Temporal Binding in Phenomenal Causality

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Published Work

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Summary

The temporal binding effect was first reported by Haggard, Clark and Kalogeras (2002) as a shift in the perceived times of intentional actions and their effects toward one another, and the compression of the perceived interval between them. Research has since shown causal inferences to be necessary for temporal binding to occur (e.g., Buehner & Humphreys, 2009), with some suggesting that the effect results from the perception of causality alone (Buehner & Humphreys, 2009; Eagleman & Holcombe, 2002).

Despite the importance of causality, the mechanisms by which it contributes to temporal binding have received little attention, with the perception of agency being the focus of most temporal binding research. In particular, the role of phenomenal causality has been almost entirely overlooked. Phenomenal causality refers to causal impressions formed visually (Michotte, 1964/63). To date, only one study had made use of visual stimuli giving rise to phenomenal causality to investigate temporal binding (Cravo, Claessens & Baldo, 2009) and no distinction between inferred and phenomenal causality had been made in theoretical accounts of the effect.

This thesis aimed to investigate temporal binding in phenomenal causality with the use of visual stimuli novel to temporal binding research. In experiments 1-6 (Chapter 3) participants provided causal impressions and temporal judgements in response to these stimuli. These experiments found no clear effect of phenomenal causality on the perceived delay intervals between perceived causes and effects. Contrary to the predictions of most accounts of temporal binding, experiment 6 found no evidence for temporal binding in phenomenal causality when intentional actions were present. Experiments 7 and 8 (Chapter 4) investigated temporal binding in inferred causality to test for the possible role of non-causal perceptual influences on the findings of experiments 1-6, such as predictability and the use of visual stimuli. No evidence was found for several such alternative explanations.

While alternative explanations cannot be entirely ruled out, the findings presented here suggest that temporal binding does not necessarily occur due to phenomenal causality in the stimuli used in experiments 1-6. Future research using a greater variety of stimuli may confirm whether this is general to phenomenal causality or specific to certain stimuli or types of visual causal impressions. The findings of experiments 1-6 were not predicted by existing theoretical accounts of temporal binding, suggesting amendments to these accounts are required.

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1. Chapter 1: Temporal Contiguity and Causality

Causal reasoning has a pivotal role in daily life. It guides us beyond reacting to events through association alone; it allows us to predict future events and manipulate our surroundings and informs our decision-making. Simple tasks may require the understanding of multiple, and at times complex, causal structures. For instance, the making of a cup of tea requires the knowledge that the flicking of a switch on the kettle will heat its contents, on the condition that it must be filled with water and plugged into an electrical supply to produce hot water. We understand that the kettle heats the water, rather than the other way around, and that all three conditions are necessary for this sequence of events to take place. We are able to make these inferences despite the temporal delay between the flicking of the switch and the boiling of the water. Beyond the mundane, causal reasoning has enabled humans to develop science, medicine and engineering.

Although causal reasoning is crucial to our understanding of the world, how causality is inferred and perceived has long vexed philosophers and has been the subject of much scientific research. Causality cannot be perceived directly; it cannot be sensed or measured by any organ or scientific instrument. Instead, causal relationships are inferred and perceived through probabilistic, temporal and spatial cues, in addition to knowledge of causal structures and mechanisms. David Hume (1739/1978, as cited in Buehner, 2002) famously conjectured on the cues used to infer causality. Contingency refers to the probability of an event taking place in the presence of another; e.g. the flicking of a switch will usually be followed by a light switching on and the light does not usually switch on when the switch is not turned, allowing the inference that the switch causes the light to switch on. Temporal priority refers the temporal order of cause effect; cause must always precede effect. Returning to the light switch example, if the light turned on before the flicking of the switch the switch one would be unlikely to infer that the switch caused this to happen. Finally, temporal and spatial contiguity refer the temporal and spatial distance between cause and effect. The closer (more contiguous) they are in space and time, the more likely a causal relationship is to be perceived. For instance, the longer the delay between the flicking of a switch and a light turning on, the less likely an observer would be to infer that the switch caused the light to switch on. Likewise, if a lightbulb turned on in the next house rather than in the same room, a similar inference would be made.

These cues – contingency, temporal priority and contiguity – have informed much psychological research of causal reasoning. This section, and indeed the remainder of the thesis, is primarily concerned with the role of temporal contiguity in causal reasoning and perception. Below is a discussion of the existing research on temporal contiguity and causal inference.

1.1. The effect of temporal contiguity on causal inferences

Hume's observation that temporally contiguous events are more likely to be judged as causally related has more recently been evidenced by experimental findings. Studies have found that, all else being equal, contiguity indeed leads to stronger causal judgements. Shanks, Pearson and Dickinson (1989) investigated this in several experiments (experiment 1a and b) and found participants made higher causal judgements when their actions were followed immediately by an outcome than when the outcome occurred following a delay. In these experiments participants' keypresses always produced an outcome (a triangle flashing on a computer screen) with a probability of 75%, while the probability of the same event occurring in the absence of a keypress and the delay between the keypress and outcome was varied between conditions. In all conditions participants were asked to report the likelihood of the outcome following a keypress (contingency judgements), used here as a measure of causal judgements. In both experiments, these judgements were close to the real contingencies in the absence of a delay, whereas judgements were significantly reduced by the inclusion of a temporal delay. Similarly, experiments 2 and 3 found that the longer the delay, the lower the causal judgements were. These findings have since been replicated in several studies (e.g. Reed, 1992; 2009). It should be noted that these experiments never made use of 100% contingencies, meaning participants could never be entirely certain that their keypresses caused the outcome. More importantly, it was clear to participants that it was possible for the outcome event to occur, at least some of the time, without the cause, meaning that if a temporal delay was present there was an increased likelihood that the outcome had occurred due to an event other than the keypress, occurring between the keypress and the outcome. In contrast, on blocks in which there was no delay between keypresses and their outcomes, any visual flash which did not occur immediately following the keypress was very unlikely to have occurred due to the keypress and every visual flash which occurred immediately following a keypress was unlikely to have done so by chance alone. It is possible, therefore, that the differences found in contingency ratings were due to the uncertainty introduced by the inclusion of a temporal delay, rather than because of inferences made due to temporal delays.

1.2. The interaction between temporal contiguity and expected delays

Several studies have elaborated on the findings discussed above by investigating the roles of temporal contiguity and knowledge of causal mechanisms, as well as their interactions. These found that, in adult participants, the effect of temporal contiguity can be mediated or even reversed by knowledge of causal mechanisms. This was demonstrated in a series of experiments by Buehner and May (2002, 2003 & 2004). In their 2002 study, participants took part in a similar procedure to the one used by Shanks et al (1989) but presented with several scenarios influencing the expected delay between their actions and their outcomes. In Experiments 1 and 2 (within- and between-subjects, respectively) these were the lightbulb scenario (switch flick – light turning on) and grenade scenario (the firing of a grenade launcher – explosion at a distance). A pilot study found the median expected delays in these scenarios were 0 and 8 seconds, respectively. Both actions were simulated using a keypress, resulting in visual and auditory stimuli simulating the outcomes. These scenarios were tested using different delays between actions and outcomes: 0, 2 or 5 seconds in experiment 1, and 0 or 5 seconds in experiment 2. After each block of trials participants were asked to estimate the contingency between the action and outcome. The results of both experiments showed an overall trend of lower contingency ratings when temporal contiguity was lower, but a greater effect of delays in the lightbulb scenarios. Experiment 3 made use of the grenade scenario alone, with different causal mechanisms offered; remote controlled explosion (short expected delay) and a grenade launcher (longer expected delay). Again, causal ratings were moderated by delays in both scenarios, with a greater effect in the remote-control scenario.

This was replicated by Buehner and May (2003) in the absence of a cover story. Here, participants were simply either instructed to expect some delay or were given no such instructions. Again, the expectation of a delay mitigated the effect of temporal contiguity on contingency ratings. A further study (Buehner & May, 2004) used a similar procedure with two lightbulb scenarios: one involving regular lightbulb and the other an energy-saving lightbulb, which takes longer to switch on. Participants reported causal ratings between 0 and 100. Here, temporal contiguity was only found to increase causal ratings in the lightbulb scenario. These studies suggest that while temporal contiguity leads to higher impressions of contingency and causality, this effect is dependent on the expected delay between cause and effect. This may have influenced the findings of Shanks et al (1989) and Reed (1992, 1999), as typically keypresses result in a swift response by the computer, rather than a several-second delay. Interestingly, immediate outcomes did not reduce causal and contingency judgements when participants were instructed to expect a delay, despite the converse being true – judgements were reduced by longer-than-expected delays. This suggest that while the effects of temporal contiguity on causal judgements are moderated by the expectation of a delay, they are not necessarily eliminated altogether.

One limitation of these studies, however, is the use of simulated causal interactions. It is not clear whether these findings resulted from participants imagining the scenarios given and their understanding of the causal mechanism, or simply responding being instructed to expect a delay (as in Buehner and May, 2003). Participants were not able to observe the actual causal mechanism responsible for the cause-outcome sequences and were fully aware that the stimuli were controlled by a computer programme. Buehner and McGregor (2006) addressed this with the use of physical apparatus implying real causal structures containing different delay intervals and found that shorter-than-expected delays did result in lower causal ratings. This experiment made use of a Bernoulli board, whereby balls dropped into the apparatus slide along it, hitting pins along the way with a 50% chance of falling either to the right or the left of each pin. The outcome of this event is probabilistic; the ball is most likely to emerge near the centre of the apparatus and less likely to emerge at the peripheries. Upon emerging, the ball had some likelihood of triggering a light switch (manipulated between conditions, repeated measures). The tilt of the apparatus was also manipulated between conditions to determine the delay between the ball being dropped and the light switching on (approximately 1,300 and 2,500ms for the two levels of tilt used in this study). The majority of the apparatus was hidden from view during experimental trials, such that participants could see the tilt of the apparatus but not the progress of the ball along it. In experimental trials the duration of the delay was in fact controlled by a switch operated by the experimenter, of which the participants were not aware. In two experiments, participants made contingency judgements of the extent to which the light switching on was contingent on the experimenter dropping a ball into the apparatus. In contrast in previous experiments (Buehner & May 2002, 2003 & 2004), contingency judgements were lower when the delay was shorter than expected, indicating that temporal contiguity can result in a reduction in causal inferences when a delay is expected. This may have occurred because participants had a full understanding of the causal mechanism presented and the knowledge that, given this mechanism, certain delays were not feasible given the tilt of the apparatus. Here, causal knowledge appears to have fully mediated the effect of temporal cues on contingency judgements; the duration of delays increased or decreased contingency judgements based on how well it fit prior expectations based on the causal mechanism, rather than temporal contiguity leading to higher judgements. By implication, temporal contiguity may only contribute to causal inferences when it is expected, or at least feasible, based on the observer's knowledge of the causal mechanism. Overall, the available evidence suggests that, in adults, temporal contiguity contributes to causal judgements but does not take presence over the knowledge of causal mechanisms and contingency. Furthermore, in some circumstances, such as in Buehner and May's studies and in Buehner and McGregor's study, temporal contiguity does not lead to higher causal impressions.

However, developmental studies (Mandelson & Shultz, 1976; Schlottmann, 1999) have found that in young children temporal cues take precedence over causal knowledge in determining causal inferences. Such findings suggest that temporal contiguity is an important early cue in causal learning, whereas the use of causal knowledge develops later. Mendelson and Shultz (1976) used the following sequence of events: a marble (A) was dropped into a box. Following a 5-second delay, a second marble (B) was dropped into a different aperture in the same box, followed by the ringing of a bell. The children, aged between 4 and 7 years, were then shown several sequences, in which marble A alone was dropped resulting in the bell ringing after 5 seconds and marble B was dropped with no consequence. Children were asked which of the two balls caused the bell to ring after each observation. The children were either informed that the marble causes the bell to ring (in reality, the bell was controlled electronically) by sliding along a runway or were given no model at all. Causal attribution was measured by asking the children several questions regarding the mechanism, e.g. whether the bell would ring if marble A was dropped, whether it would ring if marble B was dropped, etc. Surprisingly, covariation did not appear to affect causal judgments; children showed an overall preference for marble B as the cause of the bell ringing, even after being shown marble B dropping into the apparatus with no consequence. Children made more correct causal judgements when given a model explaining the delay, but most still failed to identify the correct causal mechanism (mean scores of 2.06 and 1.09 out of 5 with and without models, respectively).

Schlottmann (1999) presented children aged between 5 and 10 years with a similar problem. Children were offered one of two explanations for how one marble may cause the bell to ring (experiment 2). The first was that the second marble drops onto a seesaw causing the bell to ring almost immediately, and the second involved first marble sliding down a runway resulting in a longer delay (Mendelson and Shultz, 1976). After each stage of the experiment children were asked questions about the apparatus and causal mechanism. Participant responses suggested that participants of all ages showed a preference for the more temporally contiguous event (the second marble dropped) when determining which marble had caused the bell to ring. Following explanation of the true causal mechanism (half of the participants were assigned to the seesaw condition and half to the runway condition), adults made correct causal attributions whereas children reverted to a preference for the second, more temporally contiguous marble regardless of the true mechanism and despite previously demonstrating an understanding of the causal mechanism. Such findings suggest that while adults rely on causal knowledge and contingency cues to inform causal judgements, children show a greater preference for temporal cues. Temporal contiguity, therefore, appears to be of fundamental importance to causal reasoning, taking precedence over other, more complex cues in early life and remaining a useful cue to causality into adulthood. These findings further suggest that temporal contiguity itself informs causal inferences, rather than doing so only by affecting contingency judgements. In early life at least, outcomes are expected to be temporally contiguous to their causes, even when this is not the plausible based on the causal mechanism.

1.3. The effect of temporal contiguity on phenomenal causality

Phenomenal causality refers to visual impressions of causality. These are distinct from the causal judgements described above; phenomenal causality is perceived rather than inferred. This impression is automatic and does not require multiple exposures or causal learning. Crucially, the effect persists despite the observer's explicit knowledge that the events taking place in the visual stimulus are not causally related (Scholl & Tremoulet, 2000). The most extensively studied example of phenomenal causality is the "launching effect", first reported by Michotte (1946/63) in a series of studies. At the beginning of the sequence an object, e.g. a disc, is seen at the centre of the screen (target). A second object (launcher) moves across the screen towards the target. Upon collision, the launcher stops, and the target begins to move in the same direction as the launcher, at a similar or slower speed. Participants typically report this animation as appearing causal in nature, i.e. the impression that the launcher caused the target to move, in spontaneous reports, forced choice questions and on rating scales. This occurs despite the stimuli being abstract, two-dimensional representations. Michotte and others have reported various characteristics of the stimuli which lead to changes in the magnitude of causal impressions, as well as the qualitative nature of the impression (see Hubbard, 2013 for a review of phenomenal causality stimuli and variations). Some are distinct from the cues used to infer causal relationships. Additionally, covariation across trials in particular is not necessary for these visual impressions to take place; participants still report impressions of a “launch”, even when they had observed the same launcher object failing to move the target on other occasions (Shlottmann & Shanks, 1992).

Nevertheless, some Humean cues are necessary for phenomenal causality to take place. The cause is generally assumed to precede the effect, i.e. it is the launcher that perceived to cause the target to move, rather than the converse. Similarly, spatial and, of particular importance here, temporal contiguity are necessary for phenomenal causality to occur. A number of studies using a variety of measures, discussed below, have repeatedly found reduced launching impressions when a delay is introduced between the stopping of the launcher and movement of the target object. Unlike in experiments concerning inferred causality, discussed in the previous section, visual causal impressions appear much more sensitive to temporal delays.

The effects of temporal contiguity were first reported by Michotte (1946/1963, experiment 29), who presented participants with launching stimuli containing varying delays (0-224ms) between the stopping of the launcher and the onset of the target’s movement. He found that launching impressions were reported more than 50% of the time in delays of up to 70ms, and not at all at delays longer than 126ms. “Delayed launch” impressions were reported more than 50% of the time for delays ranging from 84ms to 140ms. Lastly, participants reported the perception of two distinct movements (causally unrelated) over 50% of the time in delays or 140ms or longer, and 90% of the time in delays of 147s or longer. This was replicated by Yela (1952), who reported reduced causal impressions due to delays between the “collision” and “launch”. Launching impressions were reported 100% of the time with delays of under 50ms and the majority of the time in delays up to 83ms. Impressions of two distinct movements were reported over 50% of the time in delays over 150ms. These studies found that participants tolerated similarly small ranges of delay durations before causal impressions were largely eliminated (up to 140-150ms). Perceived causality appears to show a greater sensitivity to temporal delays between cause and effect than inferred causality.

It should be noted that Michotte’s (1946/1963) and Yela’s (1952) studies contained some methodological flaws. Both used small sample sizes (3 and 5 participants, respectively) and lack inferential statistics. Furthermore, spontaneous reports are limited in scope; participant responses are analysed categorically and do not allow an analysis of the strength of causal impressions. Nevertheless, further research has replicated these findings. Evidence from several studies suggests that, at low ranges of temporal delays, causal ratings decrease as delays increase, with causal ratings falling below the midpoint of causal impression scales beginning at delay intervals of 100-150ms. Schlottmann and Shanks (1992, experiment 1) presented participants with launching animations with delays varying between 17ms and 289ms in intervals of 68ms. Participants reported causal impressions on a sliding scale (300-point resolution). Mean causal ratings decreased as the delay between the collision and launch increased, with mean ratings dipping below the midpoint of the scale (150) in delays of 153ms or longer. Other studies have found causal ratings to fall below the midpoint of a causal rating scale in delays over 105ms (White, 2014; experiment 2) and over 100ms (Guski & Troje, 2003).

The effects of temporal delays on causal ratings appear to be context-dependent, however, and as such there is no fixed threshold for the elimination of causal impressions due to temporal delays. Several experiments have found higher causal ratings in delayed launch animations when participants had prior exposure to longer intervals. Powesland (1959, experiment 2) used presented launching animations with varying delay intervals to find the threshold at which participants report a causal impression 50% of the time (binary responses). Participants were tested before and after a treatment period in which they were presented with launching animations with no delays, long delays or no stimuli. Post-test thresholds decreased for participants who had observed launching stimuli with no temporal delay and increased for those exposed to longer delays, suggesting some habituation to delay durations. The thresholds ranged between 131.94ms to 156.25ms between groups in the pre-tests. Participants exposed to longer delays later showed a 38.19ms increase in their thresholds, while those exposed to no-delay stimuli showed a 41.67ms decrease in their thresholds. No significant change was found for participants who did not observe any additional stimuli, and those who were not required to respond to stimuli in the treatment period. These results were replicated by Brown and Miles (1968) who found participants were more likely to perceive a launching event in delayed launches when they had previously observed longer delay intervals. Young, Rogers and Beckman (2005, experiment 1) presented participants with animations containing different combinations of temporal and spatial gaps between the launching objects. Participants who performed the causal rating task first (i.e. with less prior exposure) reported much higher causal ratings for delayed launches when no spatial gap was present, with intervals remaining at or above the midpoint of the rating scale in delays between 0 and 2,000ms. In addition, in this experiment delayed launches may have appeared more causal in nature in contrast with other animations, in contrast with other experiments in which participants had only been exposed to launching animations containing varying delays.

Nevertheless, the studies described above have repeatedly demonstrated a subtractive effect of delays on launching impressions. This finding has been established to the extent that delayed launches can be used as a non-causal control condition (e.g. Buehner & Humphreys, 2010). While context can moderate this effect, delays between cause and effect reduce launching impressions across different ranges of delays. Temporal contiguity, as Hume conjectured, appears to be an important cue to causal reasoning and perception, all else being equal. While temporal contiguity contributes to both causal inferences and visual causal impressions, the research literature to date has also found some differences between the two. Although temporal contiguity tends to lead to greater causal inferences, these inferences can be over temporal delays of indefinite durations, provided contingency cues and a causal mechanism are provided which allow such an inference to be made. In contrast, temporal contiguity appears to be essential to visual causal impressions, with causal impressions decreasing sharply due to sub-second delay intervals.

2. Chapter 2: Temporal Binding

Past research has revealed several ways in which the perception of time affects our experience of causality (see previous chapter) and agency (e.g. Blakemore, Frith & Wolpert, 1999). The temporal binding effect, first reported by Haggard, Clark and Kalogeras (2002), suggests an additional line of investigation: can our perception of causality and agency affect our perception of time? And, if so, how and why? The temporal binding effect has since attracted significant interest from researchers, both for its potential in helping us understand the role of time in the perception of causality and agency, and for its potential use as an implicit measure of causality and/or agency. Temporal binding has been referred to by several names in the past, including “intentional binding” (Haggard et al., 2002) and “causal binding” (Buehner & Humphreys, 2009). As these terms imply a theoretical position regarding the mechanism behind the effect, it will be referred to here by the more neutral term, “temporal binding”.

