielded a main effect of spatial ambiguity, $F(1,47) = 25.07$, $p < .001$, $\eta_p^2 = .35$, indicating that agency ratings were generally higher in the unambiguous condition compared to the ambiguous condition (see Table 2). This held true for all three questions individually, $t_{authorship}(47) = 5.07$, $p < .001$, $d_z = 0.73$, $\Delta = 18.29$, $t_{control}(47) = 5.14$, $p = .001$, $d_z = 0.74$, $\Delta = 19.58$, $t_{causation}(47) = 3.73$, $p < .001$, $d_z = 0.54$, $\Delta = 10.75$ (see Figure 16B). The main effect of judgement type was far from significant, $F(2,94) = 1.38$, $p = .252$, $\eta_p^2 = .03$, $\varepsilon = 0.61$ (GG-corrected). However, the interaction between spatial ambiguity and judgement type reached significance, $F(2,94) = 10.35$, $p < .001$, $\eta_p^2 = .18$, $\varepsilon = 0.81$ (GG-corrected). This interaction resulted from a difference in agency judgements in the ambiguous condition. Here the causation rating was higher than both the authorship, $t(47) = 2.35$, $p = .023$, $d_z = 0.34$, $\Delta = 7.16$, as well as the control rating, $t(47) = 2.60$, $p = .012$, $d_z = 0.38$, $\Delta = 7.99$.

Table 1

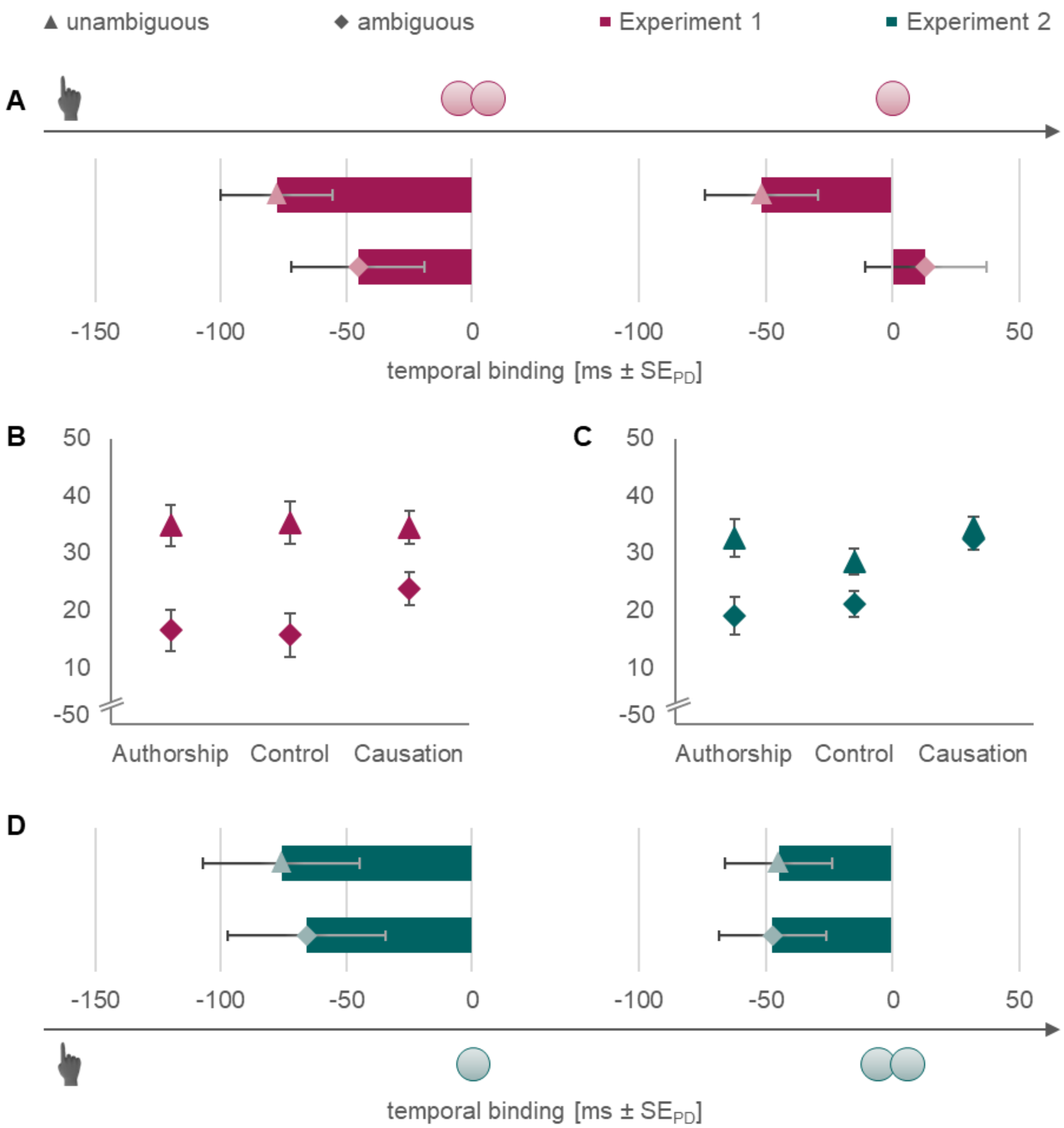
Mean estimation errors in ms (SD) for each condition in Experiment 1 & 2

Cursor movement		Unambiguous		Ambiguous	
		First	Second	First	Second
Exp. 1	Baseline	-219.68 (124.73)	-130.85 (149.12)	-215.57 (124.74)	-135.85 (131.67)
	Operant	-296.52 (159.56)	-179.18 (157.88)	-271.19 (167.99)	-153.03 (148.30)
Exp. 2	Baseline	-198.62 (126.27)	-107.01 (150.39)	-206.82 (123.77)	-97.75 (106.79)
	Operant	-274.56 (171.58)	-145.13 (138.51)	-272.70 (179.85)	-152.01 (151.10)

Note. Temporal binding is calculated by subtracting the estimation error in the baseline condition from the operant condition.

Figure 16

Temporal binding and agency ratings for spatially ambiguous outcomes



Note. Experiment is color-coded (red = Exp. 1; green = Exp. 2). Triangles (diamonds) show results for spatially unambiguous (ambiguous) outcomes. (A) Effect binding for first and second outcome relative to the baseline condition in Exp. 1. The y-axis intercept indicates the perceived outcome timing in the respective baseline condition. (B & C) Agency judgments for authorship, control, and causation of the cursor movement. (D) Effect binding for both outcomes relative to the baseline condition in Exp. 2. Error bars depict standard errors of paired differences for the factor position (Pfister & Janczyk, 2013).

6.2.3. Discussion

This first experiment was to test the influence of ambiguous intermittent feedback on temporal binding and judgments of agency. Participants' estimation errors in the operant condition compared to the baseline condition were larger when they received spatially unambiguous feedback about the cursor movement than when it was ambiguous. However, the difference in temporal binding for the first cursor movement was not significant, and variances were even higher in the spatially ambiguous compared to the unambiguous condition. This indicates that the additional sensory information provided on the first cursor movement was not sufficient to increase perceptual certainty. Temporal misjudgments of the action-effects were larger for the first than for the second cursor movement. Ruess, Thomaschke, Haering, et al. (2018) argue that temporally more distant effects produce weaker temporal binding, as the interval between the action and the respective effect is what determines binding strength.

Interestingly, spatial feedback ambiguity seemed to have a differential influence on participants' judgements of agency as compared to the implicit measure. While they reported lower authorship, control, and causation over cursor movements with ambiguous intermittent steps, the decrease in causation ratings was less pronounced than for the other two. This implies that participants still felt like they were producing the cursor movement, it just did not behave the way they wanted it to.

To further elucidate whether ambiguous feedback impairs temporal binding in general, we conducted a second experiment in which the first cursor movement was perfectly contingent to the participant's keypress while spatial feedback ambiguity was manipulated for the second cursor movement.

6.3. Experiment 2

While the movement of the cursor was informative about reaching the goal in the first experiment, this second experiment manipulated outcome ambiguity on the second cursor movement. Consequently, the cursor movement followed participants' keypresses but was spatially ambiguous in its end state.

6.3.1. Methods

Participants. We recruited a new sample of 48 participants between 20 and 60 years of age (M = 25.9, SD = 8.0, 38 females, 2 left-handed) over the university's participant pool (SONA) who received monetary compensation for their voluntary participation in the study. Prior to data collection, participants signed an informed consent form. All of them were naïve to the purpose of the study and debriefed afterwards.

Procedure. This experiment followed the same procedure as Experiment 1, however, this time instead of displaying two blue circles on the first cursor movement, participants saw two blue circles on the second cursor movement in the ambiguous condition.

Data analysis. All data was processed and analyzed as in Experiment 1. Exclusion criteria for error trials (1.7%) and outliers (2.2%) are listed above.

6.3.2. Results

Temporal binding. Paired t-tests for baseline and operant conditions revealed that all cursor movements were judged to have happened at an earlier timepoint in the operant as compared to the baseline condition. This was true both for spatially unambiguous cursor movements, $t_{first}(47) = -3.27$, $p = .002$, $d_z = 0.47$, $\Delta = -76$ ms, $t_{second}(47) = -2.58$, $p = .013$, $d_z = 0.37$, $\Delta = -45$ ms, as well as ambiguous cursor movements, $t_{first}(47) = -2.84$, $p = .007$, $d_z = 0.41$, $\Delta = -66$ ms, $t_{second}(47) = -2.76$, $p = .008$, $d_z = 0.40$, $\Delta = -47$ ms.

The ANOVA did not show any main effect and the interaction between position and spatial ambiguity did not reach significance either, largest $F_{position}(1,47) = 2.15$, $p = .149$, $\eta_p^2 = .04$, all other Fs < 1 (see Figure 16D). Looking at the variances of estimation errors, we again found larger estimation error variances in the ambiguous condition compared to the unambiguous condition, $F(1,47) = 4.83$, $p = .033$, $\eta_p^2 = .09$ as well as for the first compared to the second cursor movement, $F(1,47) = 8.22$, $p = .006$, $\eta_p^2 = .15$. Finally, temporal judgement error variances varied more in the operant compared to the baseline condition, $F(1,47) = 4.30$, $p = .044$, $\eta_p^2 = .08$. None of the interactions reached significance, $Fs \leq 3.89$, $p \geq .055$.

Agency ratings. Similar to Experiment 1, the 2 × 3 ANOVA with spatial ambiguity and judgement type as within-subject factors revealed a main effect of spatial ambiguity, $F(1,47) = 13.19$, $p < .001$, $\eta_p^2 = .22$, indicating that agency ratings were generally higher in the unambiguous condition compared to the ambiguous condition. Additionally, there was also a main effect of judgement type, $F(2,94) = 11.80$, $p = .001$, $\eta_p^2 = .20$, $\varepsilon = 0.85$ (GG-corrected). The interaction between spatial ambiguity and judgement type also reached significance, $F(2,94) = 12.77$, $p < .001$, $\eta_p^2 = .21$, $\varepsilon = 0.70$ (GG-corrected). Authorship and control ratings were higher in the unambiguous condition as compared to the ambiguous condition, $t(47) = 4.17$, $p < .001$, $d_z = 0.60$, $\Delta = 13.54$ and $t(47) = 3.29$, $p = .002$, $d_z = 0.47$, $\Delta = 7.41$, while there was no significant difference between the conditions for the causation ratings, $t(47) = 1.11$, $p = .273$, $d_z = 0.16$, $\Delta = 2.03$ (see Figure 16C).

Pooled analysis. In addition to the analyses reported above, we conducted pooled analysis to explore whether the stage of the ambiguous feedback had an in fluence on temporal binding or explicit agency ratings. Therefore, we ran the same analyses as stated above, but also included the factor experiment as between-subjects factor. To reduce redundancy, we will only report those results including this between factor.

Table 2

Mean explicit agency ratings (SD) in Experiment 1 & 2

		Unambiguous	Ambiguous
Experiment 1	Authorship**	34.83 (16.35)	16.54 (25.95)
	Causation**	34.45 (21.30)	23.70 (23.55)
	Control**	35.29 (16.26)	15.71 (27.38)
Experiment 2	Authorship**	32.59 (20.76)	19.05 (25.20)
	Causation	34.48 (22.36)	32.45 (22.77)
	Control*	28.57 (24.90)	21.16 (25.89)

Note. Participants rated their causation, authorship, and control over the cursor movement after every 8th trial on a visual scale from -50 to 50. ** $p < .001$, * $p < .05$.

Whether participants received spatially ambiguous feedback on the first or the second cursor movement did not have a significant influence on temporal binding as shown by the non-significant main effect of experiment, $F(1,94) < 1$. The between-subjects factor experiment did not interact with any of the other two factors either and there was no three-way interaction, largest $F_{\text{experiment×ambiguity}}(1,94) = 2.16$, $p = .145$, $\eta_p^2 = .02$.

In contrast, the stage of the ambiguous feedback did have an influence on explicit agency ratings. While explicit agency ratings were not significantly different between the two experiments in general, $F(1,94) < 1$, the experiment × spatial ambiguity interaction, $F(1,94) = 4.89$, $p = .029$, $\eta_p^2 = .05$ as well as the question × spatial ambiguity interaction, $F(2,188) = 19.58$, $p < .001$, $\eta_p^2 = .17$, ε = 0.89 (GG-corrected), indicated a differential influence of stage of ambiguous feedback on explicit agency ratings. In the unambiguous condition, participants' agency ratings did not differ between the questions, $F(2,188) = 1.12$, $p = .329$, $\eta_p^2 = .01$, ε = 0.72 (GG-corrected), regardless of the experiment, $F(2,188) = 1.94$, $p = .159$, $\eta_p^2 = .02$, ε = 0.72 (GG-corrected). In contrast, they did so in the ambiguous condition, $F(2,188) = 16.09$, $p < .001$, $\eta_p^2 = .15$, ε = 0.86 (GG-corrected). This effect was driven by the aforementioned higher causation ratings in both experiments when the feedback was ambiguous (see Table 2 for agency ratings).

6.3.3. Discussion

With Experiment 2 we wanted to assess whether spatial ambiguity of goal attainment influences temporal binding and agency ratings as implicit and explicit measures of sense of agency. While we found effect binding for both the first and the second cursor movement in the unambiguous as well as the ambiguous condition, it was not influenced differentially by spatial feedback ambiguity. That is, cursor movements were judged to have happened earlier when they were caused by own keypresses compared to just observing cursor movements without prior actions irrespective of spatial feedback ambiguity. Effect binding for the second cursor movement showed a non-significant tendency to be smaller than for the first cursor movement which is again in line with Ruess, Thomaschke, Haering, et al. (2018) observations that increasing delay between action and outcome decreases temporal binding. Previous work studying temporal binding in single action-outcome episodes found a similar decrease in effect binding with increasing delays (e.g., Haggard et al., 2002; Ruess et al., 2017).

6.4. General discussion

The present line of research examined whether spatial feedback ambiguity influences implicit and explicit measures of the sense of agency. Therefore, we conducted two experiments in which participants moved a cursor through a grid on an iPad. The critical manipulation of spatial feedback ambiguity was varied within participants, while the stage of the ambiguous feedback was varied between participants. We examined temporal binding as an implicit measure of the sense of agency and agency judgements as explicit measure.

As expected, both the first and the second cursor movement were perceived to have happened earlier when they were preceded by an action-sequence as compared to happening without prior actions. Additionally, temporal binding for the first cursor movement was significantly stronger than for the second cursor movement in the first and numerically larger in the second experiment. This corroborates previous findings showing the impact of temporal delay between action and outcome for temporal binding. This factor has already been studied extensively in single action-outcome links (e.g., Haggard et al., 2002; Ruess et al., 2017; for contradictory results using different methods see also G. R. Humphreys & Buehner, 2009; Wen et al., 2015a) as well as in one study examining temporal binding between one action and two subsequent action-effects (Ruess, Thomaschke, Haering, et al., 2018). The present observations suggest that even in sequences that could be construed as the first action causing the first outcome and the second action causing the second outcome, the temporal delay to the planned action-sequence (see Delevoye-Turrell et al., 2007) is decisive for the temporal attraction of the outcome(s) towards the action(s) (for a more detailed discussion see also General Discussion in Chapter III).

Regarding the influence of spatial feedback ambiguity on temporal binding, the two experiments provide mixed results. We intended to pit two possible underlying mechanisms against each other that would result in differential binding patterns. While the observed data pattern in Experiment 1, i.e., stronger temporal binding for unambiguous as compared to ambiguous feedback for both cursor movements, was not predicted by either mechanism, results of Experiment 2 support the forward model account rather than strict multisensory integration. Here, we did not find a difference in temporal binding between ambiguous and unambiguous feedback neither for the first nor for the second cursor movement. This either suggests that multisensory integration is not solely responsible for temporal binding to occur or that adding saliency, i.e., a second appearing cursor, does not increase perceptual certainty of this cursor movement. On the flip side, the idea that temporal binding depends on a forward model predicting the action's outcome (e.g., Moore & Obhi, 2012) has been challenged and at least in parts disproved. G. Hughes et al. (2013) propose that temporal binding is rather driven by the temporal controllability of action outcomes. Adding to this, in a meta-analysis Tanaka et al. (2019) found that temporal prediction and control contribute largely to temporal binding, while identity prediction does not. This possibly explains why we did not find a modulation of temporal binding by spatial feedback ambiguity, as the cursor movements were both temporally controllable and predictable, which might outweigh the cursor movement's unpredictable identity.

If this is the case, why was temporal binding different between ambiguous and unambiguous feedback in the first experiment? One possible explanation might be that causality links were more coherent and evident when the second cursor movement was ambiguous than when the first cursor movement was ambiguous. Considering only the spatially ambiguous condition, what was different between the two experiments is how one cursor movement could be construed as being caused by the previous one. Causality was evident and clear-cut in the second experiment, i.e., participants' keypresses lead to the first cursor movement which in turn resulted in a split cursor movement to two different end positions. In contrast, this was less so in the first experiment, where participants' keypresses caused a split cursor movement on the first step that might have resulted in less certainty as to which of the two cursors seen in the first step caused the second cursor movement and thus goal attainment. Hoerl et al. (2020) argue that temporal binding strength is determined both by proprioceptive cues and intentions to cause a certain event on the one hand, and causal background knowledge as well as contingency and contiguity cues on the other hand. In the same vein, Tramacere (2022) suggests a causal view on sense of agency and through this also temporal binding. According to her, a mental representation of the own action as cause for a specific consequence is necessary to evoke temporal binding. Even though her view might contrast the incidental experience of a sense of agency it is similar to the top down processes in Hoerl et al.'s (2020) model that primarily result in explicit agency attributions, which in turn influence temporal binding. Consequently, if the causality link between two events in the sequence is weakened, be it by temporal delay or spatial feedback ambiguity as in the present study, temporal binding is reduced (see also van der Weiden et al., 2013).