In Haggard et al.’s original study (2002) temporal binding was reported as “intentional binding”: the shift in the perceived time of actions toward their consequences, the perceived time of consequences toward the actions that caused them, and a compression of the perceived interval between actions and their consequences. Nine participants took part in all four baseline and three experimental conditions. Timing judgements were recorded using a Libet clock (Libet, Gleason, Wright & Pearl, 1983). In each trial, participants overserved a fast-moving clock (2,560ms per revolution), marked with “5-minute” intervals between 0 and 60. Participants were asked to report the times at which various events occurred using the markings on the clock. Different conditions were presented in separate blocks of 40 trials. In baseline trials participants reported the position of the clock hand at the onset of one of four events. In voluntary action trials condition participants performed a lever press on each trial and asked to report the time of the lever press. In involuntary action trials a movement of the participant’s finger onto the lever was induced using transcranial magnetic stimulation (TMS) and participants were asked to report the time of the finger movement. In sham-TMS trials, used as a control condition for involuntary action trials, TMS was delivered over a brain region that did not produce any muscle movement and participants were asked to report the time at which the TMS was delivered. Finally, in tone trials participants heard a tone and were asked to report the position of the clock hand at the onset of the tone. Participants were asked to make spontaneous finger movements during voluntary action trials, and the onset of the tones and TMS occurred at a random time after each trial began.

In operant conditions the participant’s movement (voluntary or TMS-induced), or sham-TMS, were followed by a tone after 250 milliseconds. Participants were asked to report the clock hand position at the time of either the first event (action or sham-TMS) or tone, in separate blocks of 40 trials. Baseline judgement errors were computed as the difference between the estimate and the actual position of the clock hand during the judged event. These single-event baseline judgements were subtracted from the judgements made in the corresponding two-event conditions to produce the estimate errors. Haggard et al. found that when voluntary actions were followed by a tone, participants perceived their action as occurring later, and the tone as occurring earlier, compared with the single-event baseline. Therefore, the action and outcome were perceived as occurring closer in time. In the involuntary conditions, Haggard et al. found the opposite: a repulsion effect whereby the action was perceived as occurring earlier and the outcome as occurring later, compared with baseline measures. The sham-TMS condition showed neither a binding nor a repulsion effect. This pattern of findings showed a shift in the perceived times of keypresses and their consequences toward one another in operant, intentional action conditions relative to both baseline measures and non-intentional operant conditions.

Temporal binding can be observed in two main forms: event perception and interval perception. Haggard et al.’s (2002) study demonstrated shifts in the perceived times of a voluntary action and its consequence toward one another. Other studies using measures of interval rather than event perception (e.g. Humphreys & Buehner, 2009) have demonstrated that temporal binding also manifests as a shortening of the perceived time interval between a voluntary action and its consequence (although it should be noted that they argued that this is the result of causality, rather than voluntary action, see Section 2.3). In both cases temporal binding observed as a shift in event or interval perception, relative to other subjective impressions. Temporal binding can be observed, for instance, if the interval between two events is overestimated relative to physical reality, but underestimated relative to a single-event baseline, or a sequence of two causally unrelated events (e.g. the sham-TMS condition in Haggard et al., 2002). Although these two forms of temporal binding do show some differences and may arise from different cognitive mechanisms to an extent, theoretical accounts of temporal binding generally see both as arising from similar causes, e.g. intentionality in intentional binding accounts and causality in the causal binding account. As such, the theoretical implications of findings from studies investigating event and interval perception will be discussed alongside one another where appropriate in this thesis. However, it should be borne in mind that event perception findings may not necessarily be applicable to interval perception, and vice versa.

2.1. Replications of temporal binding

Temporal binding has been observed repeatedly, although the exact magnitude of the effect varies between different experiments and methodological differences such as delay intervals and measures (see Section 2.2). This section describes the evidence for the existence temporal binding to date, as well as the existing variations and research techniques used to study it.

Temporal binding has been replicated repeatedly with the use of Libet clocks (e.g. Desantis, Roussel & Waszak, 2011; Dogge, Schaap, Custers, Wegner and Aarts, 2012; Moore & Haggard, 2008; Ruess, Tomaschke & Kiesel, 2017, Wohlschlager, Engbert & Haggard, 2003). In addition, the negative (i.e. backward) shift in the perceived time of the consequence of an action has been demonstrated using a variety of measures of event perception, including a key synchronisation task, in which participants were instructed to press a key at the same time as the target event (Buehner & Humphreys, 2009). This negative shift has also been demonstrated using simultaneity judgements, whereby participants were instructed to judge whether a visual event embedded between the key press and tone, or occurring after the tone, occurred simultaneously with the tone (Cravo, Claessens & Baldo, 2011). This study found lower points of subjective simultaneity (PSS) when the tone was caused by intentional action, compared with a non-causal sequence of events. This indicates that the embedded event had to occur earlier in order to be perceived as simultaneous with the tone, and that the tone was therefore perceived as occurring earlier. Although Cravo et al.'s study only investigated the shift in the perceived time of the outcome and not the action, such replications have demonstrated the robustness of the temporal binding effect, which appears to be replicable across a variety of event perception measures.

Similarly, a contraction in the perceived interval between intentional actions and their effects has been demonstrated using a variety of measures. Interval perception has primarily been investigated using interval estimations, in which participants made direct estimates of temporal delays in milliseconds (e.g. Humphreys & Buehner, 2009; Moore, Wegner & Haggard, 2009). Other studies have made use of interval reproduction tasks, whereby participants held down a key for the perceived duration of a delay (Humphreys & Buehner, 2010). Further evidence comes from the use of the method of constant stimuli (Nolden, Haering & Kiesel, 2012), where participants judged whether delays were greater or smaller than a comparison interval (tones of varying durations). Lower points of subjective equality (PSE), i.e. shorter delays were perceived as equal in duration to the delay between action and outcome, were found when participants performed an action that caused a visual stimulus to appear, compared with the passive condition, in which a key "popped up" against the participant's finger, followed by the same delays and visual stimuli.

The examples discussed above demonstrate the range of event perception and interval estimation measures which had been successfully used to replicate the temporal binding effect. Despite this, to date, Libet clocks and interval estimation have been used in the majority of temporal binding studies, and indeed the majority of studies discussed here.

2.1.1. Replications of temporal binding with non-auditory outcome events

To date, the majority of temporal binding studies have used a tone as the outcome stimulus. This may in part be because the use of auditory stimuli allows the use of Libet clocks without causing participants to shift their attention from the clock, and without drawing participants' attention away from the outcome stimulus. Nevertheless, several studies have found evidence for temporal binding with visual outcomes with a variety of measures, although usually not with the use of a Libet clock (see Ruess et al., 2018, for a more detailed discussion). Nevertheless, Ruess et al. (2018) found outcome binding of visual stimuli, compared with a single-event baseline, in delay intervals ranging between 150 and 650ms. A Libet clock was used, with a change in the colour of the clock face as result of a keypress serving as the outcome. A significant forward shift compared with the single-event baseline was found in all time intervals. However, a second experiment found greater outcome binding in auditory outcome stimuli (a brief tone) compared with visual outcome stimuli.

Temporal binding between actions and outcomes, measured using an interval estimation task, was also found by Engbert, Wohlschlager and Haggard (2008) when using auditory (experiment 1), visual (experiment 2) and tactile (experiment 3) outcome stimuli. In all experiments lower interval estimates were found when participants performed voluntary actions (lever presses), as opposed to passive movements, whereby the participant's finger was pulled down with the lever, followed by the outcome stimulus. Other evidence for temporal binding between voluntary action and tactile response, using an interval estimation procedure, was reported by Engbert, Wohlschlager, Thomas and Haggard (2007). In this study, evidence was found for temporal binding with tactile stimuli (vibration), both when applied to the participant's finger and when participants observed same stimulus applied to the experimenter's finger.

The existing literature has repeatedly replicated temporal binding between actions and auditory, visual and tactile outcome events and no studies published to date have failed to find temporal binding when using non-auditory outcome stimuli. However, while a variety of outcome stimuli have been used in temporal binding experiments, there is far less variety in the types of actions studied; intentional actions in such studies have been almost entirely limited to lever- or key-presses. Key and lever presses are markedly different from other real-world intentional actions in being swift and producing a discrete outcome accompanied by tactile feedback. Other intentional actions may be longer in duration and have continuous outcomes. At the time of writing it is not clear whether such differences may affect temporal binding and how they might do so. The findings discussed here suggest that non-auditory outcome stimuli are appropriate in studying temporal binding, with the qualification that the role of the sensory modality of the preceding event is less well-understood.

2.2. Mechanisms of temporal binding

In the seventeen years since temporal binding was first reported, much work has been done to shed light on how the effect manifests. This section discusses parameters of temporal binding which are important in understanding the phenomenon, and those particularly relevant to the experiments reported in this thesis and their interpretation. The nature of action and outcome binding, and the effects of interval duration and predictability are discussed below, in relation to the different measurements of time and event perception used in studying temporal binding.

The positive shift in the perceived time of causal actions and the negative shift in perceived time of outcomes can be referred to as action binding and outcome binding, respectively. Early evidence of temporal binding using Libet clock procedures suggested that outcome binding tends to be of a greater magnitude than action binding and may account for the bulk of the contraction of the perceived delay interval between actions and their outcomes. Haggard et al.’s 2002 study found that action binding caused a mean shift of +15ms, whereas outcome binding caused a mean shift of -46ms. Similarly, Haggard and

Clark (2003) reported action binding of +30ms, compared with outcome binding of -78ms when participants performed a voluntary action, which caused a tone to sound, using the Libet clock method.

Both the shifts in the perceived time of the action and outcome are at times used to infer the overall magnitude of the perceived duration between action and outcome. However, estimates of the times of actions and outcomes are collected in separate trials. As discussed below, the findings made using Libet clocks do not always match the findings made using measures of perceived interval durations, suggesting some differences in the mechanisms giving rise to temporal binding in event and time perception.

2.2.1. Predictive and postdictive processes

Research on action binding suggests it occurs in anticipation of the outcome (prediction), as well as following the outcome itself (post-diction). The magnitude of action binding appears similar when the outcome event is expected but does not occur to when an outcome does occur, suggesting that the predictive and postdictive components of action binding do not have an additive effect and high predictability is not necessary for the effect to occur. This was demonstrated by Moore and Haggard (2008) who manipulated the probability of a tone occurring as result of participants' key presses. Participants performed a similar temporal binding task as used by Haggard et al. (2002), with the odds of the tone occurring fixed at either 50% or 75% of trials in different blocks (low and high probability conditions, respectively). Thus, the actions performed by participants were more predictive of the tone in some trials than others. A Libet clock was used to collect estimates of the time of the keypress in single-event baseline blocks (keypress only) and operant blocks in which keypresses could result in a tone. Estimate errors were computed by subtracting mean estimates from operant conditions from mean baseline measure. In low probability blocks, a significant forward shift in the perceived time of the keypress (action binding) was only observed when the tone was heard, but not on trials in which there was no tone (mean shifts of +13ms and +4ms, respectively). In high probability blocks significant action binding was found both on trials in which the tone occurred and trials in which it did not (+16ms shift in no-tone trials, +13ms shift in tone trials). These results indicate that temporal binding can occur in anticipation of the outcome event, but that the probability of the outcome only affects predictive action binding. On the other hand, provided the outcome is perceived as caused by the action, post-dictive temporal binding appears to be unaffected by the probability of the outcome.

Other work has since expanded on this finding by distinguishing between probability and contingency, which were conflated in the above study. In the above example, the tone never occurred in the absence of the keypress and therefore the contingencies between the keypress and tone were identical to the probability of the tone sounding. Moore, Lagnado, Deal and Haggard (2009) found that both the probability of the tone sounding and the contingency between the keypress and tone contributed to action binding. More specifically, contingency contributed to predictive action binding when participants' actions were highly predictive of the outcome and contributed to postdictive action binding when they were not. In a similar design to that of Moore and Haggard (2008), the authors manipulated the probability of the tone occurring between subjects, and contingency within subjects. Participants were asked to make a key press on approximately half of the trials. In high contingency blocks, the probability of the tone occurring if an action was made was 50% at low probability and 75% at high probability, with 0% and 25% probabilities of a tone occurring regardless of action, respectively. In low contingency blocks the probability of the tone occurring when an action was made was equal to the probability of the tone occurring when no action was made (50% in low probability trials and 75% in high probability trials). This resulted in a fully factorial design of two probability conditions (low = 50%, high = 75%) and two contingency conditions (low = 0, high = 0.5). These were compared to baseline trials, in which key presses were never followed by tones, and the key was pressed on every trial. Forward shifts were observed in all conditions, with the exception of action-only trials at low tone probabilities.

This study both replicated the findings reported by Moore and Haggard (2008) and expanded on them by demonstrating an additive effect of contingency and the probability of outcome in some circumstances. The presence of action binding when the probability of an outcome or contingency between an action and outcome are high suggests that this action binding takes place before the outcome occurs. It should be borne in mind, however, that this work on predictive binding does not necessarily apply to outcome binding or to the perceived duration of the interval as a whole. This is because estimates of the time of the outcome cannot be made by participants when an outcome had not occurred.

The extent to which temporal binding in time perception, as measured by interval estimation procedures or other time perception measures, occurs due to shifts in event perception is not yet known. However, recent research has found evidence for changes to internal clocks during temporal binding tasks (Fereday & Buehner, 2017; Fereday, Rushton & Buehner, in press). This suggests that changes to time perception occur during delay intervals, in anticipation of the outcome stimulus, and that predictive processes contribute to the perceived duration of the delay. It further suggests, however, that the contraction of subjective time intervals in temporal binding does not result entirely from changes in event perception.

2.2.2. The effects of interval length on temporal binding

Findings regarding the effects of interval length on temporal binding have been mixed, with some finding longer delay intervals to lead to a decrease in temporal binding, while others have not. In their 2002 paper, Haggard et al. reported decreasing outcome binding at longer intervals (experiment 2), with outcome binding most prominent at 250m delays between action and outcome, lesser at 400ms delays, and lesser still at 650ms delays. When the intervals were presented at a random order, rather than in separate blocks, no evidence for outcome binding was found. These findings suggested that temporal binding the effect of temporal binding on event perception diminishes as contiguity decreases and may be altogether absent at longer intervals.

However, some recent findings suggest a more complex picture of how different delay intervals affect both action and outcome binding than previously suggested, indicating that the range of the delay intervals presented to participants moderate the effect of delay intervals. Although a recent study using the Libet clock method found a decrease in the magnitude of the temporal binding effect at greater delay intervals, both for visual and auditory delays (Ruess, Tomaschke & Kiesel, 2018), another recent investigation using a Libet clock (Ruess et al., 2017) found that this effect is moderated by the range and predictability of delay intervals. The first of two experiments found outcome binding increased with delay intervals at a small range of intervals (experiment 1; 200, 250 and 300ms intervals). Outcome binding did decrease at 400ms intervals, when the interval range was greater (experiment 2: 100, 250 and 400ms). The experiments additionally investigated the role of predictability: in unpredictable sessions, trials of all delays were presented in a random order within the same block, whereas in predictable blocks trials contained the same delay between the action and outcome within each block. Interval length was found to diminish outcome binding for predictable intervals only, and no overall reduction in outcome binding was observed in unpredictable intervals. Action binding increased at longer intervals in experiment 2, but not experiment 1. Similarly to the outcome binding findings, unpredictable intervals did not lead to a decrease in action binding.

The findings from experiments using the Libet clock method are mixed, although the majority of experiments did find that temporal binding is moderated by increases in delay intervals. These findings suggest that temporal binding diminishes at longer delay intervals but are not necessarily generalisable to other measures of event perception. Two experiments using a key synchronisation task have found evidence for temporal binding at intervals of 500, 900 and 1300ms (Buehner & Humphreys, 2009), suggesting that the effects discussed above may have occurred due to the use of Libet clocks and do not apply to event perception in general.

Other findings show that if there is a moderating effect of increasing delay intervals on temporal binding, it is limited to event perception. Different measures of interval estimation have repeatedly found evidence for temporal binding at longer intervals. For instance, in two experiments Humphreys and Buehner (2010) demonstrated temporal binding at intervals varying randomly between 1200 and 1600ms, using an interval reproduction task. Participants were instructed to make temporal estimates by holding a key down for the duration of the temporal gap between two events. Here, temporal binding is evident from the significant difference in estimates between action-outcome sequences and unrelated, two-event sequences. Similarly, when using direct interval estimates, Humphreys and Buehner (2009) found evidence for temporal binding at intervals of up to 4 seconds, with no evidence of a decrease in temporal binding at longer intervals. In a series of experiments, temporal estimates were consistently shorter for the interval between action-outcome events, compared with estimates of the interval between two unrelated events. Slope analyses found significantly shallower slopes in the interval estimates made for action-outcome sequences, suggesting that the difference in temporal estimates widened as the judged interval increased. The authors suggested that this disparity in the effect of interval length on temporal binding between measures of event perception (e.g. Libet clocks) and perceived intervals (e.g. interval estimates) may reflect a fundamental difference in the mechanisms leading to temporal binding in these cases. It is suggested that temporal binding in perceived intervals may result from an anticipatory slowing of an internal pacemaker. As mentioned previously, evidence has since been found for a slowing of an internal pacemaker rate during the interval between an action and its outcome (Fereday & Buehner, 2017; Fereday, et al., under review).

The findings discussed in this section have both theoretical and practical implications for temporal binding research. Measures of perceived intervals, rather than perceived events, have consistently found temporal binding at longer intervals than those initially reported. This appears to reflect a difference in the effects of temporal binding on the perception of events (as typically measured using the Libet clock method in temporal binding studies) and intervals (as typically measured using interval estimates). The two may well result from separate underlying perceptual processes. While temporal binding studies often yield similar findings with measures of event and interval perception, the effect of interval length demonstrates that this is not always the case and temporal binding findings are not necessarily applicable across these differing perceptual processes. It is further evident that in studies of temporal binding, at least when using a Libet clock, findings may be affected by the range and granularity of delay intervals.

2.2.3. Predictability

Along with contiguity and interval length, the effect of the predictability of the duration temporal delays on temporal binding has not been consistent between studies and appears to be restricted to event perception alone. Haggard et al. (2002) found diminished outcome binding when delay intervals were variable, while Ruess et al. (2017) did not find evidence for this, for either action or outcome binding. Cravo et al. (2011) reported a significant interaction between the presence of action, interval predictability and interval duration (experiment 2) in outcome binding. Cravo et al. made use of a simultaneity judgement task, in which participants reported whether a flash occurred at the same time as the outcome tone. Flashes were presented at varying intervals before or after the tone. Participants either pressed a key to cause the tone to sound, or passively observed the disappearance of a fixation cross, followed by the tone. The tone was presented either 300 or 600ms after the action/disappearance of the fixation cross. In some blocks only one of the two delay intervals was used, whereas other blocks included trials containing multiple displays of both delay durations, presented in a randomised order. Earlier points of subjective simultaneity, suggesting the tone was experienced as occurring earlier, were found in action trials, when the onset tone was predictable and the delay was short, compared with other conditions. It is possible that less predictable intervals lead to a reduction in temporal binding as, although the outcome is predicted by the preceding event, the time of onset of the outcome is not.

This lack of certainty in the delay between the action and outcome may lead to a decreased reliance on the action as a cue to the onset of the outcome, relative to other perceptual cues.

In contrast, there is no evidence to date for an effect of predictability on temporal binding as measured by interval estimation and interval reproduction procedures, with some studies finding evidence for temporal binding when using delay intervals of unpredictable duration. Buehner & Humphreys (2010) replicated the temporal binding effect in randomly varying intervals with an interval reproduction task, and Humphreys and Buehner (2009) replicated the temporal binding effect in unpredictable intervals with an interval estimation task. As discussed in the previous section, recent research has implicated internal pacemaker rate changes in temporal binding (Fereday & Buehner, 2017; Fereday, et al., under review). While the reduced predictability of delay duration may make the exact onset of the outcome stimuli impossible to predict, the prediction of a reduction in the delay between the two, and therefore the anticipatory slowing of internal pacemaker rates, may still occur.

Nevertheless, at the time of writing there have been few direct investigations of the role of predictability in temporal binding and any explanations of such effects are speculative. The effect of the predictability of temporal delays on temporal binding appears to be dependent at least in part on the experiment design and in particular whether time or event perception are measured. As with the effect of delay durations, the evidence for an effect of predictability on action and outcome binding is mixed, with some studies having found reduced binding under low predictability. These findings are unique to measures of event perception, while interval estimation and interval reproduction tasks finding no effect of predictability on temporal binding.

2.2.4. Mechanisms of temporal binding: summary

In the sixteen years since it was first reported, numerous replications have shown temporal binding to be robust and replicable. The effect has been found with the use of various measures of time and event perception, and across varying time intervals, both predictable and unpredictable. Action binding appears to be lesser in magnitude than outcome binding, with both predictive and inferential processes contributing to the overall effect. Evidence for a role of internal clock changes in temporal binding also suggests a predictive component to the shortening of the perceived length of the interval, as such a change takes place prior to the outcome. What is less clear is how the extent to which the perceived time of the outcome affects the perceived interval length postdictively.

Temporal binding does, however, manifest in different ways depending on the measures used. While the manifestations of temporal binding in event and time perception are at times treated as interchangeable, research on the susceptibility of temporal binding to changes in stimuli has demonstrated some differences. As discussed above, some studies have found diminished action and outcome binding due to delay intervals which are longer or less predictable in their duration, but this was not the case for judgements of the interval length. Another key difference is that the perceived time of the action and outcome cannot be inferred from interval judgements, and that the perceived interval of a delay may not necessarily be inferred from shifts in the perceived time of the two events. These differences suggest the involvement of different perceptual processes in temporal binding as observed in event and interval judgements. Although this need not indicate two entirely separate temporal binding effects, caution must be exercised in interpreting findings that had not been replicated across different measures. In practice, different measures inform theoretical accounts of temporal binding in different ways. Measures of event perception have been better suited to investigations of pre- and post-diction, for instance. Interval judgements, on the other hand, are more suitable when the experiment design necessitates the use of delay intervals over 500ms, or unpredictable intervals.

2.3. The roles of causality and agency in temporal binding

At the time of writing it remains the dominant view that temporal binding results from the perception of agency, although multiple accounts of how and why this might be the case have been proposed (see Moore & Obhi, 2012). Alternatively, some have proposed that the perception of causality, rather than agency, leads to temporal binding (e.g. Buehner & Humphreys, 2009; Eagleman & Holcombe, 2002). The majority of the accounts that have been put forward to explain temporal binding take the position that the perception of agency is necessary for temporal binding to occur and can be categorised as agency accounts, while the main alternative explanation – that temporal binding occurs due to the perception of causality alone is referred to here as the causal account of temporal binding.

The question of the roles of causality and agency is of both theoretical and practical value to researchers. Temporal binding has the potential to expand our knowledge of how the perception of time, and causality and/or agency are related, and to provide researchers with implicit measures of the perception of causality/agency. Studies are increasingly making use of temporal binding as an implicit measure of agency when studying a variety of disorders, personality traits and mental states. Examples include studies of schizophrenia (Haggard, Martin, Taylor-Clarke, Jeannerod & Franck, 2003; Voss et al., 2010), narcissism (Hascalovitz & Obhi, 2015), autism spectrum disorders (Sperduti, Pieron, Leboyer, & Zalla, 2014), mindfulness meditators (Jo, Wittman, Hinterberger & Schmidt 2014; Lush, Naish & Dienes, 2016) and the effects of ketamine (Moore et al., 2011). In such cases differences in temporal binding between groups are used either to provide evidence for differences in the experience of agency, to investigate the role of agency in temporal binding, or both. Yet, causal and agency accounts of temporal binding often lead to similar predictions. As such, interpretations of temporal binding studies may vary considerably depending on one's theoretical position. Temporal binding has the potential to further our understanding of how we perceive time, causality and agency, but this is contingent upon deciphering the roles that causality and agency play in producing the effect.

2.3.1. Causality as a necessary condition for temporal binding

It is widely accepted that perceived causality between action and outcome (or cause and effect) is necessary for temporal binding to occur. To date, several studies have found evidence for this (e.g. Buehner & Humphreys, 2009; Moore & Haggard, 2008). However, although the evidence consistently points in this direction, there have been relatively few studies to date which have included causality and agency as distinct factors. This may be, in part, because, while creating conditions in which causality is present and agency is absent is relatively simple, it is far more difficult to design conditions in which an intentional action is consistently and predictably followed by an outcome, while it remains clear to the participant that the outcome was not caused by the action in any way.

Buehner and Humphreys (2009) demonstrated causality to be necessary for temporal binding to occur using a key synchronisation task. Participants were asked to press a key simultaneously with two target tones, separated by a delay of 500, 900 or 1,300ms. To allow participants to predict the onset of the first target tone it was preceded by two preparatory tones of a different pitch occurring 400 and 200ms before the first target tone. Participants completed exposure trials prior to each experimental block, in which they were free to either press the keys or not, in order to learn the contingency between their keypress and the second tone. As such, predictability was identical across conditions, as was the presentation of the stimuli and presence of a keypress, but participants' knowledge of the contingency between their action and the second tone was not. In two experiments, the times of the keypresses were compared with the time of both tones. Both experiments found evidence for temporal binding when the action caused the tone compared with tones occurring independently of the action. This indicates that intentional action alone did not lead to temporal binding in the absence of a causal relationship between the action and the subsequent tone, as measured using the key synchronisation task. Temporal binding may not occur simply between an intentional action and any subsequent event, therefore, but the subsequent event must be contingent on the action.

Other studies provide indirect evidence for the role of causality in temporal binding as they have shown a decrease in temporal binding due to a decrease in Humean causal cues (see Chapter 1). As previously discussed, Moore and Haggard (2008) and Moore et al.

(2009), found evidence for greater action binding at higher than lower contingency between actions and outcomes. Similarly, temporal contiguity, which also contributes to causal inferences, has also been found to affect temporal binding, with reduced temporal binding when contiguity is reduced (e.g. Haggard et al., 2002, experiment 2). However, as discussed in the previous section, these effects appear to be specific to event perception and may not be generalisable to all forms of temporal binding.

Perceived causality as a necessary condition for temporal binding has been largely accepted by researchers investigating temporal binding. Although this has been difficult to test directly, research to date has not demonstrated temporal binding in the absence of a causal relationship between the two events. The vast majority of temporal binding research is carried out under the assumption that the two events, whether action and outcome or cause and effect, must be perceived as directly causally related. It is arguable that this may the case by definition, under both agency accounts and the causal account of temporal binding. Because “intentional binding” as originally reported referred to a temporal binding between actions and their intended consequences; if temporal binding occurs due to motor planning processes it can only take place when the aim of the action is to bring about the outcome. If an outcome is perceived to occur independently of the action, no such planning is required. Likewise, causality as necessary for causal binding is a key feature of causal accounts. The remainder of this chapter, therefore, will consider the perception of causality as necessary for temporal binding to occur.

2.3.2. Agency accounts of temporal binding

The focus of this section is the evidence for and against intentional action and agency as necessary conditions for temporal binding, rather than the precise mechanisms by which this might occur. Agency refers to subjective impression of actions as generated voluntarily by an agent (see David, Newen & Vogeley, 2008, for more detail). Intentional action refers to goal-directed actions, performed by an agent. For the purposes of discussing the temporal binding literature, this will refer to voluntary human actions, as opposed to mechanical actions. Involuntary actions include actions induced through coercion, TMS or physical force (e.g. a participant's finger pushed down onto a lever). Crucially, while the actual nature of agency is contentious and it can be argued that free will is illusory or that mechanical actions may be intentional as they were designed by an agent, it is the subjective perception of agency and intentionality that is of interest here.

In their 2002 paper, Haggard et al. termed the temporal contraction between intentional actions and their consequences "intentional binding". They proposed that the binding in time of actions and their intended effects was an adaptive process, serving to strengthen the association between them. It was suggested that this process aids the matching of actions to their intended effects via forward action models. Forward models have been proposed as a means by which motor actions are optimised by predicting the sensory outcome of an action. They are widely viewed as contributing to individuals' sense of agency over their actions; sensory outcomes which closely match the predicted outcome have been found to increase feelings of agency, whereas sensory outcomes that deviate from forward model predictions lead to decreased feelings of agency (see David et al., 2008, for a review of such findings). Temporal binding has been suggested by Haggard et al. to strengthen feelings of agency by reducing the perceived temporal gap between actions and their outcomes, in line with forward model predictions. Conversely, the "repulsion effect" found when motor action was TMS-induced was suggested to dissociate forced movements from their consequences, thereby weakening the perception of agency in those cases. It is worth noting that this repulsion effect has not been replicated since, for instance a 2003 study by Haggard and Clark found an absence of temporal binding in TMS-induced actions, but no repulsion effect. Similarly, other studies investigating the role of agency using forced actions other than TMS-induced movements (discussed below) have not found such a repulsion effect.

Although this was the first theoretical account of temporal binding, it has not been without controversy (for instance, see Hughes, Desantis & Wazsak, 2013 for a systematic review and critique of this account). Most research on temporal binding has not sought to find confirmatory evidence implicating forward models in temporal binding, yet this theoretical account had informed much of the temporal binding research since the effect was first reported. In addition, some amendments to this account had been made. For instance the assumption of a repulsion effect due to non-intentional movements had never been replicated. More recently it has been suggested that forward model predictions influenced event perception as part of a cue integration process, whereby the weighting of this information against other perceptual information affected temporal binding, but only in action binding (Wolpe, Haggard, Siebner & Rowe, 2013). The latter point would suggest that different manifestations of temporal binding - outcome binding and the shortening of the perceived interval between actions and their consequences – may in fact be driven by different perceptual processes resulting from the perception of agency. For instance, as previously discussed here (Sections 2.2.2. & 2.2.3.), temporal binding only appears to be moderated by longer or less predictable delay intervals when event perception is measured (using a Libet clock).

The forward model explanation for temporal binding relies on some assumptions which have not been confirmed, and indeed are difficult to test. One such assumption is that participants implicitly expect short delays between their actions and their sensory consequences despite repeated exposure to the stimuli, and despite the role of forward models in temporal prediction. This is a counterintuitive assumption, and one that would mean temporal binding, suggested by Haggard et al. (2002) to aid the perception of intentionality, would do so by decreasing the accuracy of temporal prediction and motor planning. As this account is not necessarily predicted by the existence of forward models, evidence for the existence of forward models does not amount to evidence for their role in temporal binding. In addition, several findings to date are difficult to explain under the forward model account of temporal binding. For instance, it is not clear how forward model predictions would affect internal pacemaker rates, yet recent findings suggest temporal binding results, at least in part, from changes to internal pacemaker rates (Fereday & Buehner, 2017; Fereday, et al., under review). Likewise, some findings discussed below, such as the lack of repulsion effect (Section 2.3.2.1), temporal binding in observed actions (2.3.2.2) and findings suggesting temporal binding results from explicit agency judgements, rather than implicit agency (2.3.2.3).

Overall, the forward model account of temporal binding has been the most prominent agency account of temporal binding in the research literature, despite the shortcomings discussed above. However, it is not the only possible reason why the perception or beliefs of agency may be necessary for temporal binding to occur and, as such, evidence against this model does not necessarily amount to evidence against a role of agency in temporal binding. A further caveat is that some research on temporal binding does not cite or propose a specific account of temporal binding, instead assuming only that agency is necessary for the effect to occur. Because of this, evaluation of the evidence for and against agency accounts of temporal binding will focus on the role of the perception of agency rather than predictions specific to individual agency accounts.

2.3.2.1. Evidence for intentionality as a necessary condition for temporal binding: involuntary movement

To date several studies have investigated the prediction of agency accounts, that temporal binding should not take place due to movements performed involuntarily and have found some of evidence that this leads to a reduction, if not an absence of temporal binding. Two previously mentioned studies (Haggard & Clark, 2003; Haggard et al., 2002) made use of TMS to induce involuntary hand movements. TMS-induced movements are, however, markedly different from ordinary motor actions and are likely to be very unnatural for the average human, who, outside of neuroscience experiments, would have no prior experience of such movements. It seems likely, therefore, that these movements, which entirely bypass ordinary motor planning and feedback, may affect event perception in other ways.

Other studies made use of different forms of forced movement and found evidence for a decrease in temporal binding. Wohlschlager et al. (2003a) manipulated agency with the use of a lever, which could pull the participant's finger down as it depressed. In voluntary trials participants pressed a lever at a time of their choosing, causing a tone. In involuntary trials, the participant's finger was pulled with the lever, followed by a tone. In "other" trials participants observed another person performing the lever press. Finally, in machine trials a rubber hand was placed on the lever and pulled down with it, as the participant's finger would be in the passive conditions. Participants estimated the time of the lever press using a Libet clock. Results found significant effects of agency; later estimates of the time of the lever-press were found when participants performed or observed an intentional action, compared with movements induced by the movement of the lever.

Wohlschlager, Haggard, Gesierich and Prinz (2003) conducted three experiments with the same apparatus and found similar results, also with the use of a Libet clock. Interestingly, however, their third experiment investigated the perceived time of actions or mechanical events in the absence of a consequent tone. It found that mechanical lever-presses were perceived as occurring significantly earlier than self-generated or observed actions. This suggests that the findings of these experiments (Wohlschlager et al., 2003a; Wohlschlager et al., 2003b) may have been driven at least in part by baseline differences in the perceived times of each type of action, rather than by differences in the magnitude of temporal binding. It is not possible to determine the shifts in the perceived time of actions as two-event and single-event conditions were not included within the same experiments.

Additionally, the experiment designs used by Wohlshlager et al. (2003a; 2003b) confound causality with agency. In trials involving the self-pressing lever, the depression of the lever and the tone are controlled by a computer programme, and the depression of the lever does not cause the tone to occur. Here, the lever press and tone share a common cause, rather than the lever press causing the tone. This means that, across studies, machine and passive action conditions have a different causal mechanism, which would not be expected to result in temporal binding. It should be noted here that this raises a separate issue: in the absence of measures of perceived causality, it cannot be determined whether participants perceived the causal structure intended by the experimenters or the actual causal structure of the apparatus. Consequently, the extent to which causality and intentionality were confounded can also not be determined.

2.3.2.2. Temporal binding in observed actions

Although most temporal binding studies have focussed on the effect of self-generated actions on time and event perception, several studies have investigated temporal binding in observed actions. The first such studies, discussed above, were carried out by Wohlschlager et al. (2003a, 2003b). These studies suggest temporal binding can occur between observed actions and their consequences, as compared with mechanical action, but that this effect may be of a lesser magnitude than the effect of self-action. These studies investigated action binding alone, however, and single-event baseline measurements were collected in separate samples, rather than within subjects. Baseline measurements indeed found a bias toward an earlier awareness of the time of the mechanical event in the absence of a tone, which may have biased the main findings. These findings, therefore, while interesting, do not paint a clear picture of temporal binding in observed actions.

More recent evidence suggests a temporal binding effect in intentional observed actions relative to observed non-intentional actions. In a study by Moore, Teufel, Subramaniam, Davis and Fletcher (2013), participants were exposed to identical stimuli, regardless of condition. Participants were shown videos of a finger strapped to a button. On each trial the finger and button depressed, followed by a tone. Participants were led to believe either that the button was pulling the finger down as it depressed (as in Wohlschlager et al. 2003a & b), or that the individual in the film was pressing the button of their own accord. Participants estimated the interval between the keypress and tone in milliseconds. Temporal estimates were significantly lower when participants believed the movement was passive. These results, again, suggest temporal binding can occur between observed actions and their effects, compared with involuntary actions, even when the stimuli are identical. It should be noted, however, that as Wohlschlager et al.'s experiments, the causal mechanisms given to participants differ in structure. In the case of the button being pulled down mechanically, the movement of the button and the subsequent tone are controlled by the same computer programme (common cause), whereas in voluntary action the button causes the tone to sound.

Other studies have addressed the question of whether temporal binding is greater for self-generated, compared with observed actions and found that the temporal binding effect in observed actions, as measured using interval estimation tasks, is smaller than the temporal binding effect found in self-performed actions. In a study by Engbert et al. (2008) participants either performed voluntary actions (lever press), involuntary actions (participants' fingers were pulled down by the lever), observed human action or observed machine action (rubber hand pulled down by the lever). Temporal estimates (made in milliseconds) were significantly lower when participants performed voluntary actions, compared with involuntary or observed actions. These results were found for auditory (experiment 1), visual (experiment 2) and tactile outcomes (experiment 3). This was also suggested by the findings of Engbert et al. (2007), who found comparable temporal estimates between observed human actions and observed mechanical (rubber hand) actions and their consequences, using interval estimation tasks.

A similar pattern of findings – temporal binding in observed actions, but of a smaller magnitude compared with temporal binding in self-performed action - was reported by Strother, House and Obhi (2010) using the Libet clock method. Participants were tested in pairs and instructed to press the space key on a computer keyboard during each trial and allow their finger to depress with the key if the other participant performed the action first. This study single-event and keypress-tone trials. In two experiments, participants showed later awareness of the time of the keypress and an earlier awareness of the time of the tone when a keypress caused a tone, compared with single-event baseline trials. This effect was greater for self-action compared with observed action.

The differences in temporal binding between self-performed and observed actions were not replicated using interval reproduction procedures, however. In a study by Poonian and Cunnington (2013), self-actions and observed actions causing a tone were compared with non-causal, two-tone sequences. Participants' estimates of the delay between the first and second event were significantly higher for both self and observed actions, compared with two-tone sequence. This study did not find a significant difference in reproduced intervals between self and observed actions. However, there have not been other investigations of temporal binding in observed actions using interval reproduction tasks and it is therefore difficult to establish whether this finding reflects a difference in temporal binding between interval estimation and reproduction, or whether other factors contributed to this finding.

Overall, the majority of studies using both interval estimation and Libet clock tasks have found evidence of temporal binding in observed actions. This suggests that, if the perception of agency is required for temporal binding to occur, the effect results from the perception of any intentional action, including those not performed by the observer. By implication, the processes involved in predicting our actions are mirrored when observing actions performed by another agent. However, some of the studies discussed here have found evidence for a reduction in temporal binding in observed actions (Engbert et al., 2007 & 2008; Strother et al., 2010), while others found comparable temporal binding in self-generated and observed actions (Poonian & Cunnington, 2013). This is further complicated by evidence of similar temporal judgements between observed actions and mechanical actions (Engbert et al, 2007; 2008). A reduction in temporal binding in observed actions may reflect a difference in the processing of the perceived agency of one’s own actions and those performed by another. However, agency accounts cannot explain a comparable temporal binding effect between observed actions and mechanical actions, which would imply an absence of temporal binding in observed actions.

On the other hand, a causal account would predict temporal binding between self-generated, observed and mechanical actions and their consequences. It is possible that perception differences between self-generated actions and other observed events, such as differences in predictability and sensory cues (e.g. tactile and proprioceptive feedback are only available to the person performing the action) have affected the magnitude of the temporal binding effect. Studies of temporal binding in observed actions do not clearly support either causal or agency accounts of temporal binding. As in other experiments, causality and agency are often confounded when self-causal events are compared with non-intentional, non-causal controls (e.g. Moore et al., 2013). Similarly, in some studies non-intentional control conditions are nevertheless causal (e.g. Engbert et al., 2007), meaning a reduction in binding due to the absence of agency does not necessarily indicate a lack of temporal binding and may have resulted from other perceptual differences between the tasks. As these studies have been carried out in order to investigate the effects of perceived agency on temporal binding, the predictions of causal accounts have not been considered in experiment designs. As such, results can often be explained by both causal and agency accounts of temporal binding.

2.3.2.3. Evidence for intentionality as a necessary condition for temporal binding: beliefs of agency

Several studies have directly manipulated beliefs of agency rather than the nature of the movement itself, as in the studies discussed above (Caspar, Christensen, Cleermans & Haggard, 2016; Desantis et al., 2011; Dogge et al., 2012). These findings of these three studies have suggested that temporal binding can be moderated by explicit agency, i.e. beliefs of agency, providing evidence for agency accounts of temporal binding, although, as discussed below, the evidence is not conclusive at the time of writing. Although Temporal binding has been previously suggested to result from implicit (pre-conscious) feelings of agency (e.g. Haggard et al., 2003) there are several studies in which, where explicit and implicit agency were mis-matched, explicit agency appeared to take precedence in causing temporal binding.

Caspar, Christensen, Cleeremans & Haggard (2016) manipulated agency with the use of coercion and found evidence for a reduction of temporal binding due to a lack of perceived agency, despite the presence of intentional action. Pairs of participants were seated opposite each other. “Agents” were told they were able to increase their financial reward for the experiment by either decreasing the passive participant’s reward (group 1) or delivering a painful electric shock to the passive participant (group 2). Participants either freely chose whether to perform these actions or were instructed act by the experimenter. Participants’ actions also caused a tone after a random delay of 200, 500 or 800ms, Participants were asked to estimate the delay between their key press and the tone in milliseconds, between 0 and 1000. In both groups, participants made longer temporal estimates when their actions were coerced, compared with actions performed freely. This implies that temporal binding was reduced or absent in coerced actions compared with freely performed actions. In a second experiment, participants also completed questionnaires regarding their feelings of responsibility for their actions, which found significantly lower feelings of responsibility for coerced actions. These findings were similar to those found for the control conditions, in which participants either pressed the key to cause a tone, or had their finger pushed onto the key by the experimenter, suggesting that coercion had a similar effect on temporal binding to that of forced actions. Crucially, a reduction in temporal binding had occurred despite the presence of motor planning in coerced actions which, unlike forced actions, were actively performed by the participant and were not passive movements, such as when participants’ fingers are pulled down by the apparatus (e.g. Wohlshlager et al., 2003a; 2003b).

It can be argued, however, that under the causal account of temporal binding a lack of binding in coerced actions may have occurred due to a binding of the action towards the signal (instructions to act by the experimenter) and therefore away from the outcome. Evidence to date suggests a sensory event can be bound due to causality and intentionality (Yabe, Dave & Goodale, 2017; see Section 2.3.3.2. for more detail), although it is not currently known whether actions can similarly bound to their causes as well as their consequences. Further, neither experiment contained a non-causal control condition, nor was causal perception measured. It is therefore not possible to ascertain whether longer temporal estimates reflect the absence of temporal binding, or merely a reduction in the magnitude of the effect (as seen in observed intentional actions, see previous section). Causal judgements can therefore not be entirely ruled out in explaining these findings.