Additionally, spatial ambiguity in the present study was confounded with predictability, which might have played a role, too. Spatially ambiguous cursor movements were always unpredictable to a certain degree, as the cursor did not only move to the intended and thus predicted node, but also to an adjacent one. Results on the influence of predictability on temporal binding are mixed depending on the dimension of predictability. Some studies report temporal unpredictability to reduces temporal binding (e.g., Desantis & Haggard, 2016; Moore & Haggard, 2008; Thanopoulos et al., 2018) while others did not find an influence of outcome (identity) predictability on temporal sensitivity (Haering & Kiesel, 2014). In the present study, the outcome's spatial dimension was partly unpredictable in the ambiguous condition while effect delay was selected randomly from one of three possible delays (150, 200, 250 ms) but kept constant within a trial in all conditions. The fact that spatial feedback ambiguity solely influenced temporal binding in the first but not the second experiment again hints at the special role of the first ambiguous cursor movement. Considering the importance of the first cursor movement for a coherent action-event sequence, only the spatially unpredictable first cursor movement might have diverted attention and thereby weakened the causal links within the sequence. This argument is backed by the fact that the temporal binding of the second cursor movement was reversed in the first experiment, indicating a repulsion rather than an integration of the two events.

In contrast to the mixed findings in the temporal binding data, participants' explicit agency ratings provide a more coherent picture of perceived agency. Irrespective of the stage of ambiguous feedback, explicit ratings of authorship and control were reduced while this was not the case for causation. This implies that the agent explicitly distinguishes between having caused a sensory event and having controlled it. That is, participants did not take responsibility over ambiguous and therefore unintended cursor movements even though they knew that they had caused them. These observations corroborate an argument by Ueda et al. (2021) who found that participants' control ratings were not mirroring actual control, but rather biased by their task performance. That is, even though participants knew that they did not have full control over their cursor movement, they rated their agency to be higher when they performed better in a tracking task. However, control ratings abruptly dropped when task performance was implausibly good (high automation condition) (Ueda et al., 2021). Similarly, Sato and Yasuda (2005) were able to induce an illusory sense of agency when computer-generated tones were in line with the own expectations. Spatially ambiguous feedback does not only decrease consistency between the agent's intention and the actual outcome, it potentially also induces confusion regarding exclusivity of having caused the event (see also Wegner & Wheatley, 1999). This is especially the case when participants encountered the spatially ambiguous condition after having worked through the unambiguous one.

To sum it up, as long as deviances are below a certain mismatch-threshold, delayed action-effects as well as spatially distorted outcomes are readily self-attributed (Daprati et al., 1997; Farrer et al., 2003; Krugwasser et al., 2019; Synofzik & Vosgerau, 2008; but see also Metcalfe & Greene, 2007). This also counts for primed action effects (for a review see van der Weiden et al., 2013). According to Wen et al. (2015b) goal-directed inference is underlying these agency (mis-)attributions. In consequence, the phrasing of explicit agency questions should be tailored to the research question as it does make a difference whether an agent caused or controlled an event. In addition, the disparate decrease of agency ratings between the two experiments as well as between the different agency questions indicates that perceived agency is not binary as an on-off-mechanism but continuous. Kip et al. (2021) found causation ratings to be more fine-grained when goal-progress was visible. As stated before, the causality link between the first and second cursor movement was probably weaker when the first cursor movement was spatially ambiguous than when the second cursor movement was spatially ambiguous. Consequently, weaker causality links in addition to reduced goal-directed inference could explain reduced agency attribution for all three explicit agency questions (Hoerl et al., 2020; Wen et al., 2015b).

6.5. Conclusion

In two experiments we examined the influence on spatial feedback ambiguity on implicit and explicit measures of the sense of agency. Explicit agency ratings on the one hand were strongly affected by spatial feedback ambiguity. Participants rated their control, authorship, and causation of the cursor movement highest when the displayed movements were unambiguous and thus in line with the participants' prior actions. When the displayed outcomes were ambiguous, all three explicit ratings dropped, however, causation ratings maintained a high level. Spatial feedback ambiguity did not have such a distinct effect on temporal binding as implicit measure for the sense of agency. While temporal binding did not differ between ambiguous and unambiguous feedback for the second cursor movement, spatially ambiguous feedback for the first cursor movement reduced temporal binding. This may be due to attentional shifts and reduced causal certainty.

V. Empirical synopsis: Processing what

7. You do you: Susceptibility of temporal binding to self-relevance

The self-prioritization effect suggests that self-relevant information has a processing advantage over information that is not directly associated with the self. In consequence, reaction times are faster and accuracy rates higher when reacting to self-associated stimuli rather than to other-related stimuli (Sui et al., 2012). This spurs the assumption that self-associated action-effects should also be perceived earlier than other-related outcomes. One way to measure this is temporal binding. Previous research indeed showed that the perceived temporal interval between actions and self-associated outcomes was reduced compared to friend- and other-associated outcomes. However, the employed method (interval estimations) and several experimental design choices make it impossible to discern whether the perceived shortening of the interval between a keypress and a self-relevant outcome is due to a perceptual shift of the action or of the action-effect or both. Thus, we conducted four experiments to assess whether temporal binding can indeed be modulated by self-relevance and if so where this perceptual bias is located. The results did not support stronger temporal binding for self- vs other related action-effects. We discuss these results against the backdrop of the attentional basis of self-prioritization and propose directions for future research.

 Muth, F. V., Ebert, S., & Kunde, W. (under review). You do you: Susceptibility of temporal binding to self-relevance.

7.1. Introduction

When humans voluntarily act to cause a change in their environment and the intended change eventually presents itself a sense of agency for the action as well as the outcome arises (Haggard & Tsakiris, 2009). Studies have shown that the interval between such voluntary actions and subsequent action-effects is perceived as shortened compared to identical intervals caused by involuntary movements or third parties (Buehner & Humphreys, 2009; Haggard et al., 2002). This perceived shortening of the interval is referred to as temporal binding or intentional binding (for a review see Moore & Obhi, 2012). There is good reason to believe that outcomes which the agent has intended and therefore predicted are perceived somewhat differently than randomly occurring events in the environment. Consequently, temporal binding is widely employed as an implicit measure for sense of agency even though it is debated whether it is fair to do so (for a critical review see Buehner, 2012; Kirsch et al., 2019; Thanopoulos et al., 2018). Temporal binding is susceptible to various factors such as valence (J. F. Christensen et al., 2016; Moreton et al., 2017; Takahata et al., 2012) and control (Beck et al., 2017) just to name a few and relates to the sense of agency a person has in a specific situation. Other strands of research have focused on the origin of temporal binding as either a phenomenon of mere causality or as product of multisensory integration (Hoerl et al., 2020; Klaffehn et al., 2021). Finally, all this research converges on the observation that the interval between an action and a causally linked sensory event is perceived as shortened in comparison to identical intervals lacking causal or intentional links. Up-close, the perceived shortening is typically comprised of a forward shift of the action towards the sensory event and a backward shift of the sensory event towards the preceding action (for a discussion of time awareness of sensory events see Tsakiris & Haggard, 2005).

While the self and self-conception are at the center of the sense of agency, research in the field has mainly concentrated on the self-relevance of the action, i.e., whether an action was voluntary or involuntary (Haggard & Clark, 2003), whether it was freely chosen or forced (Caspar et al., 2018), and whether it was executed by the actor or not (Pfister et al., 2014). Other studies analyzed the effect of joint actions on the sense of agency in human-human pairings or in human-machine-interactions (for a review on social agency see Silver et al., 2021). Surprisingly, thus far little research has been published on the self-relevance of action-effects.

Self-relevant information is processed faster and reactions to self-relevant stimuli are less error-prone than reactions to stimuli which have not been associated with the self (Sui et al., 2012). In experiments probing the self-prioritization effect, participants are asked, mostly by instruction, to associate a random geometric shape with themselves, another one with a friend, and a third one with a stranger. Afterwards, they complete a classification task in which participants see a shape label pairing and have to decide whether shape and label match. Typically, responses in self-match trials are fastest and least error-prone (Sui et al., 2012). Self-prioritization has also been found for arbitrary ownership (Constable et al., 2019; Cunningham et al., 2008) in combination with valence and reward (Golubickis et al., 2021; Sui & Humphreys, 2015). It also extends to other outcome domains such as auditory and tactile stimuli (Schäfer et al., 2016) as well as generalized concepts, i.e., a music instrument presented visually or auditory, or a shape with varying characteristics (Schäfer et al., 2015). Self-related objects are not only processed faster but also perceived to be more valuable (Kahneman et al., 1991). Thus, G. W. Humphreys and Sui (2016) proposed a neural network of personal significance in which areas for self-referential processing interact with areas of attentional control (see Sui et al., 2013). Self-relevance speeds up the focusing of attention during decision making such that when self-relevant information is processed, the attentional spotlight narrows in on them a lot faster than when a target is not self-related (Golubickis & Macrae, 2021b). Self-prioritization does, however, not only reside on a central stage influencing attention and action selection, rather it also influences movement production and execution (Constable et al., 2011; Desebrock et al., 2018; Desebrock & Spence, 2021). Consequently, it seems plausible to assume that actions involving a higher degree of agency should, conversely, also be conceived as more self-relevant than simple automated acts (Wegner, 2002). But is this link bidirectional? Does a higher degree of self-relevance also lead to a stronger sense of agency?

Makwana and Srinivasan (2019) were the first to address this question with an interval estimation task. Participants' keypresses produced either stimuli which were previously associated with the self or a friend or a stranger: Subsequently, they were asked to estimate the duration of the interval between the keypress and the appearance of one of the three stimuli. Results showed that temporal binding was stronger, i.e., the interval perceived as shorter, when participants produced stimuli associated with the self as compared to stimuli associated with a friend or a stranger. Chiarella et al. (2020) extended these findings and suggest that promoting self-other connections e.g., through meditation, eliminates advantages of self-referential processing in postdictive temporal binding (binding caused by observation of a just encountered action-effect episode) but not the early process of self-prioritization.

One crucial shortcoming of the employed method is that it is impossible to discern whether the perceived shortening of the interval stems from a perceptual shift of the action towards the action-effect or a shift of the action-effect towards the action or a mutual attraction of the two. Temporal binding is comprised of action binding (perceived later point in time of an action that produces an effect compared to an action that does not) and effect binding (perceived earlier point in time of an event that was produced by an action compared to an event that was not) and increases in one component can be associated with decreases in the other (Lush et al., 2019; Wolpe et al., 2013; Yamamoto, 2020). Hence, increased attention to the outcome might lead to increased relative precision in the temporal perception of this event but this might in turn decrease temporal precision for the action, i.e., increase action binding. Thus, with a similar study design, we intended to shed light on the origins of temporal binding for self-relevant compared to non-relevant outcomes by using a different measure, i.e., the Libet clock.

7.2. Experiment 1: Self-relevant shapes

7.2.1. Method

All experiments presented here were preregistered on the Open Science Framework (OSF) and raw data as well as additional materials are available (https://osf.io/pq43j/). Additionally, the study was approved by the ethics committee of the psychology department of the Julius-Maximilians-University of Würzburg (GZEK 2021-63).

Participants. 24 participants were recruited over the university's study platform SONA. After data exclusion as preregistered on OSF, data of 16 participants was used for the analysis. Based on the large effect (*d* = .95) of self-relevance on temporal binding reported by Makwana and Srinivasan (2019), a sample of 13 should have sufficed to detect the effect (α = .05; power = .86). However, previous studies using the Libet clock to measure temporal binding showed smaller effect sizes. For example, Ruess et al. (2017) reported an effect of *d* = .65 for action binding. Thus, we based our targeted sample size on an a priori sample size calculation using G*Power (Faul et al., 2009) with a more conservative effect estimate of *d* = 0.65, α = .05 and a power of .85 resulting in 24 participants. All participants were naïve to the study purpose and provided informed consent prior to the study. They received monetary compensation for their voluntary participation. Seven participants had to be excluded due to high error rates (> 25%) in the temporal binding task when asked to name the shape presented. Another participant was excluded because they did not remember their self-associated shape correctly after the temporal binding task. The final sample consisted of 16 (2 m, 14 f; 13 right-handed, 1 left-handed, 2 ambidexter) participants ranging between 22 to 64 in age (*M* = 31.9, *SD* = 12.4).

Stimuli and task procedure. The experiment was run on stationary lab computers connected to LCD monitors with a screen size of 21.5'' (resolution 1920 × 1080 px) and a refresh rate of 60 Hz. It consisted of four phases: induction phase, agency phase, matching phase, and questionnaires (see Figure 17).

Induction phase. | At the beginning of the experiment, participants were assigned one of two shapes (square or triangle), the shape was counterbalanced across participants. In a short induction phase, they were asked to identify themselves with the respective shape while the other shape was said to represent someone else. The information ("Imagine you are a triangle/square and another person is a square/triangle") was provided in German as text on the screen as well as over the computer's speakers. Additionally, during this induction phase and the subsequent agency phase the mouse cursor was shaped according to the own shape for participants to encounter control over the self-relevant shape.

Agency phase. | Following the induction phase, participants completed the agency phase during which they performed a temporal binding task. A clock face of 240 px diameter with a rotating clock hand was presented at the center of the screen and voluntary keypresses resulted in the presentation of either the self-associated or the other-associated shape in the middle of the clock. Subsequently, participants reported the position of the clock hand at either the time of the keypress or the time of the appearance of the shape. This report was given by means of the keyboard (0-60) in correspondence with the minutes on a clock face. A full rotation of the clock lasted 2500 ms and participants were asked not to press a key within the first half revolution. The starting position of the clock hand was selected randomly. In the agency phase, participants encountered four different conditions:

(1) Action operant – In this condition, participants pressed the spacebar to produce a geometric shape which was presented around the clockface. Once participants had pressed the spacebar, one randomly selected geometric shape appeared after a delay of 250 ms and was displayed for 60 ms (see also Haggard et al., 2002; Ruess et al., 2017). Afterwards the clock hand continued rotating for 2000-3000 ms. Then, participants were asked for the location of the clock hand at the time of their keypress (“Where was the clock hand when you pressed the key (0-60)?”). Sizes of the geometric shapes were determined to cover similar areas, that is, the square measured 384 × 384 px and the triangles base was 540 px wide and it was 540 px high. Both geometric shapes as well as the clockface were centered at the middle of the screen.

(2) Effect operant – This condition was equal to the action operant condition. However, this time, participants were asked for the clock hand position at the time of the action-effect (“Where was the clock hand when the shape appeared?”).

(3) Action baseline – In this condition, participants pressed the spacebar, but no geometric shape was displayed. After the keypress, the clock hand continued rotating for a random delay between 2000-3000 ms. Subsequently, participants indicated the position of the clock hand at the time of the keypress.

(4) Effect baseline – In this condition, participants were asked to refrain from key-presses. Instead, after a random delay of 1250-3750 ms one of the two shapes appeared on the screen for 60 ms. After the clock hand had continued rotating for an additional 2000-3000 ms, participants indicated the position of the clock hand when the shape appeared.

In every block except for the action baseline bock, each geometric shape was presented 27 times resulting in a total block length of 54 trials. When participants committed an error in a trial, i.e., pressed the spacebar too early or at all in action baseline condition, or gave an estimation greater than 60, the trial was discarded, reshuffled to a later position in the block, and error feedback displayed. Between blocks, participants could take short breaks. The specific instruction as well as the event (action or action-effect) to be attended were given at the beginning of each block. In addition to the four working blocks, there was a practice block consisting of ten unbroken trials of the first condition. The order in which participants encountered the conditions was counterbalanced across participants, however, both baseline and both operant blocks were always executed after one another. Thus, there were four different possible presentation orders of the conditions. To check whether participants did pay attention to the shape in all conditions, they were asked to indicate what shape they had just seen after every fourth trial. These attention checks were executed with the mouse cursor. Performance in these attention checks was used as exclusion criterion (see preregistration [https://osf.io/pq43j/registrations] for details).

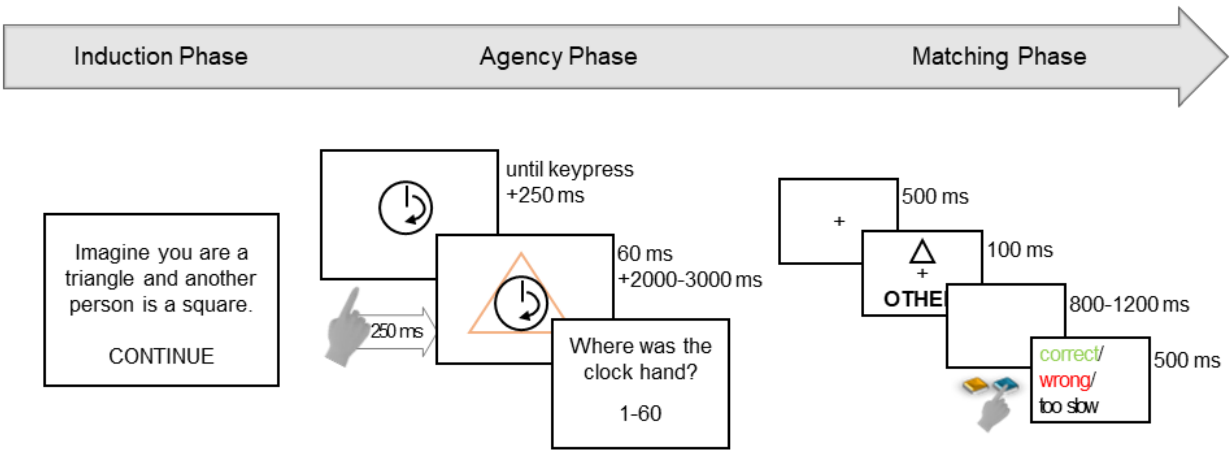
Matching phase. | In the third part of the experiment, participants completed a matching task as in Sui et al. (2012). The task was to decide whether a presented shape and label were matching according to the association learned in the induction phase. Participants responded with the *S* and *L* key to indicate matches or no-matches. Mapping of the keys was counterbalanced across participants and match/ no-match responses. Trials started with the presentation of a fixation cross in the middle of the screen for 500 ms. Subsequently, one of the two shapes (square, triangle) that were 90 px wide and 90 px high and either of the labels (self = “O O O I C H O O O”; other = “O O A N D E R E O O”) were presented above and below the fixation cross for 100 ms. Afterwards the screen turned white again until participants pressed a key or 1200 ms had passed. Finally, participants received feedback (correct, wrong, too slow) for 500 ms.