Nevertheless, Caspar et al.'s (2016) findings supported earlier findings by Desantis et al. (2011) who found both a lack of temporal binding in the absence of beliefs of agency, and evidence for agency accounts of temporal binding. Desantis et al. found, using the Libet clock method, a reduction in temporal binding when participants believed the consequences of their own actions to be caused by the actions of a confederate. Participants performed training sessions in which they either performed an intentional action themselves, or observed actions performed by a confederate, causing a tone, with the computer monitor showing the name of the participant whose action caused the tone. In the test session, participants and confederates performed intentional actions within a specified time window, with participants informed by the computer screen whether they or the confederate caused the tone, or given no instructions (the tones were, in fact, always caused by the participant). Compared with single-event baseline judgements, a shift of the perceived time of the tone toward the keypress was only found when participants believed that they had caused the tone or when no information was given, although agency ratings suggested participants believed they had caused the tone when no information was provided. No action binding was found in any condition, which the authors suggest may be due to the use of a social setting, as similar findings occurred in a social setting in a study by Strother et al. (2010). These findings cannot be as easily explained by the causal account of temporal binding; regardless of who participants believed had caused the tone, participants believed the two events to be causally related and participants were aware of when the confederate's action had been performed, as this had to have occurred near the time of their own keypresses. The lack of evidence for temporal binding relative to baseline suggests actions performed by others resulted in the absence of, or at least a reduction in temporal binding. However, this cannot be confirmed due to the absence of a non-causal control condition, as it is possible that a two-event sequence may have resulted in different shifts in the perceived times of the judged events compared with causal conditions.

While Caspar et al.'s (2016) and Desantis et al.'s studies found evidence for reduced temporal binding despite implicit agency, Dogge et al. (2012) found evidence for temporal binding in the absence of implicit agency altogether, again using a Libet clock. In a typical temporal binding experiment using a Libet clock, participants performed key presses, voluntary or passive (a key which pulled the participants' fingers down as it depressed, as used by Wohlschlager et al., 2003a & Wohlschlager et al., 2003b). A tone occurred after a short delay following the key press, and participants reported their judgement of either the time of the key press or tone, with single-event conditions used as baseline measures. Participants were either encouraged to think of their involuntary key presses as causing the tone to occur or were not given any such suggestion. A typical temporal binding effect was observed in the voluntary action condition. When self-causation did not occur, but was implied, a significant binding effect was observed. However, the outcome binding effect was smaller in magnitude than the one observed in actual self-causation, and no action binding was observed. Finally, in the absence of implied or actual self-causation, no action or outcome binding was found. These results again suggest that explicit agency, in the form of beliefs regarding self-causation, lead to temporal binding, even in the absence of motor planning. The absence of action binding in involuntary movements with implied self-causation may reflect the absence of predictive binding in this instance, as participants did not choose the time of their finger movement. These findings appear to suggest that temporal binding is at least partly caused by explicit beliefs regarding agency. However, agency and causality are again confounded in this design. The passive key presses, as in Wohlschlager et al.'s (2003a) and Wohlshlager et al.'s (2003b) studies, do not cause the tone, but rather share a common cause with it, although in the absence of causal judgements it is not possible to determine which causal structure participants believed was taking place.

The examples discussed here provide evidence for both a reduction in temporal binding, despite implicit agency, due to a lack of explicit agency (Caspar et al., 2016; Desantis et al., 2011) and temporal binding in the absence of implicit agency due to explicit agency (Dogge et al., 2012). It appears, therefore, that, although these findings seem to support agency accounts of temporal binding, implicit agency is not necessary for the effect to occur, as suggest by Haggard et al. (2002) and that implicit agency alone is not sufficient for the effect to occur. What is less clear is whether these findings had occurred due to explicit judgements of agency, i.e. self-causation, or causation alone. As with previous studies discussed in this chapter, both a lack of measurements of causal ratings and a lack of non-causal control conditions mean the causal account of temporal binding cannot be entirely ruled out as an alternative explanation for the findings.

2.3.2.4. Evidence for intentionality as a necessary condition for temporal binding: individual differences

Other support for intentionality as necessary for temporal binding to occur comes from studies of temporal binding and individual differences. It should be noted, however, that these studies do not directly manipulate the perception of agency alone and provide only indirect evidence for agency accounts of temporal binding. In these examples, individual temporal binding was investigated between groups, with participants grouped by traits known to be associated with changes in the perception of agency.

Hascalovitz and Obhi (2015) investigated whether temporal binding differed in magnitude between participants with low or high narcissism scores, as measured by the Narcissistic Personality Inventory (Raskin & Terry, 1988) and found greater temporal binding. Participants were grouped into low, medium and high narcissism score groups (less than 10, 11-17 and 21+ out of a possible 40, respectively). In each operant trial participants pressed a computer mouse key, which resulted in a tone after a short delay. Participants also performed single event baseline trials. In every trial participants reported the time of the key press or tone, using a Libet clock. Outcome binding was greater for the medium and high narcissism groups compared with the low narcissism group, with no significant differences in action binding. The authors attributed this to a tendency in those with higher narcissism scores to view themselves as powerful and dominant. Scores in the medium range, however, fell well within a normal range. This suggests that rather than greater temporal binding among those with high narcissism scores, the results may have indicated a reduction in temporal binding among those with lower-than-usual narcissism scores.

Haslovitz and Obhi (2015) noted depression as a potential confounding variable. Feelings of powerlessness are associated with depression (e.g. “depressive realism”, Alloy & Abramson, 1979) and their findings may have reflected a reduction in outcome binding due to depression, rather than an increase in outcome binding due to narcissism. This indicates that according to agency accounts temporal binding should be reduced during depressive states, which was indeed found by Obhi, Swiderski and Farquhar (2013). Here, depression was manipulated directly by asking participants to recall depressing experiences prior to the temporal binding task. Participants who recalled depressing, rather than neutral experiences reported longer intervals between their actions and their effects (tones) in an interval estimation task. A further experiment investigated the perceived delay between two tones and found that in the absence of intentionality/causality the delay intervals were judged as shorter in participants primed with depressive memories (other studies have also found a shortening effect of depression on perceived interval length, e.g. Gil & Droit-Volet, 2009).

This suggests that the results are unlikely to be explained by a general effect of depression on time perception, which would have resulted in shorter estimated intervals in participants primed with depressive memories compared with those primed with neutral memories. In both studies (Hascalobitz & Ohbi, 2015; Obhi et al., 2013) it is suggested that relatively reduced feelings of control over one's environment experienced in depressed moods and by those with lower narcissism scores are reflected in a reduction in temporal binding. As in previous examples, however, causality and agency may be confounded here. Differences in subjective feelings of powerfulness or powerlessness may also reflect differences in the participants' beliefs about their causal influence on the outside world. For instance, in the "depressive realism" effect, whereas non-depressed individuals tend to overestimate the contingency between their actions and events following their actions when there is no such contingency, depressed individuals make more accurate (lower) contingency judgements.

In a similar vein, Lush, Naish & Dienes (2017) manipulated the experience of agency with the use of hypnosis, under the assumption that participants performing actions due to hypnosis would perceive these actions to be involuntary. As predicted by agency accounts of temporal binding, it was found that a reduction in temporal binding occurred when highly hypnotisable individuals were performing key presses due to the experimenter's command, under hypnosis. Participants were grouped by hypnotisability scores (high and medium). All participants performed single-event baseline trials (key press/tone) and took part in three operant conditions: voluntary action, posthypnotic involuntariness (participants told their fingers will move involuntarily on trials in which they hear a hand clap), and passive action (the participant's finger was pushed down by the experimenter). Highly hypnotizable participants reported significantly higher involuntariness in the posthypnotic involuntariness condition than the medium hypnotisability group. The study found no significant differences in action binding. In outcome binding, the high hypnotisability group significantly lesser temporal binding in the post-hypnotic and passive action conditions, whereas the medium hypnotisability group only showed lesser outcome binding in the passive condition compared with the other two. Overall, the findings suggest that the action induced through hypnotic suggestion led to reduced temporal binding. Participants were not randomly allocated into groups, however. Participants in the two groups differed in their hypnotisability scores, which may be correlated with other traits. The high hypnotisability group, for instance, showed higher variability in their baseline action judgements.

2.3.2.5. **Evidence for intentionality as a necessary condition for temporal binding: *summary***

The examples outlined here are illustrative of several of the methods used to investigate the role of agency in temporal binding. The evidence from studies investigating self-causation typically supports agency accounts of temporal binding but is limited by the lack of accounting for perceived causality. Indeed, at this juncture, the majority of the literature on temporal binding links the effect with agency and the role of causality, therefore, remains relatively underexplored. The same can be said of studies of temporal binding in observed actions.

The omission of causality from experimental design primarily takes three forms. Firstly, causality is often confounded with agency in experiment designs. At times this is because non-intentional conditions are also non-causal, or because of differences in causal structure between conditions. Secondly, many experiments do not feature a non-intentional and non-causal baseline condition, such as a sequence of causally unrelated events. As such, it is often difficult to infer whether differences in subjective temporal judgements reflect a reduction in temporal binding, or the absence of the effect altogether. For instance, smaller interval judgements in intentional actions compared with non-intentional but causal actions does not necessarily confirm that temporal binding is absent in non-causal conditions, as such a difference may result from the nature of the task itself. Intentional actions typically involve motor planning, higher predictability and additional streams of sensory feedback (e.g. tactile and proprioceptive), which observed events do not. Finally, the vast majority of studies have not collected causal judgements from participants. As such, the way in which causality is perceived by participants is assumed but cannot be verified. This makes the interpretation of findings particularly difficult when the causal perception expected from participants differs from the actual causal mechanisms underlying the experimental apparatus (e.g. Wohlschlager et al., 2003a & b). In other words, the findings discussed in this section found that temporal binding occurs in intentional action-outcome sequences, but often do not contradict the predictions the causal account of temporal binding. It is worth noting that, as with the bulk of temporal binding research, the majority of the studies discussed in this section were carried out as investigations of agency and have, for the most part, not tested hypotheses based on the causal account of temporal binding.

2.3.3. The causal account of temporal binding

Although agency is often viewed as a necessary condition for temporal binding, some have proposed that causality alone may be sufficient for temporal binding to occur (e.g. Buehner & Humphreys, 2009; Eagleman & Holcombe, 2002). Whether causality is merely necessary or sufficient in causing temporal binding is the key distinguishing feature between the two accounts. These accounts differ further, however, in the proposed mechanisms behind temporal binding.

Eagleman and Holcombe (2002) proposed that Haggard et al.'s (2002) findings may be attributable to causal perception alone. They suggested that as time perception contains a certain amount of measurement "noise" it is plausible from a Bayesian perspective that perceptual processes may shift the perceived time of events based on prior knowledge. As discussed previously, temporally contiguous events are more likely to elicit inferences of a causal relationship between the events (See Chapter 1, Section 1.1). Eagleman and Holcombe suggested that this results in a reversal of this assumption; as causality is inferred from temporal contiguity, temporal contiguity can be inferred from causality. That is, observers assume that the temporal delay between a cause and its consequence is likely to be shorter than the temporal delay between causally unconnected events and adjust the perceived time of the events and the delay between them accordingly. Buehner and

Humphreys (2009) further proposed that temporal binding should be termed “causal binding”.

The causal binding account, therefore, parts ways with intentional binding accounts not only in the attribution of the effect solely to perceived causality, but in the function of temporal binding. Haggard et al. (2002) suggested that temporal binding strengthens the association between intentional actions (by the actor or another agent) and their effects. This approach suggests that this temporal illusion is in some way adaptive. In contrast, the causal binding account suggests it is the product of a top-down influence of prior beliefs on time perception, specifically the belief that causally related events are usually temporally contiguous. Temporal binding could therefore be regarded as a temporal illusion resulting from a typically adaptive process, rather than being adaptive in itself. The causal binding account may be viewed as more parsimonious, as it proposes fewer preconditions for temporal binding. All accounts, however, rely on theoretical assumptions regarding the nature of how time and causality or intentionality are perceived.

2.3.3.1. Evidence for causal accounts of temporal binding

To date, relatively few studies have directly compared the influence of perceived causality and agency on temporal binding directly, with the majority of experiments designed under the assumption that perceived agency is a necessary condition for temporal binding. As such, as discussed previously, the majority of studies on the effect of agency on temporal binding lack non-causal control conditions or utilise designs in which causality and agency are confounded. As such, many past findings can be viewed as supporting both causal and agency accounts, although that perceived causality is a necessary for temporal binding to occur remains uncontroversial (see Section 2.3.1)**.** This section discusses findings which support the causal binding account, either through direct evidence for causal binding or challenges to agency accounts.

Few studies to date have attempted to investigate the predictions of the causal model of temporal binding directly. One such study, which has found evidence for temporal binding in the absence of perceived agency was carried out by Buehner (2012). Two experiments investigated temporal shifts in the perceived time of a visual flash between conditions in which the flash is caused by the participant's button press (self-causal), a button press made by a machine (machine-causal), or is preceded by another flash of light which does not cause the second flash (control). Participants performed a key synchronisation task, in which they were asked to predict the time of the flash (or second flash in the control condition) by pressing a button. The results of two experiments found an earlier anticipation of the flash when it was caused by a button press, either self- or machine-caused, compared with the control condition. The findings of experiment 1 suggested that agency led to a larger perceptual shift at the fastest interval (500ms), but not at the other two. Experiment 2 did not replicate this, however. The findings of both experiments suggest temporal binding can occur in the absence of agency.

A more recent again found evidence for the predictions of the causal account of temporal binding. When comparing voluntary and involuntary actions (Buehner, 2015) evidence of temporal binding was found when participants' movements were involuntary. In voluntary trials participants pressed a key, which caused a tone after a 250ms delay. In involuntary trials participants' fingers were pushed into the key by a machine. In non-causal trials two tones were played in sequence. Single-event baseline trials were also conducted. Participants estimated the times of key presses and tones in a Libet clock task. Temporal binding was found in both causal conditions compared with the two-tone condition, but the effect was of a greater magnitude when the keypress was voluntary. These findings were in line with those of Buehner (2012; experiment 1). The evidence for temporal binding being greater for self-action remains mixed, both in the findings reported by Buehner (2012; 2015), as well as studies comparing observed action with self-action (e.g. Engbert et al., 2007; Engbert et al., 2008; Wohlschlager et al., 2003a & b; see Chapter 2, Section 2.3.2.2). At the time of writing, this effect remains relatively under-studied. It is worth noting that an "intentional boost" to temporal binding (Buehner, 2012) was hypothesised by Eagleman and Holcombe (2002), who suggested such an effect may be due to a greater certainty in causal beliefs when participants are able to interact with the stimuli, rather than observe them, although to date this has not been directly investigated.

2.3.3.2. Evidence of temporal binding of signals to actions

Agency accounts of temporal binding define temporal binding as occurring between actions and their sensory consequences. This is explicitly the case in Haggard et al.'s (2002) original theoretical account of the effect, as the effect was proposed to take place due to internal predictions of the sensory outcome of an action. Other agency accounts, while expanding on or differing from the forward model explanation, have not proposed temporal binding to take place between any other event pairs. In contrast, the causal account of temporal binding proposes that temporal binding may take place between any pairs of events, provided the first is believed by the observer to have caused the second.

A series of experiments by Yabe and Goodale (2015) investigated temporal binding between signals and reactions and found results which were difficult to explain under agency accounts of temporal binding, while fitting with the predictions of the causal account. Yabe and Goodale (2015) found evidence for a forward shift in the perceived time of signals when they caused a participant's movement, i.e. a temporal binding of a signal to the action it caused. In these experiments participants viewed a fast-rotating clock. During each trial, the clock would change colour and a rectangle would appear at the opposite side of the screen (experiments 1). Participants were either instructed to make a saccadic eye movement toward the rectangle when the signal appeared or continue to fixate on the clock. After each trial participants reported the position of the clock hand at the time of the signal (clock colour change). Results showed a shift of the perceived time of the signal toward the time of the eye movement, compared with control trials. In a second experiment participants performed a go-no-go task in which the colour of the signal determined whether an eye movement should be made. The same shift was found on both go and no-go trials, compared with the control condition, suggesting temporal binding between the signal and the intention to act, rather than the eye movement itself. Finally, experiments 3a and 3b replicated the findings of experiment 2 with finger movements; either a key release action (experiment 3a) or a finger movement recorded by a light sensor (experiment 3b).

Yabe, et al. (2017) expanded on this by investigating three-event chains in addition to the causal structures used in previous experiments (Yabe & Goodale, 2015). Here the signals and effects of actions were tones. Participants took part in conditions in which their actions caused an auditory event (AE), the action was performed in response to the event (EA) and single-event baseline conditions (E). In experiment 1 they also took part in event-action-event (EAE) sequences, and action-event-action (AEA) sequences in experiment 2. In all conditions participants reported the times of tones using a Libet clock. Both experiments found shifts in the perceived times of tones toward the action, both when the tone was the cause of the action (EA) and when the tone was the consequence of the action (AE). Interestingly, when the tone was both caused by and resulted in an action (AEA trials), no shift was found, suggesting the event was perceptually “pulled” in both directions.

These findings present a challenge to agency accounts of temporal binding. Yabe and Goodale (2015) suggested temporal binding may aid our understanding of causal relations between our actions and external events. This account places more emphasis on the role of causality, albeit still in the context of intentional actions. In contrast, the causal binding account predicts these findings, as well as a temporal binding of actions to the signals which caused them (although to date this has not been investigated). The findings of Yabe and Goodale (2015) and Yabe et al (2017) further suggest that differences in temporal binding between voluntary and involuntary actions may result from differences in the causal structure of trials present in experiment designs. It may be that the perceived times of involuntary actions are shifted toward the events which caused them, leading to what appears like lack of action binding. However, this cannot necessarily explain differences in outcome binding. As the time of the cause determines the time of the effect, one might expect a commensurate shift in the perceived time of the tone toward the perceived time of its cause. More research is required to understand how more complex causal chains may influence temporal binding, and whether actions are perceptually bound to the signals which caused them, as would be predicted by the causal account of temporal binding. Nevertheless, these findings are predicted by the causal account of temporal binding, while difficult to explain under agency accounts of temporal binding.

2.3.3.3. Evidence for causal binding from studies of individual differences

As discussed previously (Section 2.3.2.4.), some studies of temporal binding in individual differences have found results consistent with the predictions of agency accounts of temporal binding (although they might also be explainable by the causal account). Other studies, however, have found results which were inconsistent with those predictions and which were in fact more consistent with the predictions of causal accounts.

A key such challenge to agency accounts of temporal binding comes from differences in temporal binding between schizophrenia patients and matched controls (Haggard et al, 2003; Voss et al, 2010). Haggard et al (2003) carried out a similar temporal binding task to that reported by Haggard et al (2002) on schizophrenia patients and matched controls. Schizophrenia patients were of interest due to the abnormal experiences of agency such as delusions of influence present in many patients and have suggested to be linked to inaccuracies in motor predictions (see Frith, 2012, for a review). Contrary to the predictions of agency accounts, patients showed a greater temporal binding effect than controls, both in terms of absolute shifts in comparison with a non-intentional and non-causal control.

Voss et al (2010) expanded on these findings by investigating the roles of prediction and retrospective inference and found that patients showed a greater effect of retrospective inference, again using a Libet clock task. The likelihood of the tone occurring was manipulated between conditions (high likelihood = 75%, low likelihood = 50%). Patients showed action binding relative to baseline in all conditions, and greater action binding on trials in which the tone occurred compared to those in which no tone occurred. In controls, on the other hand, action binding relative to baseline measures was only found when the likelihood of the tone was high, on both tone and no-tone trials. Action binding in schizophrenia patients was not affected, therefore, by the probability of the outcome. This suggests a reduced predictive component and increased post-dictive component of temporal binding in participants with schizophrenia. The authors explained the overall difference in the magnitude of temporal binding between schizophrenia patients and healthy controls in terms of disruption to motor prediction processes in patients. The authors suggest that while schizophrenia patients often misattribute their own actions to external sources, here they showed an over-attribution of external events to their own actions, indicating a mismatch between implicit and explicit agency. It can be argued, however, that this may be due to aberrant probabilistic reasoning in schizophrenia (e.g. Garety, Hemsley & Wessely, 1991; Huq, Garety & Hemsley, 1988); patients exhibited greater action binding in the absence of a tone, which may alternatively suggest that patients failed to distinguish between the different levels of probability.

These findings suggest the tendency to attribute one's own actions to external sources did not seem to impair temporal binding, and in fact may have increased its magnitude. Under agency accounts of temporal binding this requires the assumption that explicit and implicit agency were at odds in this case, and therefore that temporal binding results from implicit, rather than explicit processes contributing to the perception of agency. However, as discussed in Section 2.3.2.3., other studies have found evidence for an effect of explicit beliefs regarding agency and causality on temporal binding (e.g. Desantis et al., 2011; Dogge et al., 2012) rather than implicit agency. Alternatively, the causal binding account would suggest that differences in the magnitude of temporal binding may reflect a higher certainty in causal relationships, regardless of whether these are associated with megalomania or delusions of influence. For instance, the “jumping to conclusions” bias, the bias towards making probabilistic judgements with less statistical information, has been found to occur significantly more often in schizophrenia patients than healthy controls (Moritz & Woodward, 2005), and in the general population among those who exhibit more paranoid ideation (Freeman, Pugh & Garety, 2008).

Schizophrenia studies are limited due to a lack of random assignment; schizophrenia patients differ from healthy control in factors other than feelings of agency, for instance. Other studies have made use of ketamine to induce psychosis-like symptoms in order to further investigate the findings discussed above and found greater temporal binding in participants who were administered ketamine, compared with the placebo group (Moore et al, 2011; Moore et al, 2013). Again, as it is often used as a drug model of psychosis, ketamine is suggested by the authors to have a similar effect on the sense of agency. Interestingly, Moore et al. (2013), using a libet clock task, found a greater predictive component in action binding in the ketamine group as well as greater action binding overall, unlike the findings of previous studies with schizophrenia patients. Although it should be noted that ketamine is not a perfect drug model of psychosis, these findings show two examples in which mental states associated with reduced explicit agency had not led to a decrease in temporal binding, and the under- or over-reliance on prediction cannot explain both sets of findings.

The interpretation of studies of temporal binding in individual differences may lead to a number of potential pitfalls. Firstly, it should be noted that the experience of agency is not the only psychological difference between groups. Secondly, there is the possibility of unfalsifiable predictions: since both reduced and increased temporal binding in schizophrenia patients and participants who were administered ketamine can be explained by agency accounts of temporal binding. Thirdly, there is a danger of circular reasoning. Findings which appear to show a positive relationship between agency and temporal binding can be seen both as evidence for agency accounts of temporal binding, and for the role of agency in certain disorders or personality traits. Nevertheless, the findings discussed in this section are inconsistent with the view that a decreased sense of agency necessarily results in temporal binding, as these examples show conditions associated with a reduced sense of agency resulting in an increase in temporal binding. In contrast, the causal account of temporal binding can account for the findings in studies of schizophrenia and temporal binding.

2.3.3.4. Causal accounts of temporal binding: summary

In summary, the scope of agency accounts of temporal binding has greatly expanded since they were first attributed to forward models (Haggard et al, 2002). After sixteen years of research they must account for the necessity of a causal relationship between actions and outcomes in temporal binding, temporal binding in the observed actions of others, the lack of a repulsion effect in between involuntary actions and their effects and a possible dissociation between explicit agency and temporal binding in some cases, such as in schizophrenia. Recent findings have further challenged agency accounts in demonstrating a temporal binding of signals to their resultant actions, and evidence for temporal binding in the absence of agency.

In contrast, the causal binding account has been relatively under-researched. While there is evidence that agency accounts may fail to account for some findings, evidence against other accounts of temporal binding does not necessarily constitute evidence for causal binding. At this time, some studies have found direct evidence for temporal binding in the absence of agency, which cannot be easily explained by agency accounts of temporal binding. It remains unclear why some studies have found a further “boost” to temporal binding due to agency, for instance whether this is due to differences in sensory feedback, increased certainty of causal relationships, or perhaps additive effects of the perception of causality and agency on time perception. Further investigations of the predictions made by causal and agency accounts of temporal binding are needed to determine whether either can better explain the effect.

2.4. Temporal binding in phenomenal causality

Although causal inference has a central role in understanding temporal binding (see Chapter 2, Section 2.3), little attention has been paid to the role of phenomenal causality. Consequently, the question of whether the effects of causality on time perception are limited to inferred causality or are applicable to phenomenal causality remains unanswered. Further, phenomenal causality being at least as sensitive to temporal cues as inferred causality, if not more so (see Chapter 1, Section 1.3), it may be expected to exert a an even greater influence on time perception. This, in addition to the large body of research on stimuli which lead to visual causal impressions makes stimuli eliciting visual impressions of causality a potentially powerful tool in investigating the effects of causal perception on time perception.

The existence of temporal binding in phenomenal causality would be consistent with the causal account of temporal binding. As phenomenal causality is affected by similar temporal cues to those affecting inferred causality, a reversal of the assumption that temporally contiguous events are more likely to be causally related may lead to changes in time and event perception. Two recent studies provide indirect evidence for the possibility of causal binding in phenomenal causality. While not investigating temporal binding directly, these have demonstrated reversals of other causal cues: spatial contiguity and temporal priority (Buehner & Humphreys, 2010; Bechlivanidis & Lagnado, 2016). Such effects are consistent with causal binding accounts, which predict top-down effects of causal perception might apply to other Humean assumptions; for instance, Eagleman and Holcombe (2002) speculated that causally related events may be perceived as closer together in space.

This has been confirmed by Buehner and Humphreys (2010). Two studies compared launching animations and delayed launches and found a contraction in the perceived spatial distance between the two launching objects at the point of collision, in conditions in which participants reported stronger visual impressions of causality. All animations contained a spatial gap filled with a grey bar. On some trials, participants reported causal ratings. On others, participants were asked to estimate the length of the grey bar seen in the animation after viewing the animation. On these trials a similar grey bar with the same height but different width was presented to participants after the animation had ended. Participants estimated the length of the bar by extending it to the same length as the bar seen in the animation. Causal ratings were significantly higher for immediate launches and, as predicted, the reported size of the spatial gap was significantly smaller. A second experiment replicated these findings with the addition of two more animation types: priority violation (the "launched" object moved before the "launcher") and upward launch, whereby the "launched" object moved vertically rather than horizontally. Again, estimates of the spatial gap were consistent with causal ratings, with more apparently causal animations eliciting smaller reported gaps.

In addition to Buehner and Humphreys' (2010) findings suggesting a reversal of the assumption of spatial contiguity between causally related events, Bechlivanidis and Lagnado (2016) reported a reversal of the perceived order of events in launching animations. In experiment 1, two groups of participants were shown one of two clips. One group was shown three objects: A (left), B (centre) and C (right). In each animation object A moved right and stopped upon reaching object B. Object C then moved right, followed by object B, which stopped at the original location of object C. The second group was presented a similar clip, but with the absence of object A. Here, object C moved first, followed by object B. Participants were then asked to report the order of events. Additionally, causal impressions were reported using a slider. The majority of participants who had observed the three-object animation reported the "causal" order of events (A, followed by B, followed by C) rather than the actual order, whereas the majority of participants in condition 2 correctly identified object C as moving before object B. Likewise, participants who had seen the three-event animation were significantly more likely to perceive object B as causing the movement of object C compared with those who had seen the two-object animation. In a second experiment the two-object animation was replaced with a three-object animation in which object B remained stationary while objects A and C moved in the same manner as in the other three-event animation. Instead of answering questions about the temporal order seen in the animation, participants were shown two comparison animations, one of which was identical to the one seen previously and the other was a “domino effect” in which object A moved first, followed by the movement of object B, followed lastly by the movement of object C. Surprisingly, the majority of participants shown a three-object animation in which all objects moved chose the incorrect comparison animation, with the reverse found for group 2. These findings suggest participants perceived a temporal order consistent with the causal mechanism they were expecting. I.e., if the three objects moved right in sequence and stopped when reaching the next object, participants assumed this must have taken the form of a sequence of collisions.

The above findings are line with the predictions of the causal account of temporal binding. However, direct investigations of temporal binding in phenomenal causality are scarce. At the time of writing, only one experiment has directly investigated temporal binding in phenomenal causality. Cravo, Claessen and Baldo’s (2009) findings suggested evidence for temporal binding when both visual causal impressions and intentional actions were present. This study made use of launching stimuli similar to those used by Michotte (1963/46; a schematic diagram of these stimuli can be seen in Figure 2.1.). Each trial began with two discs present on the screen and the borders of the stimuli were marked with vertical white lines on the right and left sides of the display. Each trial contained one of two animation types. The “collision” animations were based on Michotte’s launching effect stimuli and were intended to appear more causal. At the beginning of each collision trial, one object was shown adjacent to the left border of the display and the other in the centre. The leftmost object then moved toward the central object and stopped upon contacting the central object. Following a delay of either 200 or 300ms, the central object moved rightward, at the same velocity as the first object to move, until reaching the right border. In “non-collision” animations, intended as a non-causal control condition, both objects were presented alongside one another at the centre of the screen at the beginning of each trial. The object to the left moved first, toward the left border of the display. After the first object had reached the border and stopped, the second object moved to the right following a delay of 200ms or 300ms, as in collision animations. The experiment made use of a fully factorial design, with the two animation types presented in either “active” or “passive” blocks. In passive blocks participants viewed the animations. In active blocks participants controlled the first object to move using computer mouse buttons, whereby holding down the left mouse button caused the object to move to the left and holding down the right mouse button caused it to move to the right. Causal and temporal ratings were collected in separate blocks, for each condition. In causal blocks (10 trials per condition) participants were asked to report the extent to which they perceived the first moving object as causing the second object to move on a scale of 0-10. In temporal blocks participants were asked to estimate the delay duration in milliseconds, between 0 and 1000ms. Significantly higher causal ratings were found for collision animations than for non-collision animations and in short intervals compared with long intervals. However, contrary to the predictions of the causal account of temporal binding, significantly shorter temporal estimates were only found in active blocks, with similar temporal estimates in all passive trials, and non-causal, active blocks.

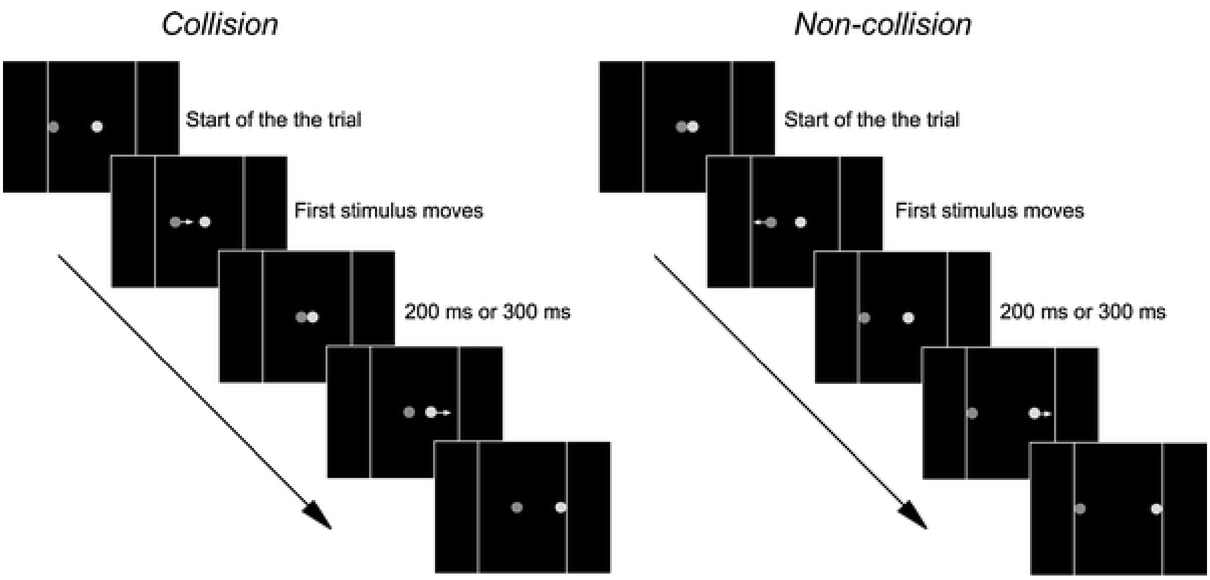

Figure 2.1. A schematic diagram of the stimuli used by Cravo et al. (2009) in the collision and non-collision conditions. Figure reproduced with permission.

These findings have since been reported as evidence for perceived causality being necessary, but not sufficient for temporal binding to occur (e.g. Moore & Obhi, 2012). There are, however, several limitations to this interpretation of the evidence. Most importantly, it does not take into consideration the distinction between inferred and phenomenal causality. On each active trial participants had caused all subsequent visual events by moving the left object to its stopping point. This was evident to participants; the left object only moved when participants held down the mouse key and the remainder of each animation played when the object controlled by participants reached its stopping point. The visual causal impressions elicited by the animations, on the other hand, were stronger for collision compared with non-collisions. As such, visual causal impressions and inferred causality were at times at odds, with only active-collision trials both appearing causal and containing an actual causal relationship between the participant's actions and the visual events that followed. Furthermore, only two animation types were used in the study and the findings cannot necessarily be applied to all cases of phenomenal causality or generalised to inferred causality.

Returning to the common interpretation of Cravo et al.'s findings, it appears that the mere presence of perceived causality is often assumed to be sufficient for temporal binding to take place and causality's role in temporal binding is restricted either to a binary, present or absent status, or a single continuous variable. However, in 75% of the conditions used by Cravo et al. some form a perceived causal relationship was present: phenomenal causality in all collision trials, inferred causality in all active trials, and both in active-collision trials. It is unknown how phenomenal and inferred causality may interact, if at all, under either agency or causal accounts of temporal binding.

2.5. The aims and scope of the thesis

While the findings discussed above suggest the possibility of temporal binding in phenomenal causality, it remains uncertain due to the dearth of research on the subject and the absence of any published replication attempts of Cravo et al.'s (2009) study. It must be considered that phenomenal and inferred causality result from different processes (Schlottman & Shanks, 1992) and cannot necessarily be said to have the same influence on time perception. This thesis aims, therefore, to further investigate temporal binding in phenomenal causality; whether it exists, how the effects of visual causal impressions on time perception may differ from those of inferred causality, and whether this supports the causal account of temporal binding.

Several hypotheses may be considered on the basis of previous research. If temporal binding results from the relationship between time perception causality in general, temporal binding should occur due to phenomenal causality. However, a lack of temporal binding in phenomenal causality may suggest that temporal binding results specifically from the cognitive processes leading to causal inferences and not visual impressions of causality. It is less clear why, under agency accounts, phenomenal causality would contribute to temporal binding. Temporal binding has been suggested to contribute to the attribution of outcomes to one’s actions (Haggard et al., 2002), with similar processes taking place when the actions of others are observed (Wohlschlager et al., 2003a). The prevailing assumption is that temporal binding results from motor prediction processes underestimating delays between actions and their sensory outcomes, provided that these outcomes are predicted by the action. To explain Cravo et al.’s (2009) findings, this requires the assumption that the illusion of causality is sufficient for such motor predictions to take place. Furthermore, visual impressions of events as non-causally related would result in a lack of temporal binding, even when the stimuli giving rise to these impressions are caused by the observer’s actions. Whether temporal binding occurs in phenomenal causality is of theoretical importance for both causal and agency accounts of temporal binding. This raises the question of why more investigations of temporal binding in phenomenal causality had not been carried out.

The majority of phenomenal causality stimuli that exist to date present a difficulty in their application to temporal binding research. Namely, the apparent cause and effect sequence is typically immediate (e.g. collision-launch), with a very short or entirely absent temporal gap between the two. Temporal delays diminish causal impression to such an extent that they may be used as non-causal control stimuli (e.g. Buehner & Humphreys, 2010). Launching animations, while extensively studied and effective in producing strong causal impressions, are highly sensitive to delays. The threshold at which delays cause stimuli to appear non-causal varies depending on context, such as the range of delay intervals presented and the types of animations presented (see Chapter 1, Section 1.3 for more detail). Consequently, variations in design, such as the types of animations used and the order of presentation, may lead to unintended causal or non-causal impressions. Indeed, despite the relative difference in causal ratings between animation types, Cravo et al. (2009) reported mean causal ratings only marginally higher than the mid-point of the measurement scale in "causal" animations, with delays of 300ms. Further, the less distinct the causal impressions between animation types are, the greater the possibility that some participants may not perceive one condition as more causal than the other, affecting the findings. An optimal, visually causal stimulus would therefore maintain causal impressions in the presence of temporal delays, irrespective of the duration of the delay.

The effects of temporal delays have been found to be overcome with the use of cues to generative transmission (White, 2015). Generative transmission refers to the transference of causal influence across a temporal and/or spatial gap. For instance, Shultz (1982) found that children as young as two showed a preference for generative transmission cues over temporal or spatial contiguity. One such example is the effect of a blower blowing out a lit candle at a distance (Shultz, 1982, experiment 1); while the blower is not spatially contiguous with the candle it is understood that air provides a medium by which the rotation of the fan blades can extinguish the flame at a distance. White (2015) used an abstract, visual representation of such a medium to retain causal impressions in launching animations containing both a temporal and spatial gap (see Figure 2.2 for an example of these stimuli). In experiment 1 the gap was filled with an array of rectangles which changed colour in sequence from the direction of the launcher object to the direction of the target (launched) object. Here, causal impressions were stronger compared with stimuli in which the gap objects did not change colour, or an empty gap. Interestingly, longer delays did reduce causal impressions, although to a lesser extent than in the absence of the colour change sequence. Experiment 2 used a reversed colour change sequence - from the direction of the target to the direction of the launcher – as a control condition and found similar results; the colour change sequence only led to stronger causal impressions when it occurred in the same direction as the movement of the launching objects. This demonstrates that the increase in causal ratings is not due to the presence of a colour change sequence alone, but that it must take place in the direction of the causal sequence.

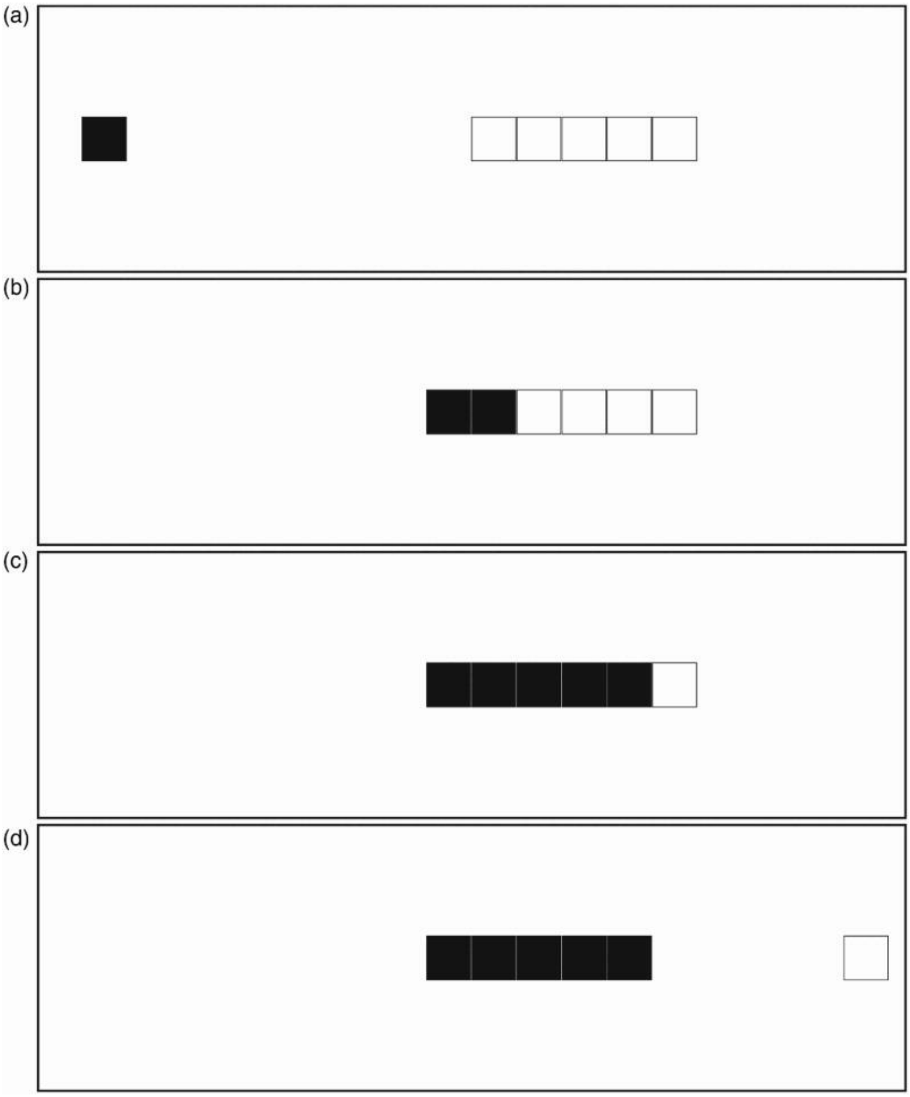

Figure 2.2. A schematic diagram of stimuli used by White (2015) to cause visual impressions of generative transmission. A moving object was seen moving rightward from the left side of the screen (a). Upon “collision” with the leftmost gap object, the gap objects began to change colour in sequence, from left to right (b & c). Finally, the rightmost object began to move rightward after all gap objects had changed colour. Figure reproduced with permission.

Further, the findings of four additional experiments suggests this is not merely the result of a “tool effect”, whereby the colour change sequence implies the movement of an intermediate object. Experiment 3 showed that causal ratings were higher as the number of gap objects increased, resulting in a more continuous cue to generative transmission. Experiments 4a, b and c used different gap events and found similar results. When the gap objects “jumped” up rather than changing colour (experiment 4a), shrank (experiment 4b) or disappeared (experiment 4c) in sequence, high causal ratings were observed despite the presence of spatial and temporal gaps. These findings cannot be explained by participants perceiving the gap sequences as a third, intermediary object. It remains the most credible explanation that the increase in causal ratings is due to the perception of the transmission of causal force from the launcher to the target, through the medium represented by the gap objects.

Such stimuli allow us to overcome the drawbacks of delayed launch stimuli, such as those used by Cravo et al. (2009). High causal ratings can be retained over much longer time duration between the stopping of the launcher and “launch” of the target object than those usually tolerated, with less susceptibility to context effects. Reversed gap sequences provide a useful non-causal control; the stimuli are visually similar and contain a similar temporal sequence of events. For these reasons, the experiments described in this thesis made use of stimuli with cues to generative transmission to investigate temporal binding in phenomenal causality, with the aim of shedding more light on the role of causality in temporal binding.