Participants completed 110 trials in the matching task (25 matching and 25 non-matching trials per shape association; see also Frings & Wentura, 2014) of which the first 10 were considered practice trials and thus not included in the analysis.

Questionnaires. | Finally, participants filled out German versions of the NPI-13 (Brailovskaia et al., 2019) as well as the PHQ-9 (Gräfe et al., 2004). We will not discuss the results of the questionnaires in this research as they were collected and used for a master's thesis.

Figure 17

Trial procedure in Experiment 1



Note. Participants started with a short induction phase where they were assigned a geometric shape while another shape was assigned to another person. Subsequently, participants performed the temporal binding task in which they pressed the space bar to randomly produce one of the two shapes. At the end of each trial, they were asked to report the position of the clock hand at either their keypress or the appearance of the shape. Finally, there was a matching task in which participants were presented a shape label pairing and were to decide whether it was a match or not.

Data analysis. To analyze temporal binding, we first calculated participants' estimation errors on each trial as the difference between the participants estimation and the actual timing of the event to be judged. Subsequently, two 2 × 2 analyses of variance (ANOVA) with condition (baseline vs. operant) and relevance (self vs. other) as within-subjects factors and the estimation error as dependent variable were conducted for actions and action-effects separately. Follow-up analyses were done via two-tailed, paired *t*-tests. The difference between estimation errors in the operant conditions and the respective baseline conditions is referred to as action binding and effect binding respectively. Positive values indicate that events were judged to have happened later in the operant compared to the baseline condition while negative values represent a shift to an earlier point in time. Finally, to illustrate the evidence, Bayesian analyses were performed in JASP (Dienes, 2014; JASP Team, 2018, Version 0.14). Based on the effects of action binding and effect binding reported for predictable delays of 250 ms by Ruess et al. (2017) and a lack of evidence that the effect size would be small relative to the maximal plausible effect size (Dienes, 2019), we modeled H1 for action binding as normal distribution with a scale factor of 14.07 ms ($BF_{\mathrm{N(0,14.07\ ms)}}$). For effect binding, we modeled H1 as normal distribution with a standard deviation of 53.68 ms ($BF_{\mathrm{N(0,53.68\ ms)}}$). As there is insufficient information on the size of the effect of self-relevance on temporal binding, the default Cauchy prior of 0.707 was used for those Bayesian t-tests. This means that we were 50% confident that the true effect size lies between −.707 and .707 which is common in social sciences (Bartlett, 2017).

As the self-prioritization effect is typically reported as the difference in RTs or error rates between self-match and other-match trials (e.g., Schäfer et al., 2021), we calculated mean reaction times and error rates for each trial type individually. Subsequently, we conducted two-tailed, paired t-tests. Additionally, we report d' as a measure of task performance. Effect sizes for all paired *t*-tests were calculated as $d_z = \frac{t}{\sqrt{n}}$. Here again, we used Bayesian analyses to illustrate the evidence for any effects using the default Cauchy prior of 0.707 in JASP (JASP Team, 2018). Additionally, in Table 3, we report d' as a measure of general task performance. Analyses including no-match trials can be found in Table 6 in Appendix III.

7.2.2. Results

When reporting the results, we start with the matching task, i.e., the self-prioritization effect as this served as manipulation check to control whether participants assigned different weights to the self-related compared to the other-related stimulus. Subsequently, we present the data of the temporal binding task (see Figure 18D).

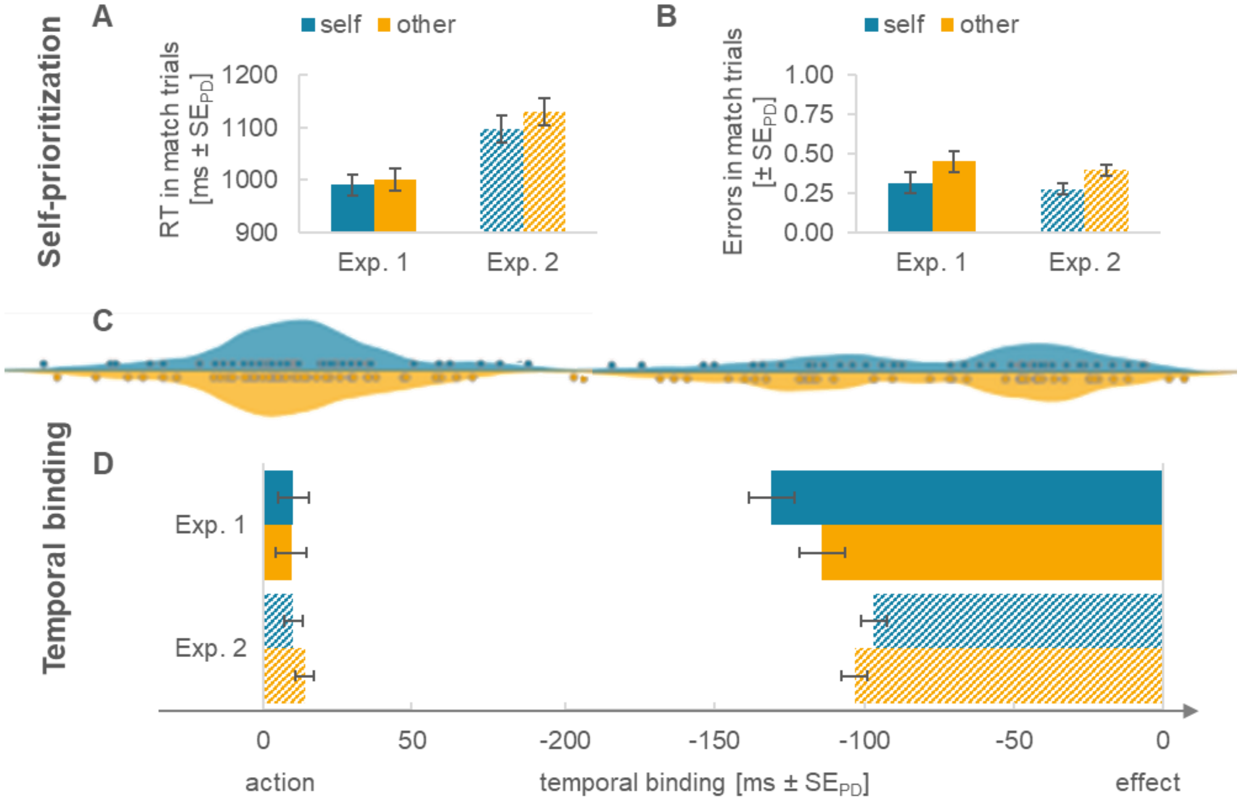
Self-prioritization. Participants only showed a trend towards self-prioritization in the error rates but not in reaction times. Thus, the results must be interpreted with caution. Error rates were descriptively but not statistically lower in self-match trials as compared to other-match trials, $t(15) = 2.09$, $p = .054$, $d_z = 0.52$, $BF_{10} = 1.422$, $\Delta = -13.5\%$. There was moderate evidence that reactions times did not differ significantly between self-related and other-related trials, $t(15) < 1$, $BF_{10} = 0.290$, $\Delta = -11.0$ ms (see Figure 18A & B).

Temporal binding. The 2×2 ANOVA for action binding with condition (baseline vs. operant) and relevance (self vs. other) did not reveal any significant main effect or interaction, main effect condition $F(1,15) = 1.60$, $p = .225$, $\eta_p^2 = .10$, all other Fs < 1. As such, there was no evidence for either action as perceived to have happened later when they were followed by a shape as compared to happening in isolation.

Contrary, the 2×2 ANOVA for effect binding with condition (baseline vs. operant) and relevance (self vs. other) revealed a main effect of condition, $F(1,15) = 23.16$, $p < .001$, $\eta_p^2 = .61$, $BF_{10} = 216.69$, as well as a condition × relevance interaction, $F(1,15) = 4.98$, $p < .041$, $\eta_p^2 = .25$. Both outcomes were perceived to have happened earlier when they were preceded by a keypress as compared to happening in isolation. Additionally, self-related outcomes resulted in stronger effect binding compared to other-related outcomes, $t(15) = 2.23$, $p = .041$, $d_z = 0.56$, $\Delta = 16.9$ ms. The main effect of relevance was not significant, $F(1,15) < 1$. Bayes factors for all comparisons between self-related and other-related action binding and effect binding are listed in Table 4.

Figure 18

Self-prioritization effect and temporal binding in Experiments 1 & 2 using visual action-effects



Note. Stimulus association is color-coded: blue is self-related, orange is other-related. (A) Mean reaction times in self-related and other-related matching trials separately for Experiment 1 and Experiment 2. Error bars in all panels depict standard errors for paired differences. (B) Mean error rate in self-related and other-related matching trials separately for Experiment 1 and Experiment 2. (C) Distribution of action binding and effect binding combined for Experiments 1 and 2. Individual points represent participants. (D) Temporal binding in ms separately for Experiment 1 and Experiment 2. Bars from left to right depict the action shift in the operant relative to the baseline condition. Bars from right to left show the outcome shift in the operant relative to the baseline condition.

7.2.3. Discussion

Experiment 1 was designed to (a) replicate the results of Makwana and Srinivasan (2019) being that self-related action-effects elicit stronger temporal binding than other-related action-effects. Additionally, we intended to locate this effect in either action binding or effect binding or both. Participants performed a temporal binding task in which they either elicited a stimulus which had previously been associated with the self or with another person. Additionally, they completed a matching task as manipulation check for the self-relevance manipulation.

Contrary to our expectations, the perceived time points of actions were not shifted to a later point in the operant as compared to the baseline conditions. However, the perceived timing of the outcomes was shifted towards the action in the operant condition compared to the baseline condition. Additionally, the difference in effect binding between self-related and other-related action-effects seemed in line with the observation that self-relevance increases temporal binding. However, due to outlier exclusions, our sample size was decreased drastically. A sensitivity analysis revealed that the effect size would have had to be at least 0.80 to be detected with a power of .85 in our data indicating that the study might have been underpowered. Moreover, the effect seemed to be smaller than initially expected. Additionally, while we found a marginally significant self-prioritization effect in the error rates, reaction times did not differ depending on self-relevance. Additionally, error rates in general were quite high which is why the results in the matching task should be interpreted with caution. This indicates that the matching task was rather difficult, and it cannot be concluded that the manipulation check was successful.

Against the backdrop of these observations, we conducted a second experiment with a larger sample size and a slightly easier matching task to replicate and scrutinize the effect of self-relevance on temporal binding and increase the self-prioritization effect.

7.3. Experiment 2: Replication – self-relevant shapes

As mentioned above, due to outliers and participant exclusion our sample size in the first experiment shrunk to 16 participants. Thus, we conducted a second experiment aiming at replicating the initial findings in an online study with a larger and more considerate sample size.

Few changes to the initial setup were made (a) to cater for the online setup of the replication study and (b) to strengthen the association between the self and the shape to maximize any effects related to self-relevance. We expected to replicate effect of self-relevance on temporal binding, i.e., to find larger effect binding for self- associated shapes compared to other-associated shapes. Self-relevance was not expected to influence action binding. The study including all changes made to the initial setup as well as participant exclusion criteria was preregistered at the projects OSF page (https://osf.io/pq43j/).

7.3.1. Method

Participants. We tested 32 healthy participants recruited over the study platform Prolific (www.prolific.co). They conducted the experiment on their own computer using E-Prime Go (Psychology Software Tools, Inc., 2020). Participants received monetary compensation for their voluntary participation. Based on the medium effect (d = .56) of self-relevance on effect binding observed in our first experiment, we conducted an a priori sample size calculation for two-tailed paired t-tests using G*Power (Faul et al., 2009) with d = 0.55, α = .05 and a power of .85, resulting in a minimal sample size of 32 participants. We had to exclude and replace two participants as they failed to complete more than 33% of the attention checks. The percentage of failed attention checks in the final data set ranged from 0%-26% (M = 7.5, SD = 6.1). Data of one additional participant had to be replaced because some of the data was lost during data collection[7]. Participants in the final sample set (17 male, 14 female, 1 other; 4 left-handed, 28 right-handed) were between 18-47 years (M = 27.6, SD = 8.4).

Stimuli and task procedure. The experiment consisted of the same four phases as Experiment 1: induction phase, matching phase, agency phase, questionnaires. However, this time we switched the order of the matching phase and the agency phase to strengthen the self-shape association as the self-prioritization effect usually becomes stronger throughout the matching task.

[7] Data of all participants including replaced subjects is available at the project's OSF repository.

We changed the display time in the agency task from 60 to 100 ms and the size of the geometric shape to fit within the clockface. That is, the square's edges measured 120 px and the triangle was both 120 px wide and high. In addition, the response window for the self-prioritization task was increased to 1500 ms to decrease the number of misses and errors.

7.3.2. Results

Self-prioritization. Participants did show a self-prioritization effect only in the error rates (see Figure 18B). Error rates for self-match trials were significantly lower than for other-match trials, $t(31) = 3.33$, $p = .002$, $d_z = 0.59$, $BF_{10} = 16.06$. Contrary, reactions times did not differ significantly between self-related and other-related trials, $t(31) = 1.23$, $p = .228$, $d_z = 0.22$, $\Delta = -32.2$ ms, $BF_{10} = 0.38$. Again, *d'* did not differ significantly between self-related and other-related stimuli, $t(31) = -1.60$, $p = .120$, $d_z = -0.28$.

Temporal binding. The 2×2 ANOVA for action binding with condition (baseline vs. operant) and relevance (self vs. other) showed that participants tended to report their action to have happened later when it was followed by an outcome than when the event did not occur. However, there is no clear evidence for or against this shift, $F(1,31) = 3.87$, $p = .058$, $\eta_p^2 = .11$, $BF_{10} = 2.00$. Neither the main effect of relevance nor the interaction were significant, $F(1,31) = 1.66$, $p = .207$, $\eta_p^2 = .05$.

Both outcomes were perceived to have happened earlier when they were preceded by a keypress as compared to happening in isolation, $F(1,31) = 39.69$, $p < .001$, $\eta_p^2 = .56$, $BF_{10} = 5.02e+4$. The 2×2 ANOVA for effect binding did not reveal a main effect of relevance, $F < 1$, or an interaction, $F(1,31) = 2.13$, $p = .155$, $\eta_p^2 = .06$. That is, there was no significant difference in effect binding between self-related and other-related action-effects.

Table 3

Reaction times, absolute error rates, and the sensitivity measure d' across all experiments

Exp.	Stimulus	RT		Error rate		*d'*
		Matching	Non-matching	Matching	Non-matching	
1	Self	990.2 (173.8)	1031.9 (216.8)	31.5 (19.9)	47.7 (19.2)	0.35 (0.89)
	Other	1001.2 (225.1)	1022.0 (183.3)	45.0 (16.2)	44.8 (18.1)	0.28 (0.69)
2	Self	1097.4 (250.5)	1163.4 (310.2)	27.6 (21.9)	32.4 (15.1)	0.36 (1.28)
	Other	1129.6 (295.4)	1132.9 (256.3)	39.5 (18)	33.0 (17.9)	0.77 (0.84)
3	Self	1052.7 (94.0)	1136.3 (110.5)	9.0 (19.7)	16.6 (23.2)	1.12 (1.54)
	Other	1133.1 (135.4)	1149.6 (122.3)	17.6 (27.8)	18.2 (25.5)	1.68 (1.24)
4	Self	1121.1 (90.7)	1192.1 (94.5)	9.5 (9.3)	12.6 (11.1)	1.56 (1.44)
	Other	1170.1 (102.6)	1512.4 (105.6)	15.8 (15.3)	19.6 (15.8)	1.42 (1.25)

Note. RTs in milliseconds and absolute error rates in % as well as the sensitivity measure d' as a function of relevance and matching condition (matching vs. non-matching). Standard deviations are in parentheses.

7.3.3. Discussion

With Experiment 2, we intended to scrutinize the influence of effect self-relevance of temporal binding. Even though we succeeded in increasing the self-prioritization effect and thereby association strength between the self and the self-relevant stimulus, we could not replicate the effect of outcome relevance on temporal binding. Neither action binding nor effect binding differed significantly between self-related and other-related action-effects[8].

To draw a preliminary conclusion, a critical reassessment of the changes made to the initial study design is required. First, increasing the response window for the matching task and changing the task order did indeed result in a stronger self-prioritization effect in Experiment 2 compared to Experiment 1. This effect might be strengthened even more when the psychological distance between the self and the other person becomes more pronounced such as the distinction between *self* and *stranger*, which is probably stronger than between *self* and *some other person* (see Golubickis & Macrae, 2021a; Sui et al., 2012). Additionally, we had to exclude fewer

[8] We conducted an additional experiment as replication of Experiment 1 with the same task order to control for possible task sequence effects. Here, we did not find any modulation of temporal binding, either. Data and analysis are available at the project's OSF page (https://osf.io/pq43j/) as well as in Appendix II.

trials due to too slow reactions. Second, the reduction of the stimulus size to fit within the clock face might have reduced the visual demand in the temporal binding task, however, it did not result in the clear binding pattern we expected. Thus, as the study by Makwana and Srinivasan (2019) used interval estimations and therefore did not have any visual distraction, in our case the clock face, on screen, this additional visual input might have skewed the results. Therefore, we conducted a third experiment with auditory instead of visual action-effects. Thereby, we did not only reduce visual demand but also chose outcomes which have shown to produce stronger and more reliable temporal binding (see Ruess, Thomaschke, & Kiesel, 2018 for a discussion).

7.4. Experiment 3: Self-relevant sounds

Following up on the non-significant effects of self-relevance on temporal binding observed in the first two experiments, we conducted another experiment minimizing visual demand in the temporal binding task by using auditory instead of visual action-effects. We were quite confident to be able to replicate the self-prioritization effect as Schäfer et al. (2016) have shown that this effect also translates to the auditory and the tactile domain. The study including all changes made to the other experiments as well as participant exclusion criteria was preregistered at the projects OSF page (https://osf.io/pq43j/).

7.4.1. Method

Participants. We tested another set of 32 healthy participants between 20 to 58 years (M = 27.4, *SD* = 7.8) recruited over the university's participants pool SONA for in-house data collection. 7 participants identified as male, 25 as female (3 left-handed, 29 right-handed). Participants were naïve regarding the purpose of the study and received monetary compensation for their voluntary participation.