3. Chapter 3: Investigations of Temporal Binding in Phenomenal Causality Stimuli Using Interval Estimation and Interval Reproduction Methods

As discussed previously, previous temporal binding research has largely overlooked phenomenal causality (with the exception of Cravo et al., 2009), with inferred causality and phenomenal causality often conflated in discussions of causality in the temporal binding literature. While the causal account of temporal binding makes clear predictions regarding temporal binding in phenomenal causality, the interpretation of such an effect requires additional assumptions in agency models. Consequently, investigation began with investigations of the predictions of the causal account of temporal binding. Temporal binding in phenomenal causality, in the absence of agency would suggest both evidence for the causal account and for an effect of phenomenal causality on time perception. A lack of such an effect may indicate that phenomenal causality does not affect time perception in the same manner as inferred causality. Experiments 1-3 were designed to test for an effect of perceived causality on perceived delay intervals. Experiments 4-6 tested alternative explanations for the findings, aside from those predicted by the causal account of temporal binding. The slope analyses presented in Section 3.10 test for a direct relationship between causal impressions and perceived interval durations across experiments 1-6.

3.1. Stimuli

The experiments discussed in this chapter employed several different animations adapted from those used previously by White (2015; discussed in Chapter 2, Section 2.5). In experiments 1-5 experiments the animations were presented inside a visual aperture placed in the centre of the screen, with a grey background. All animations contained two moving objects: the launcher (black) on the left side of the screen and the target (white) on the right side of the screen. Both moved from left to right. The launcher was always the first to move, followed by the movement of the target after a delay (delay intervals are detailed in the methods), with the two never coming directly in contact with one another. The animation types differed mainly in the contents of the spatial and temporal gap between the stopping of the launcher and the launch of the target. In all but one the gap was filled with a series of eight rectangles (gap objects) which were spatially separated. Below are descriptions of the gap sequence types used in experiments 1-6.

The “forward” gap sequence type can be seen in Figure 3.1 (below). Here, the gap objects changed colour from grey to black in sequence, from the direction of the launcher to that of the target. This sequence began at the point at which the launcher made contact with the leftmost gap object, with the launch of the target taking place at the end of the sequence. Previous research (White, 2015) indicates that this acts as a visual cue to generative transmission, and observers are likely to perceive the sequence of events as a causal launch whereby the launcher brings about the movement of the target. As such, this animation type is used here as a baseline “causal” stimulus.

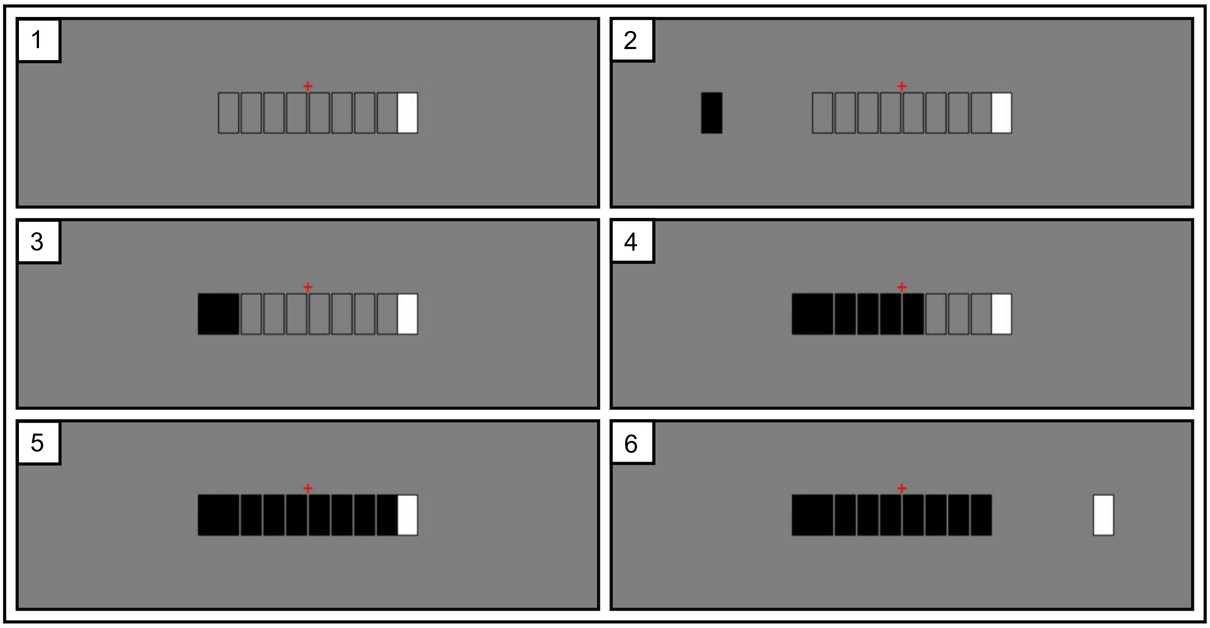

Figure 3.1. Screenshots (cropped) of the forward gap sequence type (experiments 1-6). The launcher is offscreen at the beginning of the trial (image 1) and moves toward the gap objects (image 2). Upon collision the gap sequence begins (image 3) until all the gap objects had changed colour (image 5), at which point the target begins to move to the right.

In the “backward” gap sequence type (Figure 3.2, below) a similar colour change sequence takes place in the reverse direction, from right to left, i.e. from the direction of the target to the direction of the launcher. The movement of the launching objects and the timing of the colour changes remained identical. This gap sequence was used as a baseline “non-causal” animation, as past research indicates that such a sequence results in significantly weaker causal impressions than the forward gap sequence (White, 2015).

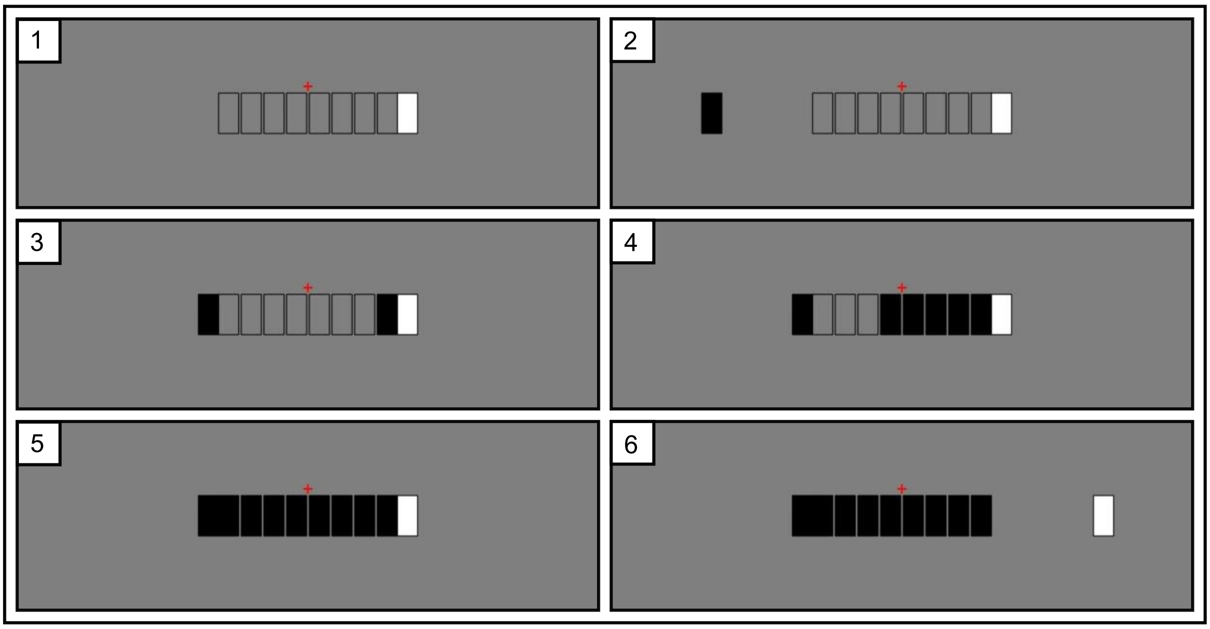

Figure 3.2. Screenshots (cropped) of the backward gap sequence type (experiments 1-6). The gap sequence (images 3-5) took place in the reverse direction to that seen in forward gap sequence animations.

“Empty gap” animations (Figure 3.3) were used to test the suitability of the backward gap sequence type as a non-causal control. Here, the gap objects are absent and the visual and spatial gap between the launching objects is empty. Several studies (discussed in Chapter 1, Section 1.3) have established that visual causal impressions are significantly lessened by the presence of visual and spatial gaps between launching objects, with lower causal impressions the larger the gaps (e.g. Michotte, 1946/63; Yela, 1952).

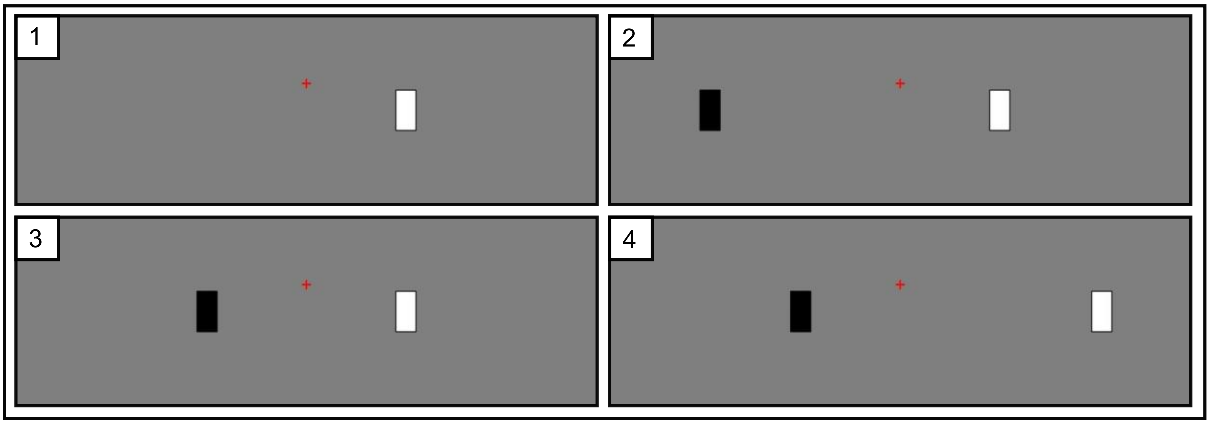

Figure 3.3. Screenshots (cropped) of the empty gap sequence type (experiment 1). The launching objects moved in the same way as in other animations but were separated by an empty gap.

The “offset” gap sequences (Figure 3.4) were the same as the forward gap sequences, with the launching objects vertically offset from the gap objects. It was hypothesised that these will lead to lower causal impressions than forward gap sequences, but higher than the backward gap sequences. This was based on previous findings showing that causal impressions elicited by launching animations decrease as the spatial gap between the launching objects increases (e.g. Michotte, 1946/63), but impressions of “launching at a distance” are possible, depending on the size of the spatial gap. Here, it was hypothesised that, in a similar fashion, the spatial separation between the gap stimuli and the launching objects would serve to lessen causal impressions compared with the other forward gap sequence type, while maintaining higher causal impressions than the backward gap sequence, which implies no generative transmission. This gap sequence type is useful in adding a further degree of causal impression. While differences in the perceived length of delays between the forward and backward and gap sequence animations might be accounted for by other perceptual differences, a consistent effect of causal impressions on perceived delays across three animation types would be less likely to result from the visual differences between these animations.

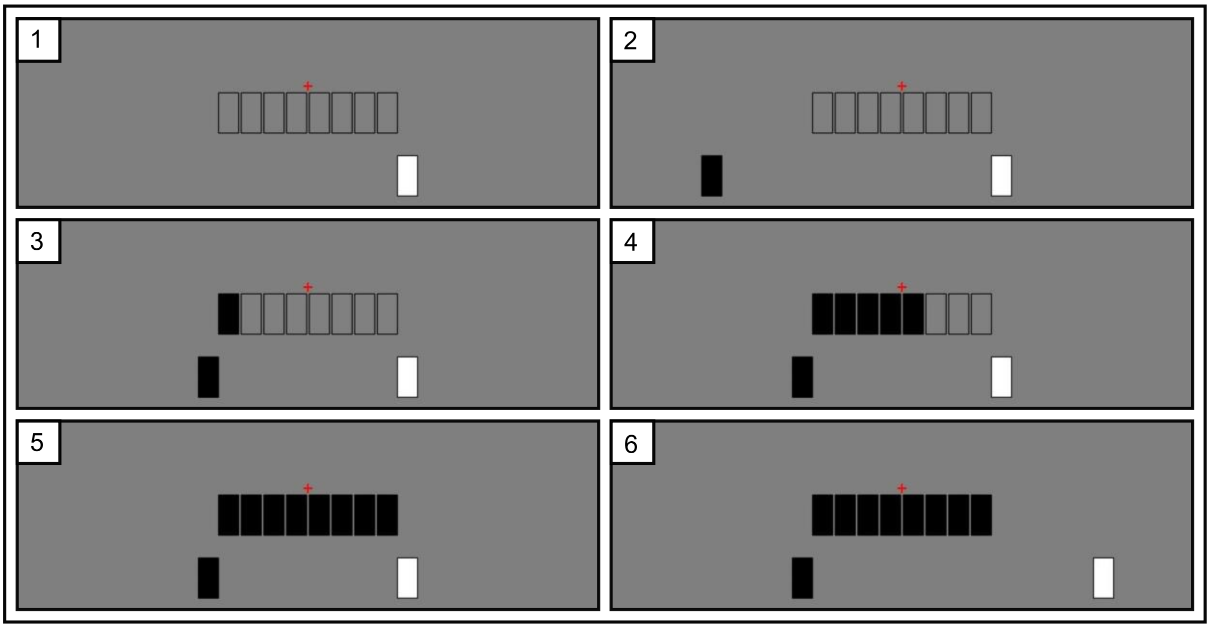

Figure 3.4. Screenshots (cropped) of the offset gap sequence type (experiments 2 and 3). The gap sequence and movement of the launching objects are the same as in the forward gap sequence type, but the launching objects are vertically offset from the gap objects

Two “continuous” gap sequences were used in experiment 1: continuous colour change, and continuous colour change with covariation cues (Figures 3.5 and 3.6, respectively). In continuous colour change animations, the colour change sequence took place continuously, from grey to black and then from black to grey during the entire length of the animation. The launcher only became visible when all eight gap objects had changed colour. The movement of the launching object was controlled such that the gap objects were all grey at the moment of collision, followed by a grey-to-black colour change sequence. As such, these animations were identical to the forward gap sequence animations between the stopping of the launcher and movement of the target, but with the colour change sequence occurring before the collision and continuing after the launch. This animation was used to test whether participants were relying on inference, rather than visual impressions, when reporting causal ratings. Here, covariation between the “collision” of the launcher with the gap stimuli and the onset of the gap sequence is low: the gap sequence is seen to take place both prior to and following the collision event. Similarly, not all colour change sequences result in the launch of the target. If participants infer causal judgements from covariation cues - rather than visual impressions of generative transmission, as suggested by White (2015) - causal ratings would be expected to be lower as a result.

The “continuous gap sequences with covariation” animations were used as a control for continuous gap sequences. Here, a continuous colour change sequence took place, but was altered following the collision. At the beginning of the trial this was a grey-to-white sequence which changed to a grey-to-black sequence on collision. As such, these animations were very similar to the continuous gap sequence, but the nature of the sequence was altered by the collision and a launch only occurred following a grey-to-black sequence, rather than a grey-to-white sequence.

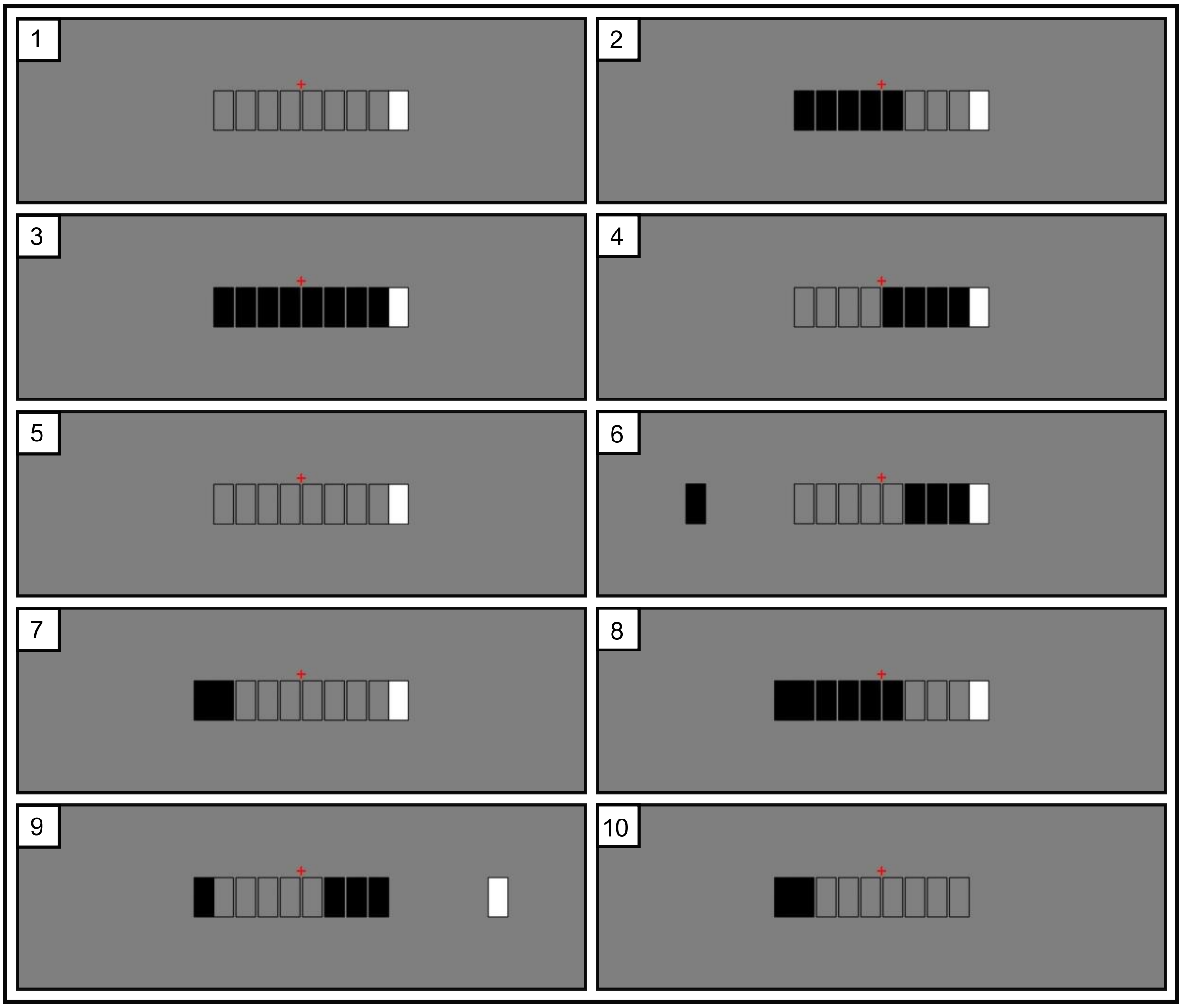

Figure 3.5. Screenshots (cropped) of the continuous gap sequence type (experiment 1). A forward grey-to-black gap sequence (images 1-3) took place at the beginning of each animation, followed by a black-to-grey sequence. This took place twice before collision (image 7). The colour change sequences continuous after the launch (images 9 and 10).

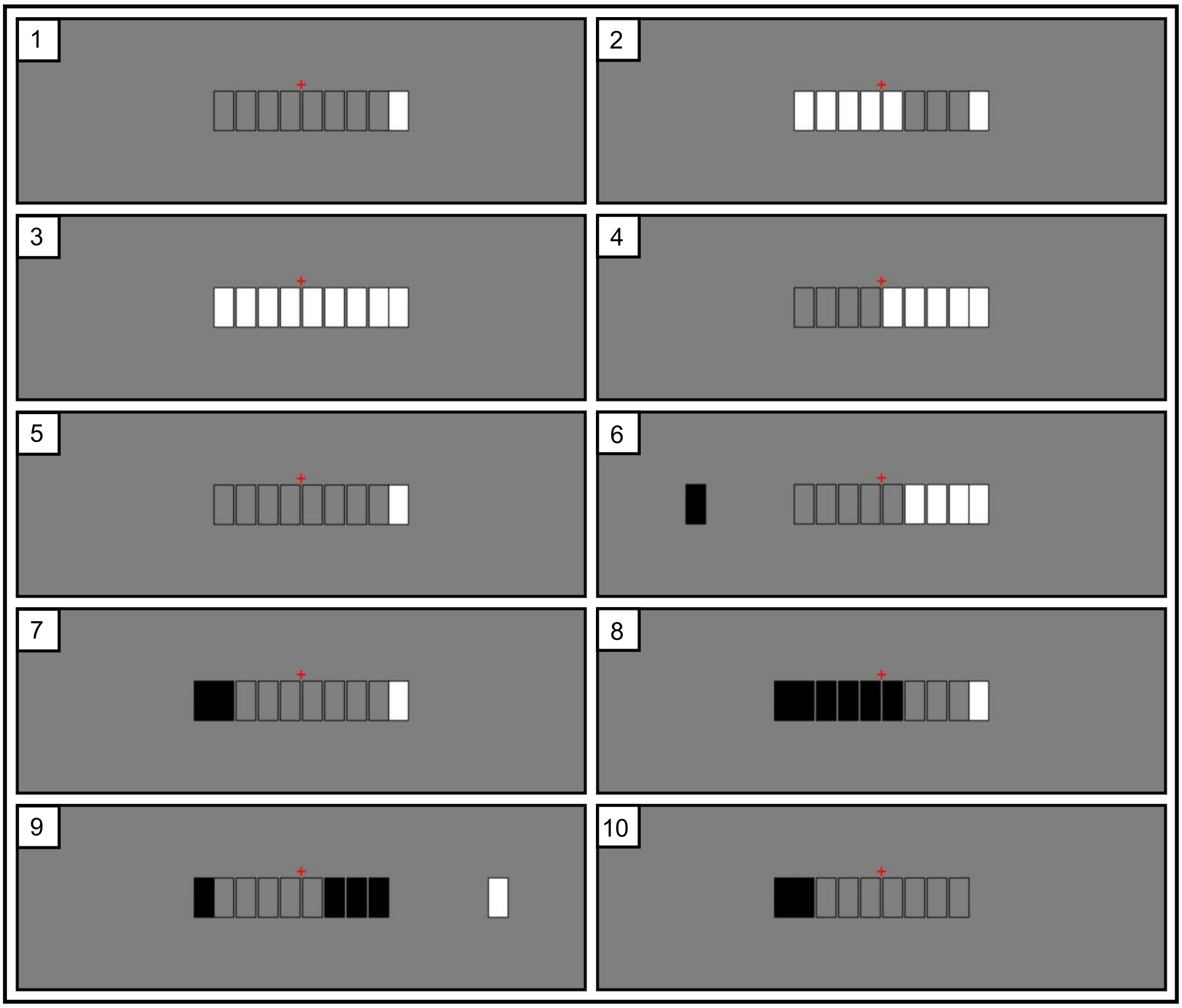

Figure 3.6. Screenshots (cropped) of continuous gap sequences with covariation (experiment 1). These were similar to the continuous gap sequence, with the exception that colour change sequences took place from grey to white and from white to grey prior to the collision.