We had to exclude and replace seven participants as they did not correctly remember their sound after completing the matching task. Two additional participants had to be replaced due to a programming error[9]. In general, participants performed excellently in the attention

[9] Again, data of these participants is available at the project's OSF repository.

checks, no participant had to be excluded because they did not identify the encountered stimuli correctly. Missed attention checks ranged between 0%-15% (M = 3.5, SD = 4.1).

Stimuli and task procedure. Experiment 3 followed the first two experiments in their setup, however, for time efficiency, in this third experiment we relinquished the questionnaires as the last phase. The order of the three remaining phases was as in the second experiment: induction phase, matching phase, and agency phase.

In contrast to the first two experiments, we used auditory stimuli instead of visual stimuli in Experiment 3 and 4. The two sounds were 300 ms long snippets of a flute and a snare drum. The sounds were neutral in valence (for information on a pilot study see Schäfer et al., 2016) and administered over headphones.

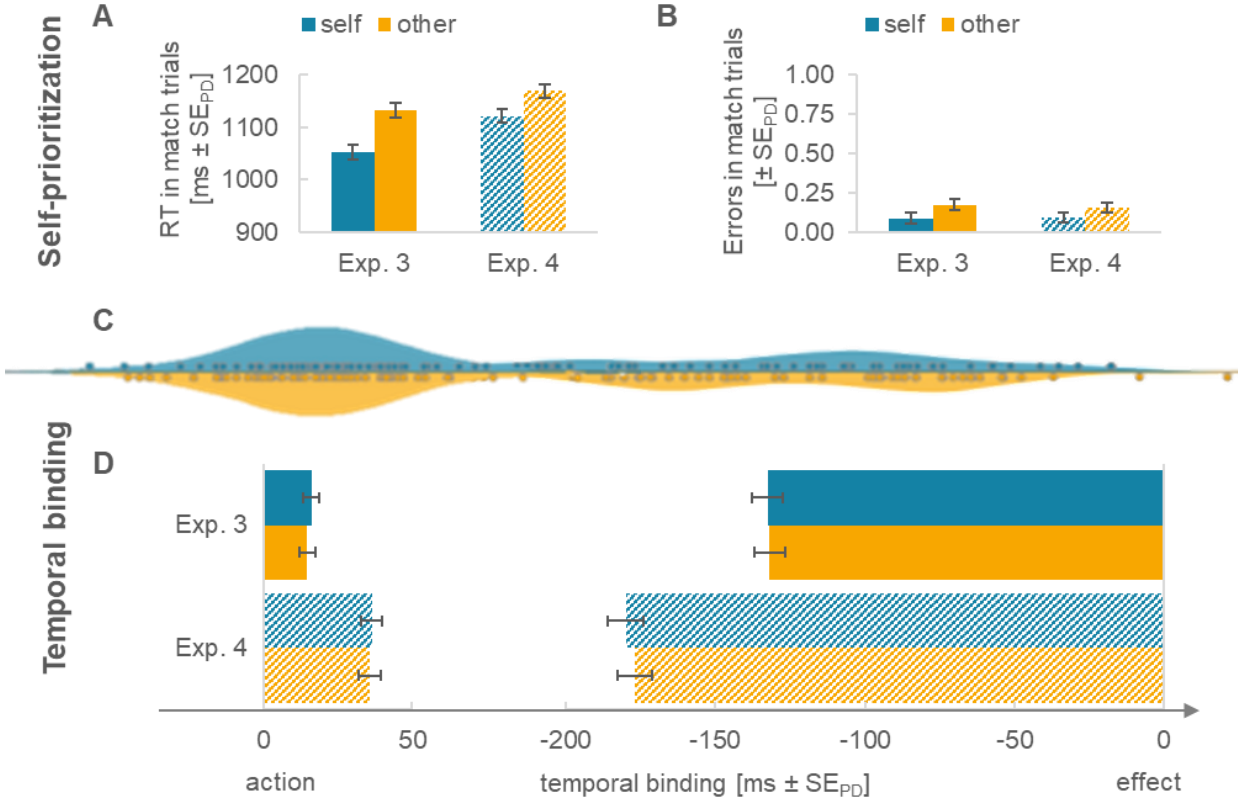
7.4.2. Results

Self-prioritization. Participants were faster in correctly identifying self-match compared to other-match trials, $t(31) = 5.61$, $p < .001$, $d_z = 0.99$, $\Delta = -80.4$ ms, $BF_{10} = 5143.26$. Additionally, they also committed fewer errors in self-match trials compared to other-match trials, $t(31) = 2.53$, $p = .017$, $d_z = 0.45$, $BF_{10} = 2.85$. The difference in d' in favor of other-related stimuli was not significant, $t(31) = -1.97$, $p = .058$, $d_z = -0.35$.

Temporal binding. As depicted in Figure 19D, actions were perceived to have happened later when they were followed by an action-effect as compared to when there was no sound, as indicated by the significant main effect of condition, $F(1,31) = 7.29$, $p = .011$, $\eta_p^2 = .19$, $BF_{10} = 6.12$. Neither the main effect of relevance nor the interaction reached significance, $Fs < 1$. Similarly, both outcomes were perceived to have happened earlier when they were preceded by a keypress as compared to happening in isolation, $F(1,31) = 139.40$, $p < .001$, $\eta_p^2 = .82$, BF_{10} = 2.55e+10. No other comparison reached significance, $Fs < 1$. Thus, there was no significant difference in effect binding between self-related and other-related action-effects.

Figure 19

Self-prioritization effect and temporal binding in Experiments 3 & 4 using auditory action-effects



Note. Stimulus association is color-coded: blue is self-related, orange is other-related. (A) Mean reaction times in self-related and other-related matching trials separately for Experiment 3 and Experiment 4. Error bars in all panels depict standard errors for paired differences. (B) Mean error rate in self-related and other-related matching trials separately for Experiment 3 and Experiment 4. (C) Distribution of action binding and effect binding combined for Experiments 3 and 4. Individual points represent participants. (D) Temporal binding in ms separately for Experiment 3 and Experiment 4. Bars from left to right depict the action shift in the operant condition relative to the baseline condition. Bars from right to left show the outcome shift in the operant condition relative to the baseline condition.

7.4.1. Discussion

In this third experiment, we used auditory stimuli to analyze any impact of outcome-relevance on temporal binding. Auditory action-effects were used as they were previously found to produce stronger temporal binding compared to visual action-effects (Ruess, Thomaschke, & Kiesel, 2018). In line with our expectations, we did find robust action binding as well as effect binding. However, they did not differ between self-related and other-related outcomes. Again, the manipulation check was successful, and participants exhibited a self-prioritization effect in both reaction times as well as error rates.

7.5. Experiment 4: Predictable self-relevance

Finally, to increase the salience of the self-relevance and reduce uncertainty, we conducted a fourth experiment with predictable action-effects. That is, participants' freely chosen keypresses contingently evoked either the self-related or the other-related outcome. We conducted this experiment to test whether controlling the identity of the upcoming action-effect has an additional influence on the null results we obtained in the first experiments. However, research on temporal binding and outcome predictability suggests that temporal binding does not differ depending on whether participants have actual control over the outcome identity or whether it is random (Desantis et al., 2012; Haering & Kiesel, 2014). Thus, we expected to replicate the results of Experiment 3. Any effect of outcome relevance here would be due to identity prediction of the outcome.

7.5.1. Method

Participants. 32 naïve participants of which 12 identified as male and 20 as female were recruited over the study platform Prolific (www.prolific.co) and received monetary compensation for their voluntary participation. Participants (3 left-handed, 28 right-handed, 1 ambidextrous) were between 19 and 38 years old (*M* = 28.0, *SD* = 5.6). The experiment was conducted on their own computers using E-Prime Go (Psychology Software Tools, Inc., 2020). We conducted Chi-square goodness-of-fit tests to test for equal distribution of keypresses and avoid unintended effects of action-effect frequency in the temporal binding task. Four participants were excluded and their data replaced as they did not meet this criterion. Five subjects were replaced as they did not remember their self-associated sound correctly after the matching task. No additional participant had to be excluded due to failed attention checks which ranging between 0%-8% (M = 1.0, SD = 1.7).

Stimuli and task procedure. We used the same stimuli as in Experiment 3 and the procedure followed that of the other experiments. One crucial change in the temporal binding task, however, was that this time, participants could choose between two keys (F and J) that were associated with either of the two outcomes. Both the self-relevant stimulus as well as the mapping of the keys was counterbalanced across participants. They were asked to press each key in about 50% of the trials without following specific patterns. After the first half of each block, participants were informed about the ratio of keypresses so they could adjust in the second half of the block.

7.5.2. Results

Self-prioritization. We found a self-prioritization effect both in the reaction times as well as in the error rates (see Figure 19A & B). Correct classifications for self-match trials were faster than for other-match trials, $t(31) = 3.82$, $p < .001$, $d_z = 0.68$, $\Delta = -49.1$ ms, $BF_{10} = 51.04$. Similarly, error rates in self-match trials were significantly lower than in other-match trials, $t(31) = 2.15$, $p = .039$, $d_z = 0.38$, $BF_{10} = 1.42$. The sensitivity measure d' did not differ between the two types of stimuli, $t(31) < 1$.

Temporal binding. Participants judged their actions to have happened later when they were followed by a tone in comparison to when no tone followed, $F(1,31) = 15.01$, $p < .001$, $\eta_p^2 = .33$, $BF_{10} = 50.70$. The same held true for the outcomes, the perceived timepoints of outcomes was shifted to an earlier time when outcomes were preceded by an action, $F(1,31) = 91.96$, $p < .001$, $\eta_p^2 = .75$, BF_{10} = 2.14e+8. There was no significant difference in temporal binding between self-related and other-related action-effects as indicated by the non-significant interaction terms for action binding and effect binding, $Fs < 1$, as well as the Bayes factors that we calculated for the comparisons between self-related outcomes and other-related outcomes for action binding, BF_{01} = 5.21, and effect binding, BF_{01} = 4.73 that both provide strong evidence for no difference.

In addition to the two 2×2 ANOVAs for action and effect binding, we conducted paired sample *t*-tests for the estimation errors when the perceived time point of the stimulus had to be judged. Estimation errors did not differ significantly between self-related and other-related outcomes in the baseline condition, $t(31) = 1.33$, $p = .193$, $d_z = 0.24$, $\Delta = 4.8$ ms, or the operant condition, $t(31) = 1.67$, $p = .106$, $d_z = 0.29$, $\Delta = 7.7$ ms. This indicates that neither of the sounds had a processing advantage, i.e., was perceived earlier than the other.

Pooled analysis. Finally, we conducted pooled analysis including data from all participants reported in the experiments above. Overall, participants were faster at correctly identifying self-match trials compared to other-match trials in the matching task, $t(111) = 4.84$, $p < .001$, $d_z = 0.46$, $\Delta = -47.8$ ms. Additionally, on average, they also committed fewer errors in self-match trials compared to other-match trials, $t(111) = 5.13$, $p < .001$, $d_z = 0.49$, $\Delta = -9.6\%$.

The combined evidence for no difference in action binding between self-related and other-related outcomes was strong, BF_{01} = 9.23 (see Figure 18C & Figure 19C). The same held true for effect binding, BF_{01} = 8.15. This was irrespective of the action-effect modality (visual vs. auditory) and its salience (large vs. small shape). Interestingly, temporal binding appeared to be stronger when participants controlled the action-effect's identity with their keypresses and therefore were able to predict it (see Figure 19D). However, post-hoc Dunnett's test indicated that action and effect binding in the last experiment were only larger than in Experiment 2 (small visual effects) but not in the other two.

Table 4

Temporal binding across all four experiments

		self-related	other-related	*t*	BF_{01}
Exp. 1	Action Binding	10.2 ms	9.5 ms	0.15	5.24
	Effect Binding	- 131.0 ms	- 114.1 ms	2.23	0.67
Exp. 2	Action Binding	10.2 ms	14.1 ms	1.29	3.32
	Effect Binding	- 96.8 ms	- 103.2 ms	1.46	2.67
Exp. 3	Action Binding	16.1 ms	14.8 ms	0.45	6.60
	Effect Binding	- 132.5 ms	- 131.8 ms	0.14	7.23
Exp. 4	Action Binding	36.4 ms	35.8 ms	0.19	7.18
	Effect Binding	- 179.8 ms	- 176.9 ms	0.49	6.49

Note. Action binding and effect binding as mean difference between baseline and operant conditions. Bayes factors were calculated for the difference between self-related and other-related action-effects.

7.5.3. Discussion

With the fourth experiment we aimed at examining whether the predictability of the outcome's identity has an additional influence on self-relevance and temporal binding. As predicted, we again did not find an influence of self-relevance on temporal binding. Thus, we conclude that being able to control and predict the outcome's identity does not moderate the influence of self-relevance on temporal binding.

7.6. General discussion

The present line of research contributes to temporal binding research as well as research on self-prioritization while at the same time bringing the two together. While theorizing as well as preliminary evidence suggest that self-related outcomes produce stronger temporal binding than other-related outcomes, we did not find any influence of self-relevance on temporal binding. In all four experiments, our manipulation checks, i.e., replicating the self-prioritization effect, were reasonably successful. Note, however, that the matching phase was fairly short in comparison to typical self-prioritization studies. We manipulated action-effect modality as well as its salience and its predictability but none of these manipulations proved to have an influence on temporal binding. Nonetheless, we did find significant effect binding in all four individual experiments and action binding in all but the first experiment. We propose two possible mechanisms how self-relevance and temporal binding influence each other. First, self-relevance might only influence temporal binding via immediate response selection. Second, simply being the cause for external events might be sufficient for these events to gain self-relevance.

Research on temporal binding for visual action-effects using the Libet clock is scarce as both time reference and effect are presented in the same modality which might result in reduced salience of action-effects (Moretto et al., 2011; Ruess, Thomaschke, & Kiesel, 2018; but see e.g., Nolden et al., 2012 for visual action-effects and interval estimations). Additionally, subjective time perception of visual outcomes could be subject to resolution constrains as the speeds of the pacemakers differ between the visual and auditory domain (Wearden et al., 1998). Our results add to this body of literature by showing that temporal binding for visual action-effects can indeed be measured with the clock method (Experiment 1 and Experiment 2). Additionally, the (null-)effects observed for visual stimuli were no different to those we observed with auditory outcomes.

The results presented here seem to contradict those of the original study by Makwana and Srinivasan (2019) and its replication (Chiarella et al., 2020). Reasons for this are manifold and there are a few non-trivial differences in the study design that might account for the diverging results. First, we measured temporal binding with the Libet clock to (a) be able to examine perceived action shifts and action-effect shifts separately and (b) minimize demand effects that might occur when participants retrospectively judge the interval between their action and a specific outcome. Demand characteristics seem to bias interval estimations more easily than the assessment of time perception via the Libet clock. Comparing time estimations between different conditions makes the method more opaque and thus harder to influence. This notion, that interval estimations might be influenced by other processes than time judgements made with the Libet clock, is strengthened by a recent study showing a divergence in these two measures (Siebertz & Jansen, 2022).

Second, the two previous studies emphasized the self-other reference also during the temporal binding task by asking participants each trial whether the shape they had just produced was associated with the self, a friend, or a stranger. We reduced such influences by simply asking for the identity of the shape (or tone) after every fourth trial and thereby ensured that participants did pay attention to the identity of the action-effect. However, this should not have reduced the strength of the self-relevance manipulation as it facilitates performance as long as a self-relevant dimension, in this case identity of the shape, is part of the task set (Falbén et al., 2019).

Third, in the present study, participants performed the temporal binding task as well as the matching task in one session, whereas Makwana and Srinivasan (2019) invited participants to the lab twice – once for a longer matching session and once to complete a short matching block followed by the interval estimation task. While this elongated period might have strengthened the association between the self and the arbitrary stimulus, data of our matching task clearly showed, that participants were able to pick up a strong association in the time provided. Consequently, we conjecture that the varying levels of induction of self-relevance are a less likely explanation of the diverging result patterns rather than other possible causes such as weaker demand effects in the current temporal binding measure than in the interval estimation procedure used in previous studies.

As we could not replicate previous findings, the question must be raised whether there is an effect of self-relevance on temporal binding at all. As of yet, there is no clear answer to this question, but we propose two arguments to explain the lack of influence of self-relevance on temporal binding in the present study. This opens new perspectives for future research in the field.

First, stimulus processing and response selection possibly moderate the influence of self-relevance on temporal binding. Initially, G. W. Humphreys and Sui (2016) argued that the self-prioritization effect stems from an early processing bias in attentional control towards self-related information. However, Schäfer et al. (2020) could show that other information such as negative valence can derail attention at an earlier stage indicating that self-related information does not trump mere perceptual input. In line with this, the lack of stronger temporal binding of self-relevant action-effects suggests that self-related stimuli are not processed faster perceptually, compared to other-related stimuli. That is, the estimation errors for both effect-occurrences (self-related and other-related) were equal, even though participants reacted faster and more accurately to self-match trials compared to other-match trials in the matching task. This suggests that the self-prioritization effect does not reflect perceptual benefits of self-related stimuli, i.e., earlier perception, but rather advantages in later/other processing stages such as response selection or response execution, in case such selection is required. Consequently, the expected modulation might occur if the identity of the outcome is required to generate an appropriate motor response to this outcome indicating prioritization in the anticipation of self-relevant action-effects (e.g., Kunde, 2001; Pfister et al., 2010). This idea is supported by Woźniak and Knoblich (2021) who suggest that the self-association has to be active in working memory to elicit a self-prioritization effect Additional work indicates that the automaticity of self-prioritization is conditional to attention on the self-relevance of the object to be classified (Caughey et al., 2021; Falbén et al., 2019). Hence, to further resolve the puzzle whether self-relevance influences temporal binding, future research could interlace the temporal binding and self-prioritization task in such a way that temporal binding is measured in combination with continuous speeded responses.

Second, causing external events might suffice for these to become self-relevant. In contrast to the matching task, where the stimulus-label combination serves as symbol to trigger an action that must be retrieved from memory, in the temporal binding task, the stimulus serves as action-effect. Here, participants retrospectively have to retrieve the timing as well as the identity (for unpredictable outcomes) of the perceived sensory input to make judgements about its occurrence (see Moore & Haggard, 2008; Reddy, 2022). Attention focusses more quickly on self-relevant stimuli (see also Golubickis & Macrae, 2021b) making them accessible earlier to the system whereby effect binding should increase. Yet, such speeded attentional focusing might only occur with immediate action planning but not with retrospective judgements of sensory events. In the same vein, Golubickis et al. (2017) found temporal influences on the self-prioritization effect such that only stimuli associated with the current self, as compared to a future or past self, facilitated reaction times and accuracy indicating that the attentional benefit of self-relevant information is timely limited. Knowing whether a sensory event in the outside world was caused by oneself or not is crucial for human learning and development throughout all stages of life (Engbert & Wohlschläger, 2007; Kunde et al., 2018; Schaaf et al., 2022). Thus, an agent's knowledge of their effectiveness in causing a certain outcome might be enough for this specific event to gain self-relevance. In consequence, stimuli which have previously been associated with someone else become self-relevant, too, just by the fact that they were caused by an own motor action. Hence, the lack of influence of the outcome's self-relevance on temporal binding. One possibility to address this would be to reduce participants' effectiveness, e.g., by introducing longer action-outcome delays, by varying action-outcome contingency, or by increasing causal uncertainty through other agents. In these cases, the outcomes self-relevance provides additional information about the agent's efficiency and might thus facilitate temporal binding.

7.7. Conclusion

We conducted four experiments to analyze influences of outcome self-relevance on temporal binding. While participants exhibited a robust self-prioritization effect in all four experiments, we only found anecdotal evidence for a modulation of temporal binding through the self-relevance of action-effects in the first experiment. All other experiments as well as the pooled analyses provided strong evidence for no effect of outcome self-relevance on temporal binding. Additionally, estimation errors did not differ between self-related and other-related stimuli. Thus, we conclude that possible attentional shifts responsible for self-prioritization might occur when response selection regarding these stimuli is required, e.g., in continuous speeded response tasks, while it does not occur when indicating the onset of an action-effect irrespective of its identity, as in temporal binding. Alternatively, merely causing any outcome in the environment might be sufficient for this event to become self-relevant irrespective of the previously formed self-association.

8. Self-relevance of action-effects in action-event sequences

8.1. Introduction

Following up on the experiments presented above, we were interested to see whether a direct contrast of self-related and other-related action-effects would have an influence on temporal binding. As the self-prioritization effect is thought to be an attentional phenomenon Constable et al. (2019) used a temporal order judgement task to contrast self-related and other-related stimuli. They found a bias to judge self-related stimuli as earlier than other-related stimuli. Thus, we conducted another experiment to contrast self-related and other-related stimuli in one trial to find out whether the self-relevance bias influences temporal judgements when two stimuli have to be processed in short succession. That is, participants pressed two keys before encountering the two respective outcomes in the temporal binding task and were asked to either judge the timing of the first or second sensory event.

In the main analyses, we tested whether self-relevance of outcomes encountered in short succession influences temporal binding, or more precisely effect binding. In line with results obtained by Muth et al. (under review), we expected to find effect binding irrespective of the self-relevance of the action-effect. Additionally, effect binding for the first effect should be stronger than for the second effect, irrespective of its self-relevance (see also Muth et al., 2022; Ruess, Thomaschke, Haering, et al., 2018). If the within-trial contrast between self-related and other-related stimuli is crucial for the self-prioritization effect to influence temporal binding, effect binding for self-related stimuli should be stronger than for other-related stimuli. Such a bias would be in line with previous findings by Constable et al. (2019). We did not have any predictions regarding an interaction between self-relevance and position. Thus, any effects regarding this interaction were treated purely exploratory.

In an additional analysis serving as manipulation check, we planned to replicate the self-prioritization effect. That is, response times should be faster and accuracy higher in self-match trials compared to other-match trials.

8.1.1. Method

Participants. We tested 40 healthy participants between 18 to 47 years (*M* = 25.8, *SD* = 6.9) recruited over Prolific and who conducted the study on their own computer using E-Prime Go (Psychology Software Tools, Inc., 2020). The majority of participants (36) reported to be right-handed (3 left-handed, 1 ambidextrous). 21 participants identified as male, 18 as female and one as other. The sample size was determined based on an a priori sample size calculation using G*Power (Faul et al., 2009) with a conservative effect size of $f = 0.18$, $\alpha = .05$ and a power of .80, r = .70. We preregistered to continue data collection in sets of 8 participants until strong evidence for or against the null hypothesis has been accumulated (for details see preregistration: https://osf.io/bpkt7). Additionally, according to the preregistered exclusion criteria, we had to replace 12 participants as they failed too many attention checks, i.e., reported to have heard the wrong sound in more than 1/3 of the attention checks. An additional 9 participants were replaced due to a wrong memory association after the matching block and data of one participant was excluded because they exhibited extraordinary effect binding for the second action-effect and was thus considered an outlier. Finally, 9 participants did not follow the instructions properly and did not press both keys equally often first[10]. This was determined by a Chi-square goodness-of-fit test testing for equal distribution of keypresses.

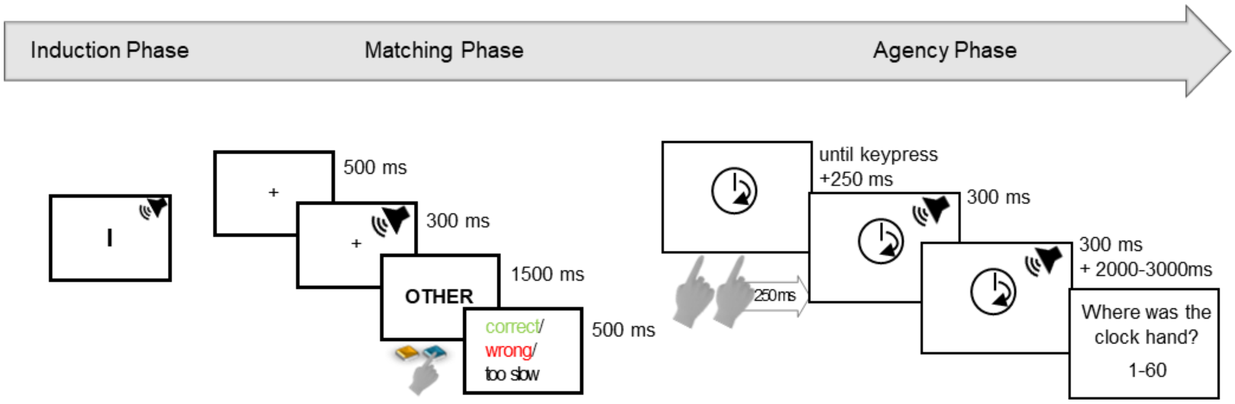
Stimuli and task procedure. The experimental setup was similar to the one used in Experiment 4 in Chapter V (7.5.) and consisted of a short association phase, a temporal binding task and a matching task (see Figure 20). If not mentioned otherwise, the stimuli and task procedure equal those in the other experiment. However, instead of one keypress that elicited a tone after a certain delay as in Experiment 4 above, this time participants performed two keypresses (F and J) associated with the two sounds (snare drum and flute) in the order of their liking. Only after they had pressed both keys, did they hear the two sounds in the order of their

[10] The number of participants that had to be replaced because they did not fulfill the preregistered criteria appears to be very high. This has multiple potential causes. The task might have been a lot more difficult than in the previous experiments. Additionally, the fact that we reduced the number of attention check from the previous to the present experiment, gave each attention check proportionally more weight, i.e., less failed attention checks led to an exclusion of the participant. Thus, I report all the analyses with more lenient exclusion criteria in Appendix III. Most importantly, this does not change the results.

keypresses. Both the self-relevant stimulus as well as the mapping of the keys was counterbalanced across participants. Participants were asked to press each key first in about 50% of the trials without following specific patterns. After the first half of each block, participants were informed about the ratio of keypresses and thus could adjust their actions in the second half of the block. Another major difference to the previous experiment was that this time, participants only completed effect operant and baseline blocks, one for each outcome position, i.e., first and second. Additionally, attention checks were collected after every 8th instead of fourth trial. Here participants were asked to indicate the identity of the sound that they were instructed to attend to, e.g., the second tone in the E2 baseline condition. In the operant conditions, the first sound was presented at a random delay of 250, 300, or 350 ms after the second keypress and the second sound followed with the same delay after the offset of the first tone. In the baseline conditions, the first sound was played after a random delay between 1250-3750 ms and the second tone followed after a delay of 250, 300, or 350 ms after the offset of the first tone.

Figure 20

Trial procedure in Experiment 5



Note. The experiment started with an induction phase in which participants learned the association between sounds (flute and snare drum) and themselves or another person. Subsequently, they completed 100 trials of a matching task to establish the self-relevance manipulation. Finally, there was an agency phase in which participants pressed two keys to elicit both tones in succession. Temporal binding for the first and second tone was measured in different blocks.

Data analysis. First, for the manipulation check, we aggregated mean reaction times for self-match and other-match trials as well as the mean accuracy scores for the respective trial types. Subsequently, two-tailed paired *t*-tests were calculated, one for RT and one for accuracy to compare the two match conditions. We did not further analyze no-match trials, as in these trials self-relevance and the assignment of label and tone is confounded.

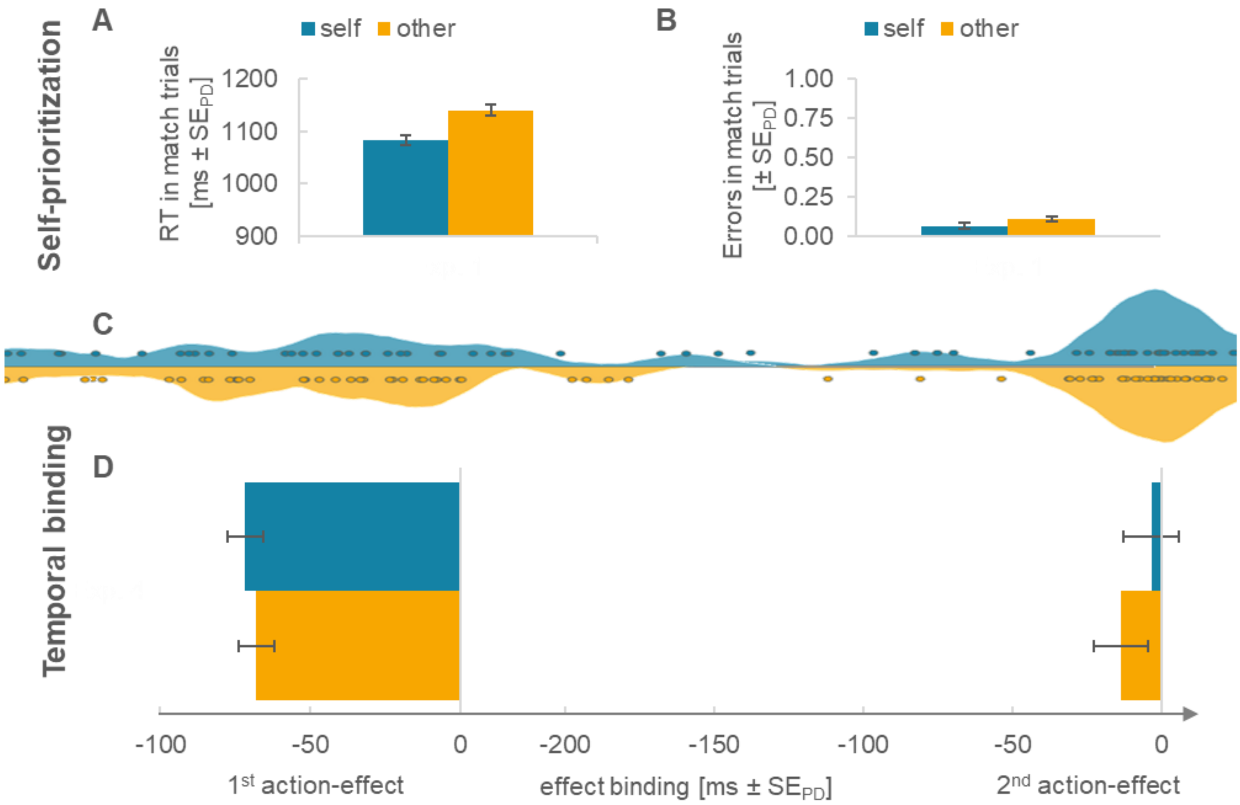
Second, we calculated estimation errors as the difference between temporal estimation and actual timing of the respective event in each condition. Then we subtracted each participant's mean estimation error in the baseline condition from that in the respective operant condition to obtain effect binding. For each effect binding score, we calculated a one sample *t*-test, to see whether events were judged to have happened earlier in the operant compared to the baseline condition. For the main analysis, we conducted a 2 × 2 ANOVA with outcome association (self vs. other) and position (1^{st} vs. 2^{nd}) as within-subjects factors and effect binding as dependent variable. Two-tailed paired *t*-tests were calculated to further analyze any differences. Effect sizes for all *t*-tests were computed as $d_z = \frac{t}{\sqrt{n}}$.

8.1.2. Results

As in the previous experiments, the manipulation check was successful (see Figure 21A & B). Participants reacted faster in self-match trials as compared to other-match trials, $t(39) = 5.59$, $p < .001$, $d_z = 0.88$, $\Delta = 57.4$ ms. Error rates were also significantly lower for self-match trials compared to other-match trials, $t(39) = 2.46$, $p = .018$, $d_z = 0.39$, $\Delta = 4.5\%$. The sensitivity measure *d'* did not differ significantly between the two types of stimuli, $t(39) = -1.27$, $p = .212$, $d_z = -0.20$.

Figure 21

Self-prioritization effect and temporal binding for two auditory action-effects



Note. Stimulus association is color-coded: blue is self-related, orange is other-related. Mean reaction times (A) and mean error rate (B) in self-related and other-related matching trials. Error bars in all panels depict standard errors for paired differences. (C) Distribution of effect binding for the first and second action-effect. Individual points represent participants. (D) Effect binding in ms. Bars show the outcome shift in the operant condition relative to the baseline condition.

Results of the one-sample *t*-tests showed that there was only effect binding for the first outcome, $t_{self}(39) = 5.27$, $p < .001$, $d_z = 0.83$, $\Delta = 72$ ms and $t_{other}(39) = 5.71$, $p < .001$, $d_z = 0.90$, $\Delta = 68$ ms. In contrast, this was not the case for the second action-effect, $t_{self}(39) = 0.48$, $p = .635$, $d_z = 0.08$, $\Delta = 4$ ms and $t_{other}(39) = 1.09$, $p = .281$, $d_z = 0.17$, $\Delta = 14$ ms (see Figure 21D). The 2×2 repeated-measures ANOVA with outcome association (self vs. other) and position (1st vs. 2nd) as within-subjects factors revealed a main effect of position indicating that the temporal binding for the first outcome was generally stronger than for the second, $F(1,39) = 14.84$, $p < .001$, $\eta_p^2 = .28$. Neither the main effect for outcome association, $F < 1$, nor the interaction reached significance, $F(1,39) = 1.73$, $p = .196$, $\eta_p^2 = .04$. Consequently, self-relevance of the outcome did not have any additional influence on temporal binding. As criterion for sample size determination, we preregistered to collect data until Byes factors provide strong enough evidence for or against either hypothesis. The Bayesian *t*-tests for paired samples provided moderate evidence against a difference between self-related and other-related outcomes both for the first outcome, $BF_{01} = 6.69$, as well as the second outcome, $BF_{01} = 4.53$.

8.1.3. Discussion

With the present experiment, we aimed to investigate whether the direct contrast of self-related versus other-related action outcomes has an effect on the perceived timing of the encountered stimuli, more precisely temporal binding. We did not find any modulation of temporal binding due to the self-relevance of the action-effect. Picking up on previous observations on temporal binding in complex action event sequences with more than one outcome, we found the perceived timepoints of the first action-effect to be shifted towards the preceding action, irrespective of its self-relevance. The perceived timing of the second outcome, however, was not systematically different between baseline and operant condition. These results conform with observations from previous studies reported in Chapter III and IV as well as research by Ruess, Thomaschke, Haering, et al. (2018) that jointly indicate that temporal binding decreases with increasing delays between action and outcome even if the delay is filled by another outcome. In this experiment, the delay was particularly long, namely 550-350 ms. The length of the delay resulted from a 300 ms presentation of the first tone followed by a random delay between 250-350 ms before the second tone was presented. Consequently, the delay might simply be too long for the second tone to be integrated into the meta-event of multiple actions and tones.

Nevertheless, the question remains why Chiarella et al. (2020) did find increased temporal binding for self-related outcomes just as Makwana and Srinivasan (2019) did, yet we failed to find any modulation in all the experiments reported here. In addition to the methodological differences between the studies discussed above, one crucial aspect might be that self-relevance only boosts stimulus processing when the previously formed association between stimulus and self is active in memory (Caughey et al., 2021). This is not necessarily the case in the present study, as participants do not have to respond to the self-relevance of the stimuli during the temporal binding task. This renders it impossible to assess whether they pay attention to the previously formed association or not. Additionally, Barton et al. (2021) raise the idea that conscious processing might be necessary for ownership to have an influence on responses in an approach and avoidance task. Even though this might not mirror the present study and its goals, it is possibly still informative with regard to the depth of processing which is required for self-relevance to influence action and perception. Ultimately, temporal binding as a low-level perceptive phenomenon does not seem to be susceptible to the self-relevance of own action-outcomes, at least not when they are irrelevant for task performance.

VI. Theoretical integration

9. Sense of agency for complex actions and their effects

Throughout this thesis, I aimed at uncovering facets of the sense of agency for actions and outcomes comprised of multiple steps. To this end, the presented experiments employed measures to assess participants' agentic experiences both implicitly[11] as well as explicitly. The combined results provide valuable insights on how the sense of agency is construed in such complex sequences and point out limitations of current research methods.

In the following, the presented work and experimental data will be integrated into the existing body of research. I start with a brief summary of the most important findings from the empirical work conducted within this thesis. Subsequently, I present a reflection of complex action-event sequences on the backdrop of two accounts mentioned in the introduction, namely multisensory integration and mere causality, and how the empirical data contributes to the debate of what sense of agency actually is. Additionally, I sketch out promising avenues for future research to better understand the subject at hand. Finally, I glance over the rim of psychology's teacup and argue why research on the sense of agency can also inform related disciplines.