3.2. Measures of perceived intervals

Measures of the perceived interval length were used, as they are most appropriate for use with the stimuli described above. As discussed in Chapter 2, Libet clocks have been the most commonly measure in studying temporal binding. However, as participants were required to attend to visual stimuli in these experiments, it would have required their attention to be split between the rotating clock hand and the launching and gap objects. Direct interval estimation and interval reproduction have both been used successfully in the past to replicate the temporal binding effect and do not require participants to attend to additional visual stimuli (e.g. Humphreys & Buehner, 2009; Moore et al., 2009). In direct interval estimation tasks participants report their perceived duration of time intervals between two events in milliseconds (0-1000). In interval reproduction tasks the participant holds down a key for the perceived duration of each interval.

Both measures were used in these experiments: direct interval estimation in experiments 1 and 2, and interval reproduction in experiment 3 onward. Direct interval estimation has the advantage of having been more extensively used, and therefore more established, in temporal binding research and in time perception research in general. The task is less natural to participants, however, most of whom would not have much experience of measuring time intervals in milliseconds. This method may create a risk of participants responding in a stereotyped way, for instance by rounding temporal judgements. There is further a risk of anchoring effects (Tversky & Kahneman, 1974) whereby estimates are made relative to the previous estimate made, rather than being independent, absolute estimates. Direct interval estimates further necessitate the use of an arbitrary upper threshold to the rating scale to avoid participants providing largely outlying estimates due to a misunderstanding of the scale. This means participants’ responses are capped, affecting the range and distribution of each participant’s responses. Lastly, the use of a numeric scale may create a greater risk of demand characteristics as participants are able to accurately match temporal ratings to causal judgements, depending on what they believe the intention of the experimenter to be. Interval reproduction tasks overcome these issues to an extent. Although anchoring and demand characteristics may still affect findings, participants cannot be as deliberate in adjusting their estimates due to these influences, as they do not have access to objective feedback of their estimate as they are when the estimate is typed. Further, participants can make use of the full resolution of the scale as it does not require them to explicitly measure their estimates in milliseconds. In addition, no arbitrary upper threshold is needed. The drawback of using interval reproduction tasks is that they have not been used as often in temporal binding research. Nevertheless, both tasks have been shown to be useful in studying temporal binding. Here, both were used in order to establish that any effects, or lack of effects, found are replicable across different measures of time perception.

3.3. Experiment 1

3.3.1. Methods

3.3.1.1. Participants

31 Cardiff University students and staff (4 male, age range 18-52, one not reported) were recruited using Cardiff University's Experiment Management System. Participants took part in exchange for a payment of £3. All participants had normal or corrected-to-normal vision.

3.3.1.2. Apparatus and materials

The experiment was run using an i-Mac 27" computer, running Apple Mac OS X 10.9.4 (Mavericks). Stimuli were presented on the monitor, at a resolution of 2,560 by 1,440 pixels (59.5 by 33.5cm) and refresh rate of 60Hz. Participants responses were recorded using a computer keyboard. The experiment was run using PsychoPy (Peirce et al., 2019).

All instructions were presented on the screen with a text height of 20 pixels (0.46cm) and wrap width of 800 pixels (18.59cm). The experiment made use of five animation types: the forward, backward, empty, continuous and continuous-with-covariation gap sequences described above. In all animations a red fixation cross (15 by 15 pixels, 0.35 by 0.35cm) was visible 44 pixels (1.02cm) above the centre of the screen. The gap objects and launching objects were rectangular, 32 pixels wide by 64 pixels high (0.74 by 1.49cm). Gap objects were separated by a 4 pixel (0.09cm) gaps. The animations were presented inside a visual aperture 926 pixels wide and 600 pixels tall (25.12cm by 13.95cm), which was placed at the centre of the screen and surrounded by a black border.

All animations were presented at fast, medium and slow speeds, and with corresponding short, medium and long delays between the stopping of the launcher object and “launch” of the target object. At all animation speeds the rate of the colour change sequence was proportionate to the speed of the launching objects, such that the gap sequences were of the same duration as the duration taken for the launching objects to travel the width of the row of the spatial gap. The colour change sequence occurred at the same speed as the movement of the launching objects, therefore. At fast speeds, the launching objects travelled 18 pixels (0.42cm) per frame, and the gap sequence took place at a rate of one colour change every 2 frames (33.33 milliseconds). At medium speeds the launching objects travelled at 12 pixels (0.28cm) per frame and the gap sequence took place at one colour change every 3 frames (50 milliseconds). Lastly, at slow speeds the launching objects travelled at 9 pixels (0.21cm) per frame and the gap sequence took place at a rate of one colour change every 4 frames (66.67 milliseconds). The target object launched after the same period of time as a single gap object colour change, such that the temporal delays between the stopping of the launcher and movement of the target were 266.67, 400.00 or 533.33 milliseconds at fast, medium and slow speeds, respectively.

To account for the number of colour change sequences required to take place before the collision in continuous gap sequence animations, the launcher entered the aperture after 800, 1,200 and 1,600ms at fast, medium and slow speeds, respectively. In all other animations, the same timing was retained to ensure that the length of each trial did not vary within speed conditions. For the same reason, after the beginning of the “launch”, each trial ended after twice the duration of the temporal delay between collision and launch.

3.3.1.3. Design and procedure

The study used a two-factor design, with the five gap sequence types and three animation speeds described in the materials section. All gap sequence types were presented at all animation speeds, with a total of 15 unique animations.

For each of the 15 animations, participants completed 5 causal and 5 temporal trials. In causal trials, participants were asked to report their impression of whether the launcher object caused the target object to move on a scale of 0-100 (0 = “definitely no”, 100 = “definitely yes”). Participants were prompted with the following instructions: “did you have the impression that the black rectangle brought about the motion of the white rectangle?”. In temporal trials participants were asked to report their perceived length of the temporal delay between the stopping of the launcher object and the movement of the target object (referred to as the black and white objects) in milliseconds, between 0 and 1,000. Participants were reminded that there are 1,000 milliseconds in a second prior to the temporal trials.

Participants completed causal and temporal trials in separate blocks. Within each block the order of presentation of the 75 trials, including all combinations of speed and gap sequence type, was randomised. The presentation order of the two blocks was counterbalanced, with each participant randomly assigned to carry out either the causal or temporal block first. Each block contained five practice trials, during which data was not collected. In these, the five gap sequence types were presented at randomly selected speeds, such that each gap sequence type was presented once.

Participants were tested individually. Upon arrival, all participants were presented with an electronic consent form. After consent was granted, participants were presented with instructions prior to beginning the experiment, and before the practice and experimental blocks. Participants were instructed about the nature of the task and the animations which would be presented (full instructions can be seen in Appendix A). For causal trials, the instructions explained the task and only mentioned that the animations may or may not appear causal to the participant, without any implications regarding which animation types are designed to give a stronger causal impression. Similar instructions were provided for the temporal block, explaining the nature of the task. In both blocks of trials participants were informed that they may alert the experimenter at any time if they have questions regarding the task. At the end of the experiment, participants were debriefed and given their payment.

3.3.2. Results

3.3.2.1. Exclusions

One participant's data was lost due to a technical error and is not included in the analysis. In two cases, a technical fault allowed participants to enter temporal estimates over 1,000 milliseconds. These trials were excluded from analysis (one trial for each of the two participants).

3.3.2.2. Causal ratings

For each condition, the mean causal rating was computed per participant. Overall, participants reported the highest mean causal impressions for the forward gap sequences (81.19, SD = 14.34). Lower mean causal impressions were reported for the continuous + covariation gap sequences (72.79, SD = 17.96) and continuous gap sequences (69.85, SD = 18.49). The lowest mean causal ratings were reported for the backward (35.73, SD = 25.40) and empty gap sequences (31.91, SD = 25.60).

Causal ratings varied to a lesser degree between animation speeds; the highest mean causal ratings were found at fast speeds (61.79, SD = 14.76), followed by the medium (57.40, SD = 14.50) and slow speeds (55.70, SD = 17.03). See Figure 3.7 for a summary of these findings.

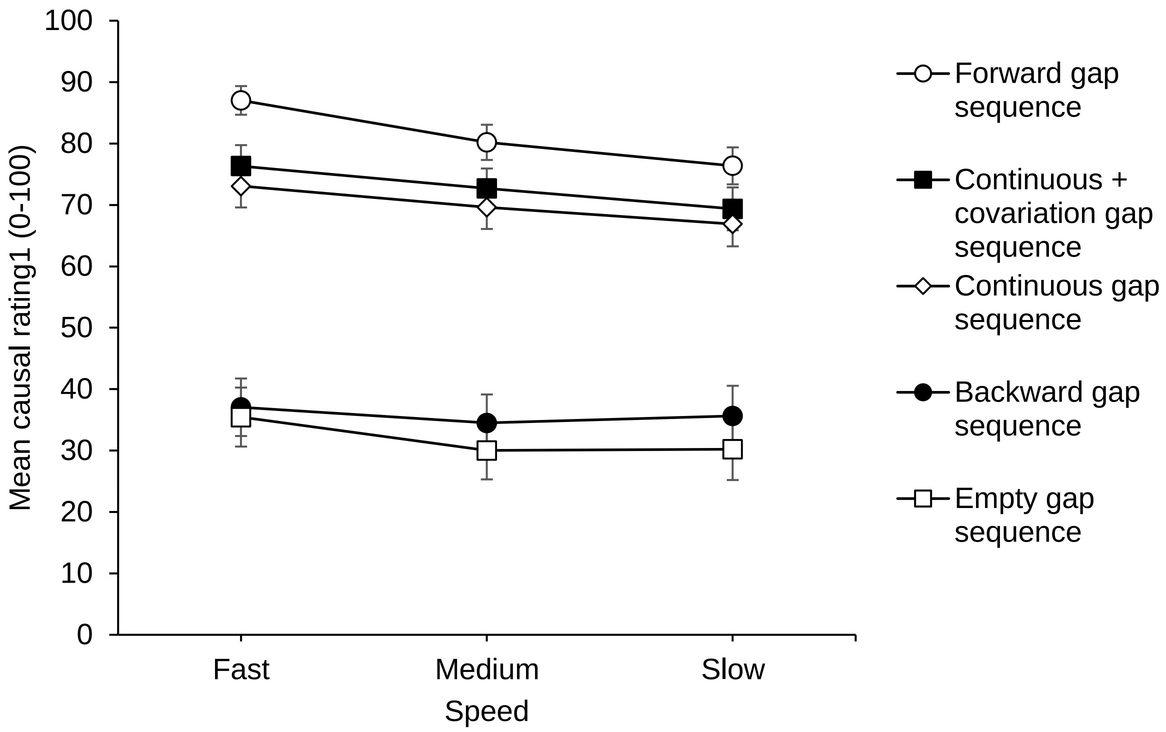

Figure 3.7. Mean causal rating by gap sequence type and animation speed. Error bars represent 95% confidence intervals.

Each participant's mean causal ratings were analysed using a two-way, repeated-measures ANOVA. The assumption of sphericity, as tested using Mauchly's test of sphericity, was violated in all comparisons; the main effect of gap sequence type ($\chi^2(9)$ = 77.53, p < .001, ε = .61), animation speed ($\chi^2(2)$ = 20.41, p < .001, ε = .66) and the interaction between the two ($\chi^2(35)$ = 75.09, p < .001, ε = .57). As such, the degrees of freedom reported below have been corrected using the Greenhouse-Geisser correction.

The analysis found significant main effects of gap sequence type (F(2.42, 70.29) = 57.25, p < .001, η_p^2 = .66) and animation speed (F(1.32, 38.22) = 8.78, p = .003, η_p^2 = .23). The interaction was not statistically significant (F(4.56, 132.22) = 1.76, p = .13, η_p^2 = .06).

Planned contrasts were carried out on the two main effects. Repeated contrasts were used, with the gap sequence types tested in the expected order from most to least causal: forward, continuous + covariation, continuous, backward and empty gap. Based on previous research, both the backward and empty gap sequences were expected to appear least causal, as they contained no cues to generative transmission; it remained uncertain, however whether a difference would be observed between the two. Likewise, both of the continuous colour change gap sequence types were novel and it was not known whether they would elicit different causal impressions. However, there was no theoretical reason to expect they would lead to greater perceived causality than the forward gap sequence types, while both may be expected to lead to lower causal ratings if they were affected by contingency.

These comparisons found significant differences in causal ratings between the forward and continuous + covariation gap sequences ($F(1, 29) = 8.71$, $p = .006$, $\eta_p^2 = .23$), between the continuous + covariation and the continuous gap sequences ($F(1, 29) = 11.48$, $p = .002$, $\eta_p^2 = .28$) and between the continuous and backward gap sequences ($F(1, 29) = 56.347$, $p < .001$, $\eta_p^2 = .66$). There was no significant difference in causal ratings between the backward and empty gap sequences ($F(1, 29) = .70$, $p = .41$, $\eta_p^2 = .02$).

Similarly, repeated contrasts were carried out on the speed conditions, with the fast and medium, and medium and slow conditions being compared, due to previous findings showing that causal ratings decrease as the temporal gap between the stopping of the launcher and movement of the target increases (see Chapter 1, Section 1.3). A significant difference in mean causal ratings was found between the fast and medium speeds ($F(1, 29) = 17.33$, $p < .001$, $\eta_p^2 = .37$), but not between the medium and slow speeds ($F(1, 29) = 1.60$, $p = .22$, $\eta_p^2 = .05$).

3.3.2.3. Interval estimate errors

Each participant’s mean temporal estimate was calculated for each condition. To produce the estimate errors, the real gap intervals (266.67, 400 and 533.33ms in the fast, medium and slow conditions, respectively) were subtracted from each mean estimate.

Overall, the mean estimate errors were lowest for the forward gap sequences (2.58ms, SD = 145.09ms), followed by the continuous + covariation gap sequences

(15.87ms, SD = 157.77ms), the continuous gap sequences (16.76, SD = 153.24ms), the backward gap sequences (43.56, SD = 154.69ms) and the empty gap sequences (135.71ms, SD = 157.91ms). Notably, the estimate errors were much higher for the empty gap sequences than all other gap sequence types.

The estimate errors suggest a tendency to overestimate at all animation speeds, with mean estimate errors of 36.44ms at fast speeds (SD = 110.76ms), 49.65ms at medium speeds (SD = 141.35ms) and 42.60ms at slow speeds (SD = 176.12). A summary of the temporal estimate errors can be seen in Figure 3.8.

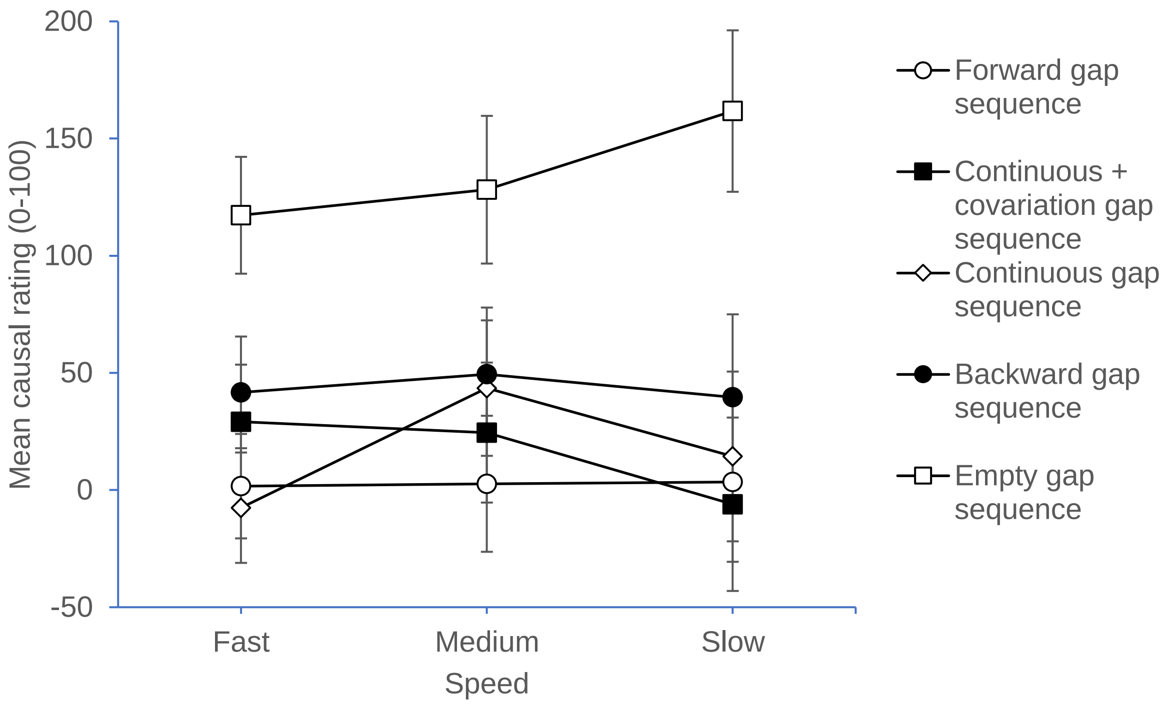

Figure 3.8. Mean estimate errors by gap sequence type and animation speed. Error bars represent 95% confidence intervals.

The data were analysed using a two-way, repeated-measures ANOVA. The assumption of sphericity, as tested using Mauchly's test of sphericity, was violated for the main effects of gap sequence type ($\chi^2(9) = 72.90$, $p < .001$, $\varepsilon = .42$) and animation speed ($\chi^2(2) = 30.57$, $p < .001$, $\varepsilon = .60$). The assumption of sphericity was not violated for the interaction term ($\chi^2(35) = 35.19$, $p = .47$). The degrees of freedom reported below have been corrected with the Greenhouse-Geisser correction where appropriate.

The analysis found significant main effects of gap sequence type ($F(1.67, 48.49) = 14.79$, $p < .001$, $\eta_p^2 = .34$) and a significant interaction between gap sequence type and animation speed ($F(8, 232) = 2.41$, $p = .02$, $\eta_p^2 = .08$). There was no significant main effect of animation speed ($F(1.2, 34.85) = .41$, $p = .57$, $\eta_p^2 = .01$).

As causal binding would predict estimate errors to be the converse of causal ratings, i.e. higher causal ratings would be expected to lead to lower temporal estimate errors, the same planned contrasts were used here as for the causal ratings. Planned comparisons were not carried out on the different speeds, as the main effect was not found to be statistically significant. Of the pairwise comparisons carried out on the gap sequence types, a significant effect was only found when comparing the backward and empty gap sequence types ($F(1, 29) = 15.79$, $p < .001$, $\eta_p^2 = .35$). No significant effects were found when comparing the forward and continuous gap sequence with covariation ($F(1, 29) = 1.99$, $p = .17$, $\eta_p^2 = .06$), the two continuous gap sequence conditions ($F(1, 29) = .01$, $p = .93$, $\eta_p^2 < .001$), or the continuous gap sequence and backward gap sequence ($F(1, 29) = 2.99$, $p = .10$, $\eta_p^2 = .09$). As no interaction between animation type and speed was found in the causal ratings analysis, any interactions found here were not of theoretical interest as they do not present clear evidence for or against temporal binding (see Appendix B for an exploration of the interaction between gap sequence type and animation speed).