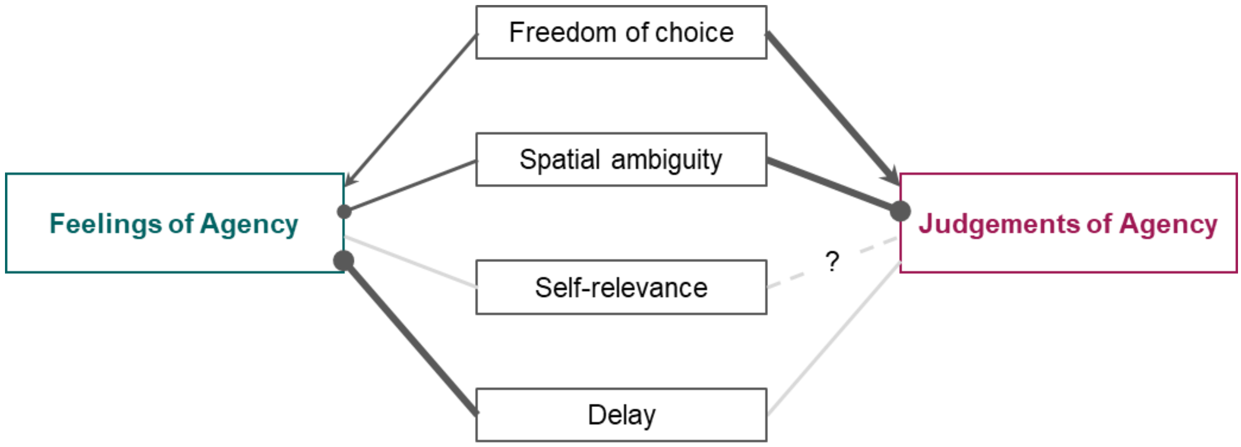
First, the auditory timer introduced at the beginning of this thesis has proven to be suitable for measuring temporal binding. It helps to *evaluate when* events, i.e., actions and outcomes, are perceived and was thus employed in the following experiments. Second, *choosing how* a goal is reached had an influence on effect binding. The perceived timing of the first outcome showed a stronger temporal attraction to the preceding event than the second one in the forced choice condition. This was not the case for freely chosen paths. Additionally, there was no action binding for multi-step action sequences. Potential causes for this observation are discussed below. Third, spatial ambiguity reduced temporal binding, especially, when it occurred early in the event-sequence. Additionally, reliably *knowing where* an outcome is going to appear has a

[11] Note that I discuss temporal binding as an implicit measure of sense of agency. However, all results and interpretations should be understood to reflect the implicit assessment of sense of agency to the extent that this is possible with the current methodology as it currently is the best proxy available.

substantial influence on agency judgements. Fourth, the outcome's self-relevance did not have any effect on temporal binding. Thus, *processing what* the identity of the outcome is, does not facilitate temporal binding.

Figure 22

Overview of influencing factors on sense of agency in complex action-event sequences



Note. Freedom of choice significantly increased explicit agency ratings, while it only had a slightly facilitating effect on feeling of agency (assessed with temporal binding). Spatial ambiguity, especially on the intermittent cursor movement deteriorated agency ratings and decreased temporal binding. Self-relevance did not influence temporal binding, agency ratings were not collected. Overall, increasing delays between action-sequence onset and outcome lead to a decrease in temporal binding. This effect was not observed in agency ratings.

Together, the results suggest that on the one hand, explicit agency judgements can include entire preceding sequences and there does not seem to be a limit to the length of these sequences. Rather, there was a differential evaluation of causal relationships in contrast to perceived control and authorship. On the other hand, temporal binding, as implicit measure, was not able to fully mirror the processes taking place in multi-step action-event sequences. The lack of action binding might have resulted from integration of the two actions into one action chunk or a biased baseline condition that already consisted of two actions or a combination of both. Additionally, in the last experiment presented, we did not find any temporal binding for the second outcome.

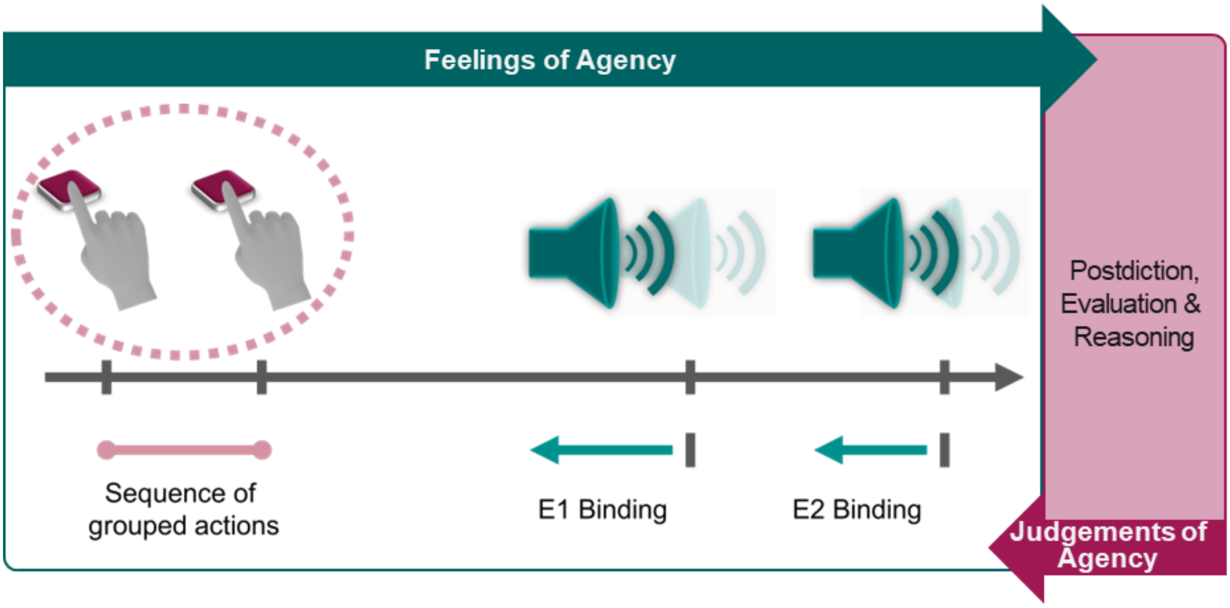
Thus, there seems to be a limitation to the scope of events that are temporally bound. In the following, the presented results will be integrated into the existing body of research (see Figure 22).

9.1. Feelings of agency – a question of multisensory integration and causal inference

When performing sequences consisting of multiple actions and sensory events, each of these instances contributes sensory information that aids the temporal resolution and grouping of all instances in the meta-event, i.e., the action-event sequence. Both the multisensory integration approach as well as the two-step model of sense of agency outlined in the introduction of this thesis agree that multiple cues can contribute to the agency experience depending on their perceptual certainty, prior knowledge about the environment, and reflective awareness. Throughout this thesis, I presented multiple studies with the intention to extend the field of research to more realistic complex action-event sequences (see Figure 22).

Figure 23

Schematic model of temporal binding in multi-step action-event sequences



Note. Actions performed in succession are grouped prior to execution of the first action. Sensory events are temporally shifted towards this meta-event. Effect binding strength is primarily determined by temporal delay and causality.

Current research on the sense of agency assumes that voluntary actions that are construed to be the cause of a subsequent sensory event are perceived as temporally shifted towards this sensory event. This observation constitutes the foundation of temporal binding as implicit measure for the sense of agency. To this date, this has almost exclusively been studied for single actions but not for multi-step actions, which are quite common in our daily life. Temporal binding in single action-event sequences has been found to be influenced by different features especially features of the sensory event, namely valence (e.g., Barlas et al., 2018; Yoshie & Haggard, 2017), identity prediction (Tanaka & Kawabata, 2021; Yoshie & Haggard, 2017; but see also Haering & Kiesel, 2014), and temporal predictability (Cravo et al., 2011; G. Hughes et al., 2013), but also action features such as choice (Barlas & Kopp, 2018), physical effort (Demanet et al., 2013), and perceptual certainty (Klaffehn et al., 2021). Surprisingly, no temporal bias for perceiving self-generated actions was found for multi-step actions. These results resonate with observations by Damen et al. (2015) who found reduced temporal binding and agency ratings for single planned actions which they attribute to increased automaticity and reduced effort of preplanned actions. In contrast to single actions, multi-step actions are performed in succession and thus very likely planned as a sequence rather than distinct actions. This action chunking has possible implications for the construal of causality links within action-event sequences such that single outcomes might not be linked to individual actions anymore but rather to the action chunk. Drawing on the literature on multisensory integration provides further explanations for the lack of action binding for multiple actions. Due to action planning and proprioceptive feedback, actions generally yield a higher perceptual certainty than external sensory events. Additionally, action binding is typically less pronounced than effect binding (e.g., Borhani et al., 2017; Muth et al., 2021; Wolpe et al., 2013). Consequently, the concatenation of two fairly certain actions might have led to a partial integration between the two, which in turn (a) makes it hard to pinpoint the timing of each individual action and (b) reduces its proneness to be biased towards ensuing external events (see Figure 23). Another possible explanation for the lack of action binding for action sequences is that we used a baseline condition with two actions to make it most similar to the operant condition. Here, a temporal attraction between the two executed actions, i.e., a perceived shift of the two actions towards each other, could have occurred. Thereby, action binding would have already been present in the baseline condition, which in turn presents itself as a null-result when comparing the perceived time points of actions between baseline and operant conditions.

In contrast to the lack of action binding, the aggregate results presented in Chapters III to V suggest that temporal binding for outcomes, i.e., effect binding, does occur in complex action-event sequences. Generally, outcomes with higher temporal proximity to the preceding actions exhibit stronger effect binding than those occurring later in the effect-sequence. That is, temporal binding decreases with increasing delay between start of the action-event sequence, i.e., initiation of the first action, and the individual outcome. Consequently, the absolute duration between an action and ensuing events is a limiting factor to temporal binding. In Chapter V, the delay until onset of the second tone seems to be so long that it is no longer integrated into the meta-event. Similar observations have been reported by Ruess, Thomaschke, Haering, et al. (2018) who propose that the delay between an action and a subsequent effect determines the magnitude of temporal binding disregarding their causal connection. It appears functional for the organism to distinguish between immediate and delayed effects to evaluate own effectiveness and establish action-effect associations (see also Engbert & Wohlschläger, 2007). During instrumental learning, one should be cautious and receptive to all kinds of effects that an own action might have produced to form action-effect associations (see also Schaaf et al., 2022; Wirth et al., 2018). An increased receptiveness for distinct outcomes of own actions also helps to explain why effect binding was decreased for spatially ambiguous cursor movements in Chapter IV when spatial ambiguity was introduced at the first step in the sequence. Here, no proper causal relations between an action and a distinct effect could be established. Humans seem to be more lenient in perceiving causal relations between an action and an outcome when there is action-outcome congruency (Bonnet et al., 2022). Moreover, from a multisensory integration view, the additional cursor might have served as an additional temporal cue to be integrated and might thus have increased perceptual certainty of this particular event. This was less so the case when the second cursor movement was spatially ambiguous as causal relations between the actions and subsequent effect had already formed and the effect binding of the second effect was weaker than that of the first. Provided action-effect associations have been established, the receptiveness to changes in the environment should narrow over time as more concrete expectations about the outcome can be formed. One observation supporting this idea is that temporal binding decreases over time. That is, estimation errors are larger at the beginning of experimental blocks than at the end of the respective blocks. This could indicate an increasing stability in the action-effect association and thereby reflect a learning process (Matute et al., 2017). Thus, I conclude that prior knowledge about causality and instrumental learning as well as the delay between the action-sequence and the outcome influence temporal binding, more precisely effect binding.

As there is compelling evidence that multisensory integration can explain at least substantial parts of temporal binding and thus contributes to the current understanding of the implicitly measured sense of agency, the question of the magnitude or the extent of meta-events becomes more pressing. How big can these multisensory integration events be and how do they scale? Is there a fixed number of sensory cues that can be taken into account, or a certain amount of time over which cues can be integrated? What determines the start and the end point of such meta-events? While research has shown that a fixed temporal window is unlikely to be the limiting factor (e.g., G. R. Humphreys & Buehner, 2009) it seems intriguing to study which events are integrated and why. First efforts in this direction have been made under the claim of the unity assumption, i.e., various simultaneously perceived unisensory cues are part of a single event (Welch & Warren, 1980). Here, research has concentrated for example on the modality of cues to be integrated and their relatedness (e.g., Doehrmann & Naumer, 2008; Stein & Meredith, 1993). Unsurprisingly, temporal proximity plays a role for two or more cues to be perceived simultaneously. Accordingly, the window of simultaneity constitutes a timeframe in which multiple unisensory cues can be ideally and fully integrated into one event (Desantis & Haggard, 2016; N. F. Dixon & Spitz, 1980; Spence & Squire, 2003). Yet, most studies in the field concentrate on the degree of integration between multiple cues and the direction of the perceptual shift, but little is known about determinants for cues to be integrated (for a review see Chen & Spence, 2017).

Following from the results presented in Chapter III and IV, I propose that whether or not multiple cues are integrated for an agency experience could be a strategic process. If causal inference is high, for example when there is no ambiguous feedback, multisensory integration is beneficial to attain a more coherent picture of the environment. In contrast, it might be detrimental to interactions with the environment when causal inference is low, i.e., one cannot be sure which outcomes were caused by the own actions, as each sensory cue should be perceived as such. That is, if there is a clear causal link between two events the grouping of these two strategically helps to maintain consistent representations of our surroundings while in case of a lack of causal inference, such a coherent representation is rather achieved by separating individual events in the environment. This is a proposition based on the data I present in this thesis and should thus be subject to further experimental studies.

9.2. Facets of agency judgements

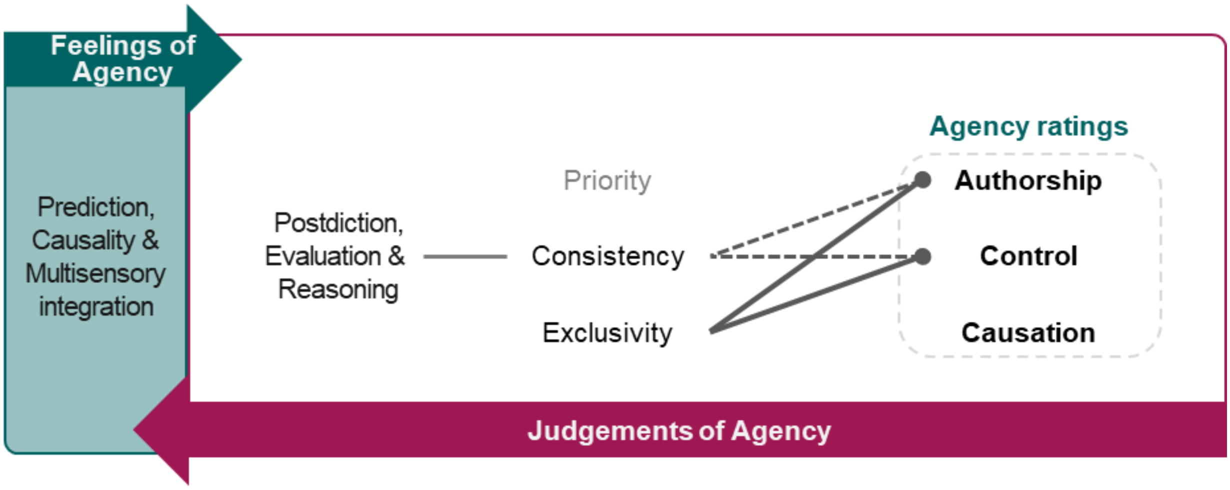
In contrast to temporal binding, here used as implicit marker of sense of agency, that largely seems to exploit multisensory integration in combination with causality, explicit judgements about own agency are more sensitive to the specifics of the question and reflect a rather broad agency experience across a whole action-event sequence. They draw on causality-links including prior knowledge and reflective awareness. As suggested by previous accounts, this is especially the case when outcomes are distorted (e.g., Haering & Kiesel, 2015; Majchrowicz & Wierzchoń, 2018). Causation ratings in contrast to authorship and control were most stable across different experimental manipulations in Chapters III and IV. Consequently, agency judgements seem to be separate and at least partly independent evaluations in the sense that even though an outcome might not be exactly as intended it can still be accurately assigned to the own actions. This is similar to the construction of causation as “making a difference” irrespective of its nature (see Chambon et al., 2018). In line with this, humans are especially receptive for changes in the environment resulting from own actions. Merely causing external sensory events is rewarding in itself and constitutes a motivator for voluntary actions (Eitam et al., 2013; Karsh & Eitam, 2015). Thus, it seems plausible that causation, i.e., one’s own bare effectiveness is evaluated differently than more elaborate evaluations about controllability of the sensory event for example.

In contrast to previous work, where participants are simply asked to rate their perceived agency over a single action followed by a distinct effect, the studies presented in this thesis broadened the scope of explicit agency judgements. As predicted, agency ratings tended to be higher when participants had more choice over how to reach a goal as well as when there was not spatial ambiguity in the visual feedback. Interestingly, ambiguity affected control and authorship ratings stronger than causation ratings. The influence of freedom of choice was most pronounced in authorship ratings.

Interpreting the results on the backdrop of Wegner and Wheatley's (1999) theory of apparent mental causation suggests that consistency and exclusivity determine authorship and control ratings but not causation ratings (see Figure 24). Admittingly, there was no condition in which participants did not have a prior intention to act. Nonetheless, influences of both consistency as well as exclusivity are reflected in the experimental data (Chapters III and IV). First, manipulating the consistency between intended outcome and actual outcome through spatial ambiguity drastically reduced authorship and control ratings. This detrimental influence was more pronounced for early than for late inconsistencies. As consistency was given in both the free choice and the forced choice condition, the difference in agency ratings can probably be attributed to increased exclusivity for freedom of choice. Second, freedom of choice increases exclusivity as the actions are determined exclusively by the agent and not by a predefined pattern to be followed. The same holds true for spatially unambiguous compared to ambiguous feedback as unintended, co-occurring events raise the question for their source. Accordingly, authorship and control ratings were lower when exclusivity was not unequivocal. Explicitly introducing other potential causes should strengthen this influence.

Figure 24

Differential influence on agency ratings



Note. Manipulations of consistency between intended and actual outcome as well as exclusivity influence authorship and control ratings but not causation ratings.

To sum up, agency experiences in complex action-event sequences appear to rely heavily on multisensory integration processes, especially with regard to the perceived timing of actions as well as the temporal delay between the start of the action-event sequence and individual sensory events. Contrary, the outcome's identity, more precisely self-relevance, does not affect temporal binding – its effect on explicit judgments stands to be tested. Additionally, causal inference seems to play a special role as causality judgements were not influenced by either of the manipulations administered throughout this thesis. Such accurate causality attributions foster for example self-other classifications.

9.3. Sense of agency is not sense of agency

Decades of research on the sense of agency have studied single action-effect episodes in diverse settings to uncover how human agency comes about and which factors influence its extent. In the following, I will reflect on the construct of sense of agency in general and the methods employed to measure it. Finally, I will sketch out why it is important to better understand sense of agency for complex action-event sequences and pave the way for the final section on the sense of agency beyond cognitive psychology.

Clearly, sense of agency is not sense of agency. Or put differently, the results presented throughout this thesis support the notion that sense of agency is more than isolated feelings and judgements of agency. This thesis is the first effort to systematically scrutinize sense of agency for multi-step action-event sequences. Previous work has shown that depending on the research discipline, the measurement, and the supposed underlying mechanism, researching identical questions regarding the sense of agency reveal different results, which is corroborated by findings form Chapter IV where implicit and explicit measures deviate. Numerous studies have previously shown that implicit and explicit measures of the sense of agency often diverge (e.g., Dewey & Knoblich, 2014; Pfister et al., 2021; Reis et al., 2022; Saito et al., 2015) but sometimes also coincide (Imaizumi & Tanno, 2019). Additionally, varying the method employed to depict sense of agency also seemingly changes the extent of agentic experience (Siebertz & Jansen, 2022). And clearly asking whether an outcome corresponds to the own intention is not the same as asking for perceived control over a manipulandum. All this holds true for simple action-effect links as well as for more complex sequences of actions and outcomes. But does this necessarily mean that different agency experiences are involved? I argue that this is not the case. Evidently, temporal binding or any other currently employed “implicit” measures of the sense of agency are not *the sense of agency*. The lack of a concise and especially unanimous theoretical framework for the sense of agency renders it almost impossible to establish reliable paradigms for measuring it. Over the years, several tasks with recurring dependent variables have repeatedly been used to scrutinize agency experiences making these the state-of-the-art paradigms regardless of their actual suitability. One reason for their popularity, despite these doubts, may be that these measurements, e.g., temporal binding produce highly replicable results (see Ivanof, 2021 for a structured manipulation of the Libet clock’s parameters).

Taking into account the constraints, discussed in the previous sections, these results can be transferred to more complex action-event sequences that bridge a gap between simple experimental studies and real-life action-event sequences. However, the employed measurements usually only zoom in on singular sensations that contribute to the overall agency experience without being able to fully reflect all aspects of it. This way, manipulating single influencing factors, like delay or spatial ambiguity, might alter feelings of agency or judgements of agency but not necessarily the other. In other words, the study of sense of agency can “always be [just] as good as its underlying operations” (Stevens, 1935a, p. 323).

Psychology is devoted to the idea of making human experience and behavior measurable, ideally in an objective manner. Thus, the challenge remains to neatly measure the experience of causing an action and through this a change in the environment not only through subjective reports but also objectively with an implicit measure. It follows that precise operationalizations are required to pin down and define psychological concepts (Stevens, 1935a). While it should have become clear by now that temporal binding is evidently not the all-encompassing implicit measure regarding sense of agency, it still appears to reflect some aspects which lay the ground for sense of agency. According to De Houwer et al. (2009) an implicit measure should meet three criteria; its outcome should be causally produced by the measured attribute (in this case sense of agency), the mechanisms by which variations in the measure are caused should be known, and it should be automatically produced. When critically examining these criteria and applying them to temporal binding, the only one that can be ticked off is the automaticity criterion, under the assumption that automatically produced is equal to an incidental generation of temporal binding. In contrast, researchers are still arguing over the mechanisms by which temporal binding is caused and the causality link between temporal binding and sense of agency is in no way uncontested. Thus, a bracket should be put around these criteria. To sum up, a better understanding of the mechanisms that constitute temporal binding could help to strengthen its case as an implicit measure or ultimately disqualify it. This again highlights the importance of precise models with testable assumptions and quantifiable components. Stevens (1935b) argues that concepts only have empirical value if they are concrete and reproducible. These two attributes are necessary for a transfer of the empirical work to applied settings. All the research presented and discussed here is reproducible even in multi-step action-event sequences. But what about the concreteness? Does the sense of agency which by definition entails aspects from planning to execution of motor commands and beyond that to changes in the environment fulfil this criterion?

Gallagher (2013) would probably argue that this is one of the most interesting peculiarities of sense of agency. Sense of agency, as he defines it, includes everything from the formation of distal D-intentions (Pacherie, 2008) over the perceptual and pre-reflective monitoring of bodily movements and consequences in the world to the retrospective reflection about agency. The ambiguity of implicit and explicit measures of sense of agency thus are part of the phenomenology itself. On the backdrop of the aggregate results in this thesis and previous work, I support a bidirectional relation between feelings of agency and judgements of agency. However, the mere interaction between feelings and judgements of agency does not necessarily mean that these two reflect the same agency experience. As such there is no all or nothing principle but rather a complex and holistic agentive experience which comes in degrees (Gallagher, 2012, 2013). Or as Synofzik and Voss (2010) quite accurately and intricately put it: “The sense of agency might […] in fact represent a complex supramodal phenomenon of largely heterogeneous functional and representational levels, with different agency cues receiving a different weighting on each level” (Synofzik & Voss, 2010, p. 148). Consequently, it might be time to stop studying *the* sense of agency but rather concentrate on concrete individual puzzle pieces or refine the concept of sense of agency in such a way that it becomes feasible to measure the *new* agency experience both implicitly and explicitly and obtain consistent results.

9.4. Beyond cognitive psychology

In the preceding section I focused on how the results for the experiments in this thesis fit to the existing body of literature and raised questions and criticism with regard to the underlying concept as well as its conception in current research. In this final part, I would like to shift the focus and make a case why it is still interesting and informative to study the sense of agency and why this makes it even more important to converge on a clear definition of the concept and agree on suitable measures. A sound understanding of the sense of agency, its underpinnings and related mechanisms cannot only be an exciting field of research for young scientists, it can also inform decision making for example in clinical populations, in the design of user interfaces, or in legislature.

9.4.1. Psychopathology

As discussed before, sense of agency is something humans are usually not explicitly aware of. However, when it comes to psychiatric and neurodegenerative disorders such as schizophrenia or anorexia nervosa its significance becomes evident.

Schizophrenic patients often misattribute actions to themselves which in fact they did not perform (Daprati et al., 1997; Fourneret et al., 2001; Knoblich et al., 2004). Additional studies with schizophrenic patients have shown that they do not only show lower discriminative accuracy when it comes to recognizing their own versus someone else's movements (Franck et al., 2001) but also exhibit differences in the typical implicit agency measures of temporal binding (Haggard et al., 2003) and sensory attenuation (Shergill et al., 2005). It is assumed that motor prediction is impaired in schizophrenic patients. Consequently, they rely more on external cues to infer authorship leading to the aforementioned hyper-associativity (Voss et al., 2010; for similar observations see Maeda et al., 2012). This seems to be consistent with reports by Delevoye-Turrell et al. (2007) who showed that while schizophrenic patients did not have difficulties producing triggered sequences, i.e., the action-effect of one action triggers the next action, their performance in producing (pre-)planned sequences was significantly impaired compared to healthy controls. This study did not analyze sense of agency per se, however, the idea that patients lack the ability to execute multiple planned motor elements in succession should result in even more erratic agency experiences for complex action sequences. Additionally, if outcomes are vague, e.g., due to spatial ambiguity, patients should have even more difficulty in accurately attributing such outcomes to own or someone else's actions.

Impaired action initiation and the control of voluntary movements are typical symptoms of Parkinson's disease which is accompanied by a decrease in dopamine-producing neurons (Moore et al., 2010). Surprisingly, temporal binding was only increased in patients who received dopaminergic treatment compared to healthy age-matched controls and themselves when not under medication (Moore et al., 2010). Consequently, medical treatment on the one hand aimed at compensating for impaired action initiation might on the other hand have implications for the sense of agency. Saito et al. (2017) reported similar results as in contrast to a healthy control sample, a patient sample did not show action binding but only effect binding. This difference was purely driven by the perception of the patients' actions in the operant condition as estimation errors did not differ between groups in baseline conditions. In addition to temporal binding, the authors also assessed explicit agency ratings. Here, patients reported lower self-attribution of spatially distorted outcomes compared to healthy controls. The authors conclude that Parkinson's disease patients rely on external cues to guide their movements (Saito et al., 2017). This could render action control and accurate agency ascriptions even more difficult in complex action-event sequences where action-sequences are preplanned, and no immediate external feedback helps to guide the movement. Consequently, the study of (impaired) sense of agency in clinical populations helps to carve out the peculiarities of the concept itself (see also Jeannerod, 2009).

In an recent account, Evans (2022) tried to integrate contradictory agency experiences in patients suffering from anorexia nervosa into a coherent picture. Thereby, she attributes the experience of fully controlling the own body and food intake that patients often report before admitting their disease to explicit judgement of agency. At that stage patients claim that they choose to eat very little and that they could start eating more whenever they want. These explicit reports stand in stark contrast to the habitual restrictive food intake that patients exhibit and their struggle with returning to “normal” eating behavior. The author compares these habits to typical habitual behavior which the individual usually does not become aware of which is why it resides in the realm of feeling of agency (Evans, 2022). This integration account draws parallels between experimental research on sense of agency and everyday agency experiences of patients suffering from anorexia nervosa. When making such connections, a critical assessment of the comparability should be done. While the case vignettes refer to agency experiences over a prolonged time and especially the explicit reports reflect complex action-sequences of restricting food intake, the concepts used as reference have thus far mostly been studied in single action-event links. Therefore, further research on the sense of agency in complex action-event sequences as well as complex environments should inform real-life applications and vice versa.

9.4.2. Human-Machine-Interfaces

The importance of system feedback in human-computer interactions has been emphasized numerous times (e.g., Beckerle et al., 2017; Limerick et al., 2014; Shneiderman, 2007). However, these accounts mostly refer to interactions in which the human is in control or at least supposed to be kept in the loop by receiving online feedback (Berberian, 2019; Coyle et al., 2012; see Endsley & Kiris, 1995 for a discussion of the Out-Of-The-Loop performance problem). Studies on the sense of agency in human-machine interactions mirror the results presented in this thesis. For example, efficient support of technical agents resulting in better performance boosted agency ratings and even increased temporal binding in one study (Endo et al., 2020). Contrary, Barlas (2019) reports that judgements of agency were diminished when participants had to execute tasks instructed by a robot. In the same vein, a recent study showed that interacting with artificial agents influences agency ratings differently than merely interacting with a technical device. Agency ratings were lower when jointly acting with an artificial agent compared to interacting with a non-agentic device that could not have any intention by itself (Ciardo et al., 2020). Similar results were obtained when examining sensory attenuation and temporal binding for either self-generated, other-(human)-generated, and machine-generated tones. Participants showed sensory attenuation for all human-generated tones while there was no difference between a control condition and the machine-generated tones (Sahaï et al., 2019; Sato, 2008). While humans exhibit good performance in predicting intentions and movements of conspecifics this is often not the case for machines. With longer action sequences emerges the possibility of system opacity and thereby a lack of system predictability. This in turn could result in poor performance, specifically in joint tasks. Reports from case studies examining how the design of human-machine networks affects performance support this notion (Følstad et al., 2018). When tasks were clearly allocated to either the machine or the operator, synergy effects arose and overall task performance improved (see also Yu et al., 2017). Consequently, such considerations should be taken into account when designing robots because an unintended decrease in agency could result in diminished system acceptance or an inaccurate understanding of the system's capabilities.

As day-to-day human-machine interactions are often more complex than the typical sense of agency study, the results presented in Chapter III and IV add to this picture. They suggest that humans can deliberate about temporally more distant and even ambiguous effects of their actions to conclude own agency or not. It seems plausible that these observations can be transferred to human-machine interactions. In contrast, processing these complex and ambiguous agency cues appears to be more difficult. The fact that there was no action binding for multi-step actions suggests that richer environments, in terms of multi-sensory cues, provide enough information to the sensorimotor system to render temporal judgements very accurate. Thus, the focus should lay on clear causality links and unambiguous system feedback to enable smooth human-machine interactions. Finally, with advancing technologies and the rise of (partially) autonomous artificial agents being operated by laypersons, the focus will shift from human agency to joint human-machine agency. Hereby, a reciprocal understanding of intentions should be developed and methodologies used in psychology and the field of robotics harmonized (Kirtay et al., 2021).

9.4.3. Jurisdiction

Responsibility and legal culpability go hand in hand within the law. Young children for example or people under the influence of delusional substances are exempt from certain forms of punishment or receive milder sentences as reduced liability is considered as mitigating circumstance (Malatesti et al., 2020; StGB, 1998/BGBl I S.3322§ 21). Coercion is another form of mitigating circumstances as actions performed under threat might well be fully intentional but oftentimes they simply reflect a natural desire for the actor’s own safety (see Morse, 2007). Anecdotal evidence therefore has been reported in a study where participants gave lower agency ratings when they were coerced to perform a certain action as compared to acting voluntarily (Caspar, Christensen, et al., 2016). An extreme example of coercion, where subjective agency could be questioned was presented, by Milgram (1963). Here participants administered allegedly unbearable shocks to a confederate when the experimenter instructed them to do so. Additionally, patients with certain forms of psychiatric disorders also fall into the category of reduced liability and the debate is not new who is to blame for fatal system errors of autonomous systems operated by humans (Hage, 2017).

Even though sense of agency is not typically the object of investigation in criminal charges, initial studies acknowledge the potential informative value to the field (see Maoz & Yaffe, 2016). Render and Jansen (2021) found perceived timing of actions to be altered by sexually arousing material while temporal effect perception remained stable irrespective of arousal. The authors state that research in this domain could help understand violent behavior (Render & Jansen, 2021). In contrast, De Pirro et al. (2020) investigated how alcohol influences temporal binding as implicit measure of sense of agency. They found that mild alcohol consumption (within legal limits for driving) significantly increases effect binding, which potentially challenges the current interpretation of reduced culpability under intoxication and relates to moral aspects of sense of agency (De Pirro et al., 2020; Moretto et al., 2011). These studies point towards the importance of sense of agency in vast parts of everyday life (Bandura, 2006).

Another bordering subdiscipline in sense of agency research is the study of sense of agency in social settings (for a review see Silver et al., 2021). Experiments in this field usually examine the sense of agency when humans jointly act or compete against each other to achieve specific outcomes. Beyer et al. (2017) for example examined how joint-responsibility influences perceived control over negative outcomes, i.e., loosing points. Participants rated lower feelings of control over the outcomes when they were co-acting with another person even for their own action-outcomes indicating that sense of agency is subject to social context information (see also Reis et al., 2022). Another study found mutual coordination, joint-performance, and the own role/ contribution as leader or follower to have an influence on participants' control rating from individual to joint-control (Bolt et al., 2016; see also Dewey et al., 2014). These are just few examples that highlight how perceived control and thereby individual responsibility decreases in minimal social settings. Consequently, it does not seem farfetched to assume that diffusion of responsibility or at least shared responsibility will increase with growing complexity of the action-event sequence at hand.

The empirical results presented in this thesis provide first indications of factors that should be kept in mind when assessing an agent's sense of agency and beyond that their responsibility in complex action-event sequences. I have shown that spatial ambiguity for example does not impair a person's ability to infer causal relations, but it still affects control and authorship experiences as well as temporal binding. Additionally, freedom of choice influenced both the strength of temporal binding as well as explicit agency ratings which corroborates previous observations (Bu et al., 2022; Caspar, Christensen, et al., 2016). Damen et al. (2015) reported that participants' sense of agency negatively was influenced by deliberate action plans. Such a detrimental influence becomes even more important when considering complex action-event sequences, as the data presented suggests that participants tend to preplan their actions in these sequences. However, additional research is needed to further detail the influencing factors and carve out how much they contribute to the agency experience that is decisive for jurisdiction. Finally, making the case for an inclusion of sense of agency in jurisdiction inevitably opens up the longstanding debate on the existence of free will which I will not dive into at this point (see also Nichols, 2011). However, in this case it might be somewhat less existential as the sense of agency is defined as the conscious experience of volitional or willed control rather than the affirmed existence of free will (Wolpe & Rowe, 2014). Or as Morse (2007) elaborates, a general capability to use and adhere to rules and regulations suffices to apply the law because the failure to exercise a capacity does not prove a lack thereof. Ultimately, it is important to include both abstract and concrete intentional as well as motor components in the assessment of responsibility and legal culpability as they constitute the basis for the ethical and juridical constructs discussed here (Braun et al., 2018).

10. Concluding remarks

This thesis opens new perspectives on cognitive representations of action-event sequences. Together the results demonstrate that multi-step action-event sequences in some regards are an extension to the single action-event links cognitive psychologists usually study. Phenomena such as temporal binding and manipulations in explicit agency judgments could be replicated in the presented work. However, there is more to it. The combined results suggest that explicit reflections about agency gain more importance as action-event sequences grow more complex and longer in temporal extent. To represent such sequences the cognitive system draws for example on multisensory integration. However, the meta-events deployed for multisensory integration are not infinite which is why explicit agency judgements might still be biased towards own agency ascription while temporal binding used as implicit measure is not. Consequently, integrating research and discoveries from adjacent disciplines, including psychopathology, in addition to agreeing on a narrower definition of the sense of agency are necessary for a holistic understanding of the sense of agency, its underlying mechanisms and resultant interpretations. Finally, the insights presented in this thesis cannot only inform cognitive psychology and the study of consciousness but might also give indications to important questions of life in the near-future with increased human-machine interactions, especially automated systems programmed and initialized by human agents, as well as jurisdiction.

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Appendices

Appendix I: Translated instructions and agency questions of Chapter III

Hello,

Thank you for taking the time to participate in this experiment. During the experiment, you will perform navigation tasks. To do this, you will be asked to follow the specified paths with the help of the arrow keys. Please use only the index finger of your right hand to press the arrow keys.

While performing the task, you will hear letters over the headphones. At the end of each task, you will either be asked when you pressed one of the arrow keys, or when the cursor moved. Please try to make your judgements as accurate as possible by using the entire scale also between the letters. To be able to make good estimations, please wait at least three letters with your first keypress and then press the keys whenever you want. At the beginning of each block, you will be told which event to judge.

In the following there will be an example, helping you to imagine your task:

Forced choice: This is the navigation grid. The path always starts in the middle of the grid and consists of two steps. When you have pressed all the necessary buttons, the navigation begins. After that, a rating scale appears. Once you have submitted your ratings, the next trial starts automatically.

Free choice: This is the navigation grid. The start is always in the middle of the grid. To navigate to the destination, please always press two different arrow keys (even if you could reach the destination in one step or by repeatedly pressing one arrow key). When you have pressed two keys, the navigation begins. After that, a rating scale appears. Once you have submitted your ratings, the next trial starts automatically.

To be able to make good estimations, please wait at least three letters with your first keypress and then press the keys whenever you want. At the beginning of each block, you will be told which event to judge.

Please contact the researcher to clarify open questions and start the first block.

Experimental blocks:

In the next block, the navigation follows your input.

Please focus on pressing the *FIRST arrow key/ SECOND arrow key.*

Please focus on the *FIRST STEP/ SECOND STEP* of the circle.

Baseline blocks:

In the next block, no navigation follows your input. Please focus on pressing the *FIRST arrow key/ SECOND arrow key.*

In the next block, the navigation starts without your input. Please *DO NOT press any arrow key*. Please focus on the *first step/ second step* of the circle.

Temporal judgement questions:

When did you press the *FIRST arrow key/ SECOND arrow key*?

When did the circle take the *FIRST step/ SECOND step*?

The block is now complete. You can take a short break.

To continue, press the Play button in the lower right corner.

Explicit agency judgments:

Continuous scale from strongly disagree to strongly agree

Authorship: The circle moved the way I wanted it to.

Control: I controlled the movement of the circle.

Causation: I caused the movement of the circle.

Appendix II: Analysis of explicit agency judgments of Chapter III

Data analysis

After every eight trial participants rated their control, causation, and authorship over the cursor movement resulting in 6 agency judgements per block (see Appendix I). On these judgements, we performed a 3 (question: control vs. causation vs. authorship) × 2 (event: action vs. outcome) × 2 (position: 1st vs. 2nd) within-subjects analysis of variance. Whenever the sphericity assumption was violated, we report Greenhouse-Geisser corrected statistics. This ANOVA was followed-up by two-tailed paired *t*-tests for pairwise comparisons, with corresponding effect sizes calculated as $d_z = \frac{t}{\sqrt{n}}$.

Results

Forced choice

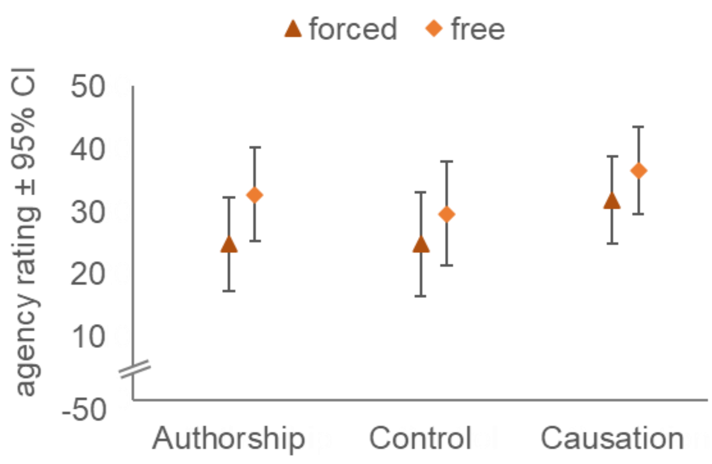
The 3 × 2 × 2 ANOVA with question, event, and position showed a main effect of question, $F(2,94) = 5.87$, $p = .006$, $\eta_p^2 = .11$, $\varepsilon = 0.85$ (GG-corrected) (see Figure 25). Causation ratings were higher than both control ratings, $t(47) = 3.08$, $p = .003$, $d_z = 0.44$, $\Delta = 7.12$, and authorship ratings, $t(47) = 2.50$, $p = .016$, $d_z = 0.36$, $\Delta = 7.06$. Control and authorship did not differ significantly, $t(47) = 0.03$, $p = .977$, $d_z < 0.01$, $\Delta = 0.06$. Ratings tended to be lower when participants had to concentrate on their actions compared to the outcomes, as indicated by the borderline significant main effect of event, $F(1,47) = 3.89$, $p = .055$, $\eta_p^2 = .08$ (see Figure 25). Participants also showed a descriptive tendency to give higher ratings in blocks where they focused on the second event rather than the first, $F(1,47) = 3.50$, $p = .068$, $\eta_p^2 = .05$. No interaction was significant, largest $F_{\text{question×event}}(2,94) = 2.33$, $p = .109$, $\eta_p^2 = .05$, $\varepsilon = 0.90$ (GG-corrected).

Free choice

The 3 × 2 × 2 ANOVA with question, event, and position yielded a main effect of question, $F(2,94) = 3.40$, $p = .037$, $\eta_p^2 = .07$. Causation ratings were higher than control ratings, $t(47) = 2.41$, $p = .020$, $d_z = 0.35$, $\Delta = 6.91$. Contrary, causation and authorship, $t(47) = 1.64$, $p = .109$, $d_z = 0.24$, $\Delta = 3.88$, as well as control and authorship, $t(47) = 1.12$, $p = .268$, $d_z = 0.16$, $\Delta = 3.03$, did not differ significantly. No other main effect or interaction reached significance, largest $F_{\text{question×position}}(2,94) = 3.04$, $p = .059$, $\eta_p^2 = .06$, $\varepsilon = 0.99$ (GG-corrected), all other $ps \geq .149$.

Figure 25

Explicit authorship, control, and causation judgements



Note. After every eight trial, participants rated their authorship, control, and causation of the cursor movement. Triangles show the rating in the forced choice experiment while diamonds represent those with free choice with 95% confidence intervals of the difference of the means.

Table 5

Mean explicit agency ratings (SD) of the experiments in Chapter III

		Action		Outcome	
		1st	2nd	1st	2nd
Forced choice	Authorship	24.94 (19.44)	27.33 (18.18)	21.27 (23.40)	26.00 (22.46)
	Causation	31.32 (19.77)	33.16 (16.24)	31.45 (21.22)	31.88 (20.90)
	Control	26.69 (21.18)	26.88 (17.74)	21.22 (23.71)	24.53 (21.82)
Free choice	Authorship	34.82 (18.35)	32.44 (21.79)	32.38 (19.29)	31.35 (18.97)
	Causation	39.64 (13.18)	35.96 (17.87)	36.46 (18.14)	34.44 (19.59)
	Control	31.58 (21.92)	29.60 (25.00)	28.27 (26.56)	29.42 (23.43)

Note. Participants rated their causation, authorship, and control over the cursor movement after every 8th trial on a visual scale from -50 to 50.

Pooled analysis

To test whether the two experiments differed with regard to the explicit agency ratings, we performed the same analysis as above but also included the factor choice (forced vs. free) as between-subjects factor. Follow-up independent *t*-tests were conducted to further analyze differences.

All explicit agency ratings showed a trend towards higher ratings in the free choice compared to the forced choice experiment, $F(1,94) = 3.27$, $p = .074$, $\eta_p^2 = .03$ (see Table 5). Interestingly, freedom of choice increased authorship ratings, $t(94) = 2.10$, $p = .039$, $d_z = 0.43$, $\Delta = 7.86$, while this was not the case for control, $t(94) = 1.17$, $p = .245$, $d_z = 0.24$, $\Delta = 4.89$, and causation ratings, $t(94) = 1.34$, $p = .185$, $d_z = 0.27$, $\Delta = 4.68$ (see Figure 25). Additionally, there was a position × choice interaction, $F(1,94) = 5.58$, $p = .020$, $\eta_p^2 = .06$, that was spurred by participants' tendency to give higher agency ratings in the free choice experiment especially when concentrating on the first event (action/outcome), $t(94) = 2.30$, $p = .024$, $d_z = 0.47$, $\Delta = 7.71$. Choice did not interact with any other factor, all *F*s ≤ 1.56.

Appendix III: Supplement to Chapter V

Table 6

2 × 2 repeated measures ANOVAs for RTs and % correct in the matching task

Experiment		RT			% correct		
		F	*p*	η_p^2	*F*	*p*	η_p^2
1	Pronoun	0.51	.486	.03	5.04	.040	.52
	Trial type	3.91	.067	.21	2.84	.113	.16
	Interaction	0.01	.944	.01	2.08	.170	.12
2	Pronoun	3.44	.073	.10	5.63	.024	.15
	Trial type	5.46	.026	.15	0.10	.752	.01
	Interaction	0.01	.962	.01	9.13	.005	.23
3	Pronoun	15.06	.001	.33	4.09	.052	.12
	Trial type	38.59	.001	.56	6.00	.020	.16
	Interaction	26.96	.001	.47	3.78	.061	.11
4	Pronoun	2.99	.094	.09	0.07	.790	.01
	Trial type	46.85	.001	.60	7.23	.011	.19
	Interaction	12.04	.002	.28	8.57	.006	.22

Note. Pronoun (self vs. other) and trial type (match vs. no-match) as within-subject factors.

In Experiment 1, we could not replicate typical findings of the self-prioritization effect, i.e., faster RTs for match compared to no-match trials, which raises concern about whether the task setup was suitable to elicit a self-prioritization effect. However, in all other three experiments, match trials were faster than no-match trials (main effect of trial type), replicating typical results from previous studies on self-prioritization.

Appendix IV: Follow-up experiment of Chapter V

In the first experiment of Chapter V, we had to exclude a substantial number of participants due to missed attention checks and incorrect memory of their associated shape. Thus, we conducted another experiment to replicate the initial findings.

Participants

We tested 24 healthy participants recruited over the study platform Prolific (www.prolific.co) who conducted the experiment on their own computer using E-Prime Go (Psychology Software Tools, Inc., 2020). Participants received monetary compensation for their voluntary participation. Two participants were excluded as they did not remember their self-associated shape correctly and one additional participant had to be excluded because they did not respond correctly to any self-match or other-match trial in the matching task[12]. The percentage of failed attention checks in the final data set ranged from 0%-26% (*M* = 7.5, *SD* = 6.1). Participants in the final sample set (14 male, 7 female; 4 left-handed, 16 right-handed, 1 ambidextrous) were between 20-41 years (*M* = 30.0, *SD* = 6.3).

Stimuli and task procedure

The experiment consisted of the same four phases as in Experiment 1: induction phase, agency phase, matching phase, questionnaires. The only change made to the initial study setup was that this time shapes were displayed within the clock face rather than outside of it, to prevent participants from shifting attention away from the clock. That is, the square's edges measured 120 px and the triangle was both 120 px wide and high.

[12] Data of all participants including replaced subjects is available at the project's OSF repository.

Results

Self-prioritization

There was a self-prioritization effect only in the error rates (see Figure 26B). Error rates for self-match trials were significantly lower than for other-match trials, $t(20) = 2.85$, $p = .010$, $d_z = 0.62$. Contrary, reactions times did not differ significantly between self-related and other-related trials, $t(20) = 0.18$, $p = .859$, $d_z = 0.04$, $\Delta = 3.75$ ms. Participants were better at responding to self-related ($d' = 0.51$) compared to other related ($d' = 0.08$) trials, $t(20) = 2.74$, $p = .013$, $d_z = 0.60$.

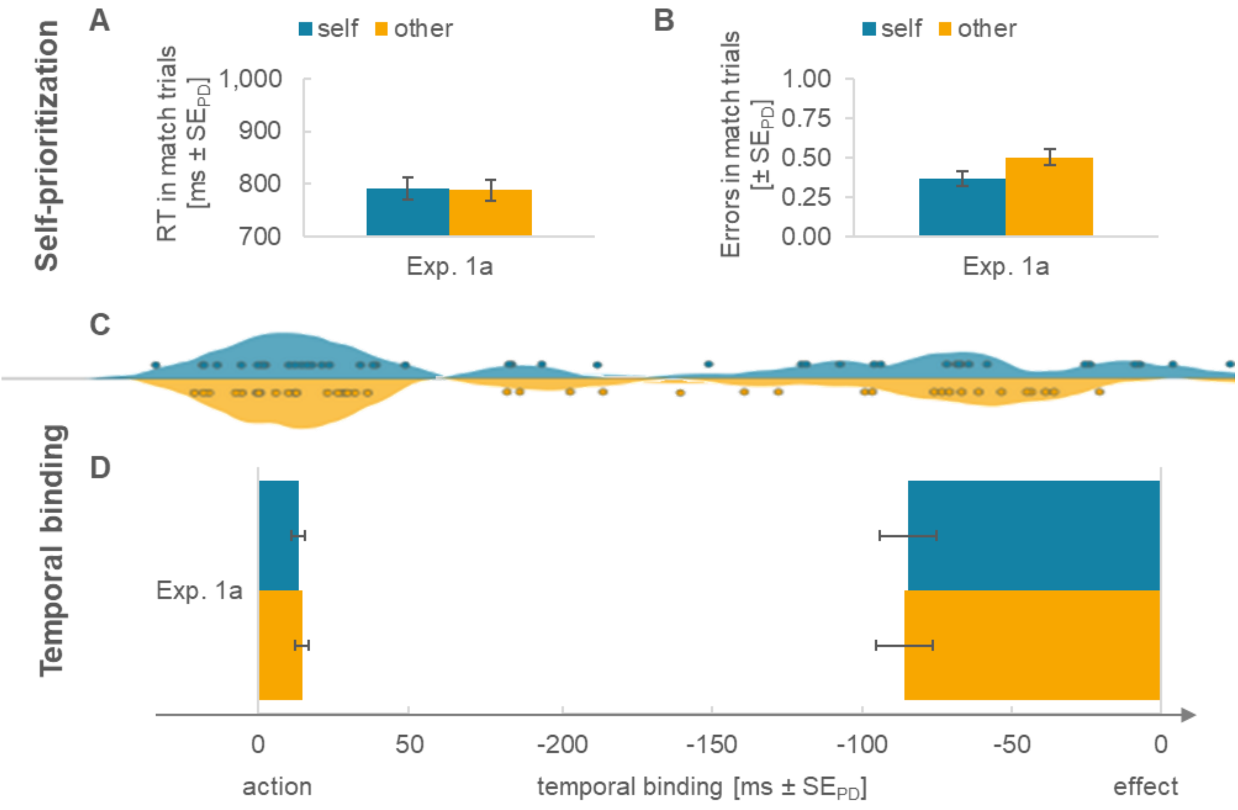
Temporal binding

The 2×2 ANOVA for action binding with condition (baseline vs. operant) and relevance (self vs. other) showed a near significant main effect of condition, $F(1,20) = 4.26$, $p = .052$, $\eta_p^2 = .18$ indicating that participants perceived their action to have happened later when it was followed by an outcome than when the event did not occur. Neither the main effect of relevance nor the interaction reached significance, *Fs* < 1.

The ANOVA for outcome binding also revealed a main effect of condition, $F(1,20) = 33.91$, $p < .001$, $\eta_p^2 = .63$. Consequently, both outcomes were perceived to have happened earlier when they were preceded by a keypress as compared to happening in isolation. There was no main effect of relevance or an interaction, *Fs* < 1. Hence, there was no significant difference in effect binding between self-related and other-related outcomes.

Figure 26

Self-prioritization effect and temporal binding for visual stimuli



Note. (A) Mean reaction times in self-related (blue) and other-related (orange) matching trials. Error bars in all panels depict standard errors for paired differences. (B) Mean error rate in self-related and other-related matching trials. (C) Distribution of action binding and effect binding. Individual points represent participants. (D) Temporal binding in ms. Bars from left to right depict the action shift between the operant and the baseline condition. Bars from right to left show the outcome shift between the operant and the baseline condition.

Appendix V: Complete data analysis to Chapter V

As stated in the method section in 8.1.1, we had to exclude and replace a large number of participants as they did not fulfill the preregistered inclusion criteria. For full transparency, I replicate all the original analyses for temporal binding with more lenient exclusion criteria here. Most importantly, the exclusion criteria did not change any statistical decision.

Failed attention checks

In the original analyses all participants were excluded that failed more than 1/3 of the attention checks. When this criterion was omitted, data of 52 participants could be included in the analyses. They failed between 17%-46% of the attention checks (M = 31, SD = 7). Results of the one-sample t-tests showed that there was only effect binding for the first outcome, $t_{self}(51) = 5.79$, $p < .001$, $d_z = 0.80$, Δ = 75 ms and $t_{other}(51) = 6.26$, $p < .001$, $d_z = 0.87$, Δ = 73 ms. In contrast, this was not the case for the second action-effect, $ts < 1$. Again, effect binding for the first outcome was generally stronger than for the second as indicated by the main effect of position of the 2×2 repeated-measures ANOVA with outcome association and position as within-subjects factors, $F(1,51) = 26.14$, $p < .001$, $\eta_p^2 = .34$. Neither the main effect for outcome association, $F < 1$, nor the interaction reached significance, $F(1,51) = 1.35$, $p = .250$, $\eta_p^2 = .03$. Consequently, even without the stricter exclusion criterion for failed attention checks self-relevance of the outcome did not have any additional influence on temporal binding. This was also reflected in the Bayesian t-tests for paired samples. They provided moderate evidence against a difference between self-related and other-related outcomes both for the first outcome, $BF_{01} = 7.90$, as well as the second outcome, $BF_{01} = 5.77$.

Unequal distribution of keypresses

In the original analyses, we replaced all participants that exhibited a skewed pattern in which key they pressed first. When this criterion as well as the failed attention checks were omitted, data of 61 participants could be included in the analyses. Again, only effect binding for the first outcome was significantly different from zero, $t_{self}(60) = 5.28$, $p < .001$, $d_z = 0.68$,

Δ = 220 ms and $t_{other}(60) = 6.37$, $p < .001$, $d_z = 0.82$, Δ = 265 ms, while this was not the case for the second sound, *ts* < 1. There was again a main effect for position in the 2×2 repeated-measures ANOVA, $F(1,60) = 29.59$, $p < .001$, $\eta_p^2 = .33$, but no other main effect or interaction, *Fs* < 1. Bayesian *t*-tests for paired samples provided moderate evidence against a difference between self-related and other-related outcomes both for the first outcome, $BF_{01} = 9.06$, as well as the second outcome, $BF_{01} = 8.63$.

Memory association

In the original analyses, all participants that did not remember their self-associated sounds correctly after the matching phase were replaced. When this criterion was omitted in addition to the aforementioned criteria, data of 70 participants could be analyzed. There was effect binding for the first outcome, $t_{self}(69) = 5.45$, $p < .001$, $d_z = 0.65$, Δ = 62 ms and $t_{other}(69) = 6.40$, $p < .001$, $d_z = 0.76$, Δ = 64 ms, but not for the second, *ts* < 1. Again, temporal binding for the first outcomes was stronger than for the second, $F(1,69) = 33.83$, $p < .001$, $\eta_p^2 = .33$. Neither the main effect of outcome association nor the interaction reached significance, *Fs* < 1. Consequently, irrespective of the applied exclusion criteria, self-relevance of the outcome did not have an influence on temporal binding. This was also reflected in the Bayesian *t*-tests for paired samples. They provided moderate to strong evidence against a difference between self-related and other-related outcomes both for the first outcome, $BF_{01} = 10.12$, as well as the second outcome, $BF_{01} = 8.21$.