3.3.3. Discussion

Causal rating corroborated previous findings. Participants made a clear distinction in causal ratings between forward gap sequences and backward or empty gap sequences. Significant differences in causal ratings between the forward and continuous + covariation, and the continuous + covariation and continuous gap sequences, suggested that covariation may have played a role in causal judgements. However, these differences were much smaller in magnitude and effect size than the differences between backward gap sequences and continuous gap sequences, suggesting that covariation cannot account for the bulk of the difference in causal ratings between the forward and backward gap sequence types.

Additionally, the covariation between the colour change sequence and launch in the backward gap sequence type did not appear to increase causal ratings compared with the empty gap sequence animations. Overall, the forward and backward gap sequence types succeeded in eliciting distinct causal (forward) and non-causal (backward) impressions, while being visually similar. The similarity of causal ratings between the backward and empty gap sequence types suggests the backward gap sequence type acts as an effective non-causal control condition as it does not moderate the effect of the temporal and spatial gap on causal ratings.

Analyses of temporal estimate errors did not find evidence for temporal binding in phenomenal causality. The main effect of gap sequence type on estimate errors appears to be driven by a substantial over-estimation of empty gap intervals compared with other animation types, while there was no evidence for any other significant differences in estimate errors. This is surprising given that past research has shown that time intervals tend to be overestimated when they are filled with stimuli, including visual stimuli (the “filled duration illusion”, e.g. Buffardi, 1971). However, the empty gap sequence animations differ from others in predictability. When the gaps were empty, participants did not have as reliable a cue to the point in time at which the launcher will stop. Similarly, the colour change sequences, regardless of direction, allowed participants to anticipate the time of the launch regardless of the animation speed. The effect of the empty gap sequences on interval estimate errors appears, therefore, to be driven by factors other than perceived causality. Predictability may be one such factor, although it is not possible to determine whether this was the case based on the findings of experiment 1.

One possibility is that the lack of significant differences in temporal estimate errors is due to a lack of statistical power. However, the sample size used here (30 participants) is larger than many repeated-measures temporal binding studies, and smaller samples have been used to replicate the effect in the past. The number of trials per condition was relatively small, however, which may have increased the variance of mean temporal estimates.

Experiments 2 and 3 retained the forward and backward gap sequence types, as these were effective in yielding distinct high and low causal impressions, respectively. In Experiment 2 the measures of interval perceptual and causal judgements were also retained but increased to 10 trials per condition. Experiment 3 made use of an interval reproduction task in which participants were asked to hold down a key for the duration of the gap interval in order to test whether the same results are replicated across different measures of interval judgements. The offset gap sequence type (see Figure 3.4 in Section 3.1) was added as an intermediary level of perceived causality, between the extreme high and low ratings found for forward and backward gap sequences. The aim of using three levels of causal impressions rather than two was to prevent an effect of animation type from being inconclusive. Due to perceptual differences between the forward and backward gap sequence animations it would not possible to entirely discount factors unrelated to causality as contributing to any effects found. Such factors are, however, less likely to explain a pattern of findings consistent with causal binding at three levels of perceived causality and containing different perceptual differences.

3.4. Experiment 2

3.4.1. Methods

3.4.1.1. Participants

31 Cardiff University students (3 male, age range 18-24) were recruited using Cardiff University's Experiment Management System. Participants took part in exchange for course credit. Participants who took part in experiment 1 were not permitted to sign up for this study. All participants had normal or corrected-to-normal vision.

3.4.1.2. Apparatus and materials

All visual stimuli were presented on a computer monitor, with a computer keyboard used for participant input. The computer monitor was 47cm wide by 30cm tall. Stimuli were presented at a resolution of 1,680 by 1,050 pixels, at a refresh rate of 60 frames per second.

The experiment programme was coded and run in PsychoPy (Peirce et al., 2019). All instructions were presented on the monitor.

The experiment made use of the forward, offset and backward gap sequence animations. The stimuli dimensions, set in pixels, were retained from experiment 1. Due to the difference in monitor size and resolution, the absolute sizes of the stimuli differed from experiment 1, with the relative sizes of visual objects remaining the same. The fixation cross was 0.42cm by 0.42cm in size and positioned 1.23cm above the centre of the screen. The launching and gap objects were 0.90cm wide and 1.79cm tall, with gap objects separated by 0.11cm gaps. In offset gap sequence animations the launching objects were presented 100 pixels (2.80cm) below the vertical centre of the screen.

The timings of each event remained unchanged from experiment 1. The absolute velocity of the launching object changed along with object sizes, such that the launching objects moved at 0.50 cm per frame at fast speeds, 0.36cm at medium speeds and 0.25cm at slow speeds. The durations of the gap intervals remained at 266.67ms, 400.00ms or 533.33ms in fast, medium and slow animations.

3.4.1.3. Design and procedure

The experiment used a factorial design, with three gap sequence types (forward, offset and backward) and three animation speeds (fast, medium and slow). Altogether there were 9 different animation types. See the materials section for more detail on the gap sequence types and speeds.

Two outcomes measures were collected in separate tasks. After viewing each animation participants were asked to report either causal ratings or temporal estimates. The causal judgement task and interval estimation task used in experiment 1, including the instructions, were used in experiment 2, with the number of trials per condition increased to 10 (90 causal judgement trials and 90 interval estimation trials in total).

Causal and temporal trials were presented in separate blocks, with a counterbalanced order. Each block contained 10 trials per condition (90 in total), preceded by a practice block containing one trial of each gap sequence type, at a randomly selected speed. Data was not recorded during practice trials. Each block was preceded by a set of instructions (see Appendix A for full instructions). The two measures were tested in separate counterbalanced blocks. Each participant was randomly assigned to one of the two block orders. All animation types at each speed were presented in a randomised order within each block.

Up to three participants at a time were tested in separate rooms. Upon arrival participants were presented with an electronic consent form. After granting consent, participants were presented with electronic instructions before each block of trials and informed that they may ask the experimenter for help if they do not understand the task. Participants were debriefed at the end of the experiment.

3.4.2. Results

3.4.2.1. Exclusions

Two exclusions were made in total, based on predetermined criteria. One participant's data was removed from analysis due to a consistent tendency to report longer intervals as shorter intervals. To determine this, each participant's mean temporal estimates were calculated across the three animation speeds and animation types, and participants who did not report shorter intervals at fast speeds than at medium speeds, and shorter intervals at medium than slow speeds across all animation types, were removed from the analysis. It should be noted that this exclusion criterion was deliberately conservative. A second participant was removed from the analysis due to outlying data (over three standard deviations from the sample mean in one condition and over 2.5 in three others) and a mean estimate of 56.89ms (equivalent to 3.41 frames).

3.4.2.2. Causal ratings

Each participant’s mean causal rating was calculated per condition for inclusion in the analysis. As expected, participants reported the highest mean causal ratings for the forward gap sequences (71.30, SD = 18.52), followed by the offset gap sequences (49.72, SD = 23.51) and the backward gap sequences (24.93, SD = 22.67). Overall, participants reported higher mean causal ratings for faster animation, with the highest ratings for the fast animations (55.60, SD = 19.10), followed by the medium (47.12, SD = 18.37) and slow speeds (43.33, SD = 17.68). A summary of this data can be seen in Figure 3.9.

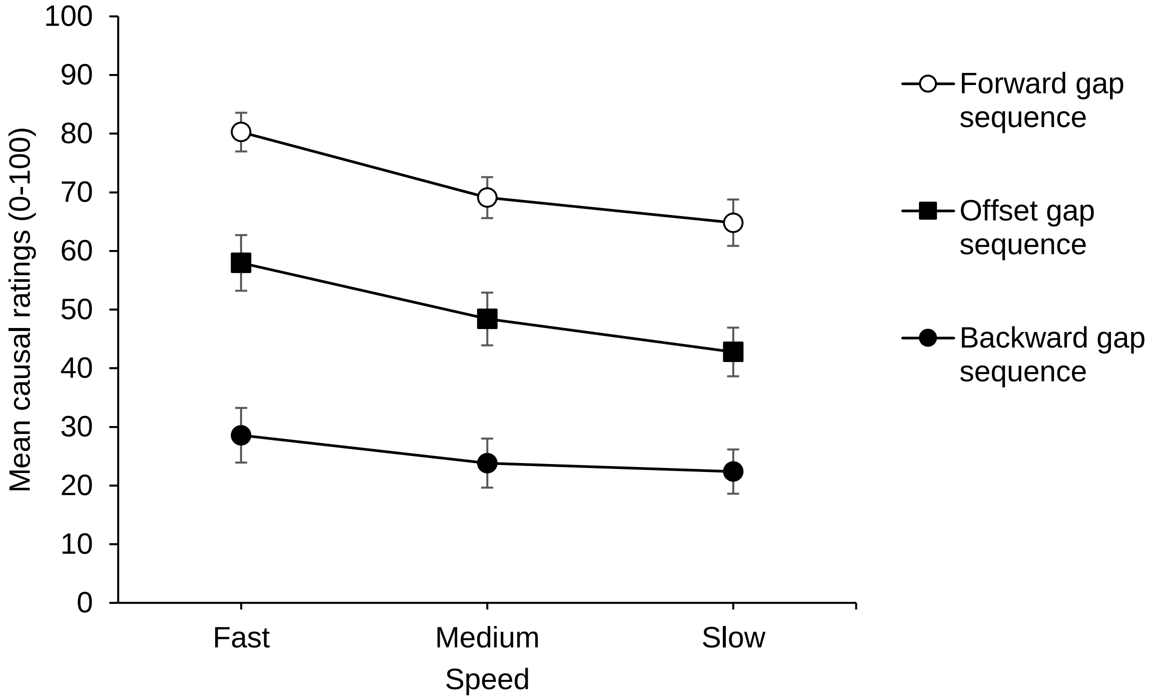

Figure 3.9. Mean causal ratings by gap sequence type and animation speed. Error bars represent 95% confidence intervals.

A two-way, repeated-measures ANOVA was carried out on the mean causal intervals. The test for the main effect of gap sequence type violated the assumption of sphericity, as tested using Mauchly’s test of sphericity (χ^2 = 12.41, *p* = .002). The degrees of freedom reported for this test have been corrected with the Greenhouse-Geisser correction (ε = .73). The assumption of sphericity was not violated by the main effect of animation speed (χ^2 = 4.68, *p* = 0.10) or the interaction term (χ^2 = 12.53, *p* = .19).

There was a significant effect of gap sequence type on causal ratings ($F(1.45, 40.92) = 35.30$, $p < .001$, $\eta_p^2 = .56$), as well as a significant main effect of animation speed ($F(2, 50) = 68.24$, $p < .001$, $\eta_p^2 = .71$) and a significant interaction ($F(4, 100) = 4.92$, $p = .001$, $\eta_p^2 = .15$).

Planned contrasts were carried out, comparing the forward gap sequence with the offset gap sequence type, and the offset gap sequence with the backward gap sequence type. Similarly, comparisons were carried out between the fast and medium speeds, and the medium and slow speeds. Significant differences were found between the forward and offset gap sequences ($F(1, 28) = 33.32$, $p < .001$, $\eta_p^2 = .54$), as well as between the offset and backward gap sequence types ($F(1, 28) = 12.99$, $p = .001$, $\eta_p^2 = .32$). Additionally, significant differences were found between the fast and medium speeds ($F(1, 28) = 42.69$, $p < .001$, $\eta_p^2 = .60$) and the medium and slow speeds ($F(1, 28) = 41.70$, $p < .001$, $\eta_p^2 = .60$).

The interaction between gap sequence type and speed (slow and medium) appeared due to an increase in the difference between the forward and offset gap sequences. This change was of a small magnitude (1.35 on the 0-100 rating scale), however, and large differences between the offset and forward gap sequences can still be seen at all animation speeds. See Appendix B for more detail on this interaction.

Overall, as expected, participants reported the highest causal ratings for the forward gap sequence type, followed by the offset gap sequence type and, lastly, the backward gap sequence type. The differences in causal ratings between these appear consistent and of a large magnitude. While significant differences in causal ratings between the different animation speeds were observed, these were of a much smaller magnitude.

3.4.2.3. Interval estimate errors

Each participant’s mean temporal estimate was calculated per condition, for inclusion in analysis. Estimate errors were produced by subtracting the actual duration of the temporal gap by from each mean interval estimate (milliseconds). Overall, estimate errors were similar for all gap sequence types, with the highest mean estimate errors reported for offset gap sequences (61.66ms, SD = 128.00ms), followed by the forward gap sequences (46.13ms, SD = 119.29ms) and the backward gap sequences (43.89ms, SD = 106.19ms). Participants tended to overestimate the delay intervals at all speeds, with the highest mean overestimation at slow speeds (85.51ms, SD = 150.07ms), followed by medium speeds (63.59ms, SD = 128.35ms) and fast speeds (2.59ms, SD = 93.26ms). See Figure 3.10 for a summary of the mean estimate errors.

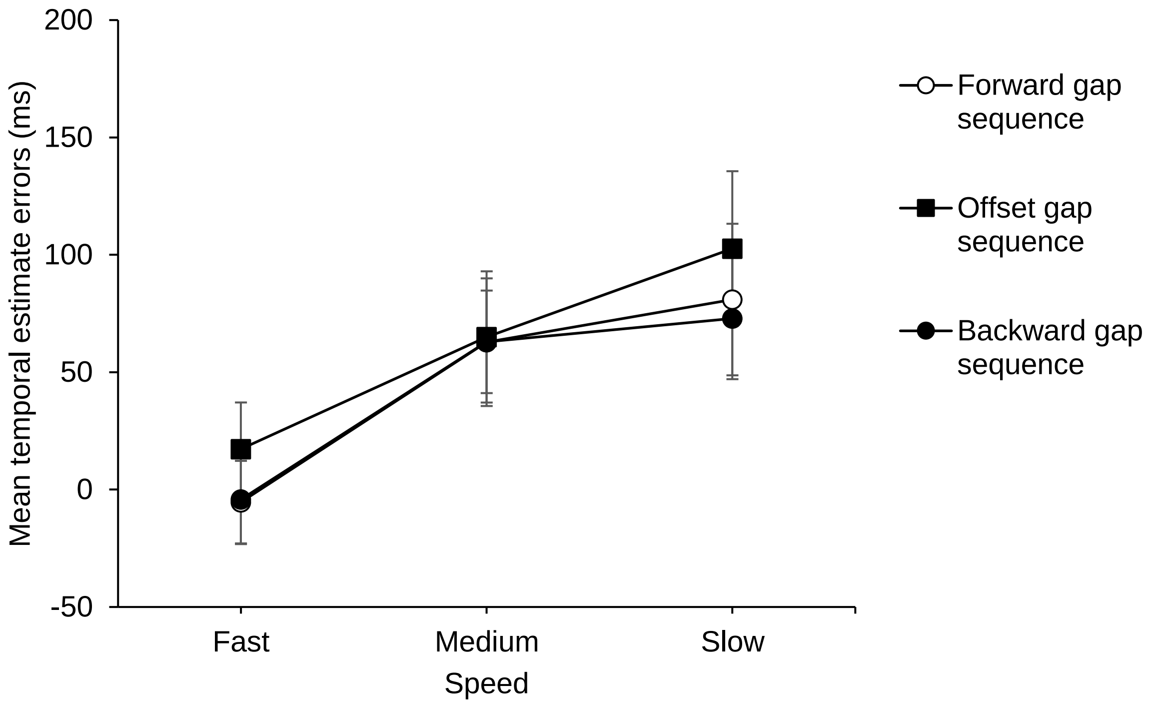

Figure 3.10. Mean estimate errors by gap sequence type and animation speed. Error bars = 95% confidence interval.

A two-way, repeated measures ANOVA was carried out on the mean estimate errors. Mauchly's test of sphericity found sphericity violations in the main effects of gap sequence type ($\chi^2(2) = 8.43$, $p = .02$) and animation speed ($\chi^2(2) = 6.67$, $p = .04$), and the interaction between the two ($\chi^2(9) = 17.15$, $p = .047$). As such, all degrees of freedom reported here have been corrected with the Greenhouse-Geisser correction, where appropriate ($\varepsilon = .79$, .82 and .76, respectively).

There was no significant main effect of gap sequence type on estimate errors ($F(1.58, 44.16) = 1.10$, $p = .33$, $\eta_p^2 = .04$). A significant main effect of animation speed was found, however, suggesting estimate errors were higher at longer intervals ($F(1.64, 45.94) = 10.03$, $p = .001$, $\eta_p^2 = .26$). There was no significant interaction between the two ($F(3.04, 85.08) = .37$, $p = .78$, $\eta_p^2 = .01$). Planned contrasts comparing the fast and medium, and slow and medium speeds found a significant difference between the fast and medium speeds ($F(1, 28) = 14.08$, $p = .001$, $\eta_p^2 = .34$), but not between the medium and fast speeds ($F(1, 28) = 1.60$, $p = .22$, $\eta_p^2 = .05$).

3.4.3. Discussion

As in experiment 1, no evidence for temporal binding was found, although causal judgements suggest that stimuli were effective in eliciting distinct causal impressions, with the highest causal ratings for forward backward gap sequences, the lowest for backward gap sequences and intermediary causal ratings for the offset gap sequences. Notably, the lowest estimate errors were found for the backward gap sequence animations in direct contradiction with causal accounts of temporal binding, although this effect was not statistically significant.

Experiment 3 was a replication of experiment 2 using interval reproduction, rather than interval estimation. This change was made to test whether the same findings occur when using a different measure of perceived intervals and to test the suitability of interval reproduction tasks in researching temporal binding in phenomenal causality. Interval reproduction has the benefit of being a faster and more intuitive task. The use of a different measure of time perception can clarify whether the findings of experiments 1 and 2 were affected by the measure used, or whether these effects can be replicated across different measures of interval perception. In addition, the experiment design was altered such that causal and temporal trials were interleaved in order to ensure participants attended to the apparent causality or lack thereof in the stimuli. Here, participants were not informed before viewing each animation whether they will be asked to make causal or temporal judgements.

3.5. Experiment 3

3.5.1. Methods

3.5.1.1. Participants

31 Cardiff University students were recruited via Cardiff University's Experiment Management System (age range 18-33, 5 male; data missing from one participant). Participants were excluded from taking part if they had participated in any of the previous experiments on temporal binding in phenomenal causality. All participants had normal or corrected-to-normal vision. Participants took part in exchange for course credit or a payment of £3.

3.5.1.2. Apparatus and materials

The experiment was carried out using a computer screen with participant responses recorded using a keyboard. The experimental programme was run in PsychoPy (Peirce et al., 2019). All stimuli were presented at 60 frames per second, on a screen size of 59.5 by 33.5cm and screen resolution of 2,560 by 1,440 pixels.

As in experiment 2, participants were presented with the forward, offset and backward gap sequence animations at fast, medium, and slow speeds. Stimulus sizes in pixels were retained from experiment 2, with changes to the absolute sizes of stimuli due to the change in monitor. Instructions were presented with a text height of 0.46cm. The aperture in which animations were presented was 21.53cm wide by 13.95cm in height. The fixation cross was 0.35cm by 0.35cm and presented 1.02cm above the centre of the screen. The gap and launching objects were 0.74cm wide and 1.49cm in height. The gap objects were separated by 0.09cm gaps. Finally, in offset animations, the launching objects were placed 2.32cm lower than the gap objects.

The delay intervals and the timing of all visual events remained the same as in experiments 1 and 2. The speed at which the launching objects travelled was 0.42cm per frame at fast speeds, 0.28cm at medium speeds and 0.21cm at slow speeds.

3.5.1.3. Design and procedure

The experiment used the same two-way repeated-measures factorial design as experiment 2, with 3 animation types and 3 speeds. Participants performed 20 experimental trials of each condition (9 conditions in total). 10 were causal trials, as in experiments 1 and 2. Temporal trials measured the perceived duration of the delay between the stopping of the launcher and movement of the target object using an interval reproduction task. Participants were asked to hold the control key on the keyboard for the duration of the interval. Participants were only asked to provide one of the two measures on each trial. All experimental trials were included in a single block. Prior to each trial, participants were not informed as to which task – causal judgements or interval reproduction – they would be asked to perform after viewing the animation.

All animation types, speeds and measures were presented in a randomised order within a single block. Before the experimental block, participants performed a practice block of six trials (3 causal and 3 temporal), in which each animation type was presented twice at a randomly selected speed.

Participants were tested individually. All participants were required to agree to an electronic consent form before the experiment began. Instructions were presented on the screen as needed (see Appendix A for the full main instructions) and participants initiated the beginning of each block of trials. At the end of the session participants were paid and debriefed as to the purpose of the experiment.

3.5.2. Results

3.5.2.1. Exclusions

Four participants were excluded according to predetermined exclusion criteria. The mean reproduced intervals were calculated for each participant in each of the speed conditions and participants were excluded from analysis if they failed to estimate short intervals (266.67 milliseconds) as shorter than medium intervals (400.00 milliseconds), and medium intervals as shorter than long intervals (533.33 milliseconds) overall and in at least 2 of the three animation types. One participant's data was lost due to a technical error, leading to a total of 26 included in the analysis.

3.5.2.2. Causal ratings

The mean causal ratings per participant, per condition, were calculated for use in the analysis. As expected, mean causal ratings were highest in the forward gap sequence animations (71.40, SD = 16.30), followed by the offset gap sequence animations (59.81, SD = 18.91) and the backward gap sequence animations (41.12, SD = 20.20). Additionally, there was a smaller difference between the animation speeds, suggesting that faster animations were perceived as more causal, with the highest mean causal rating found in fast speeds (62.80, SD = 15.10), followed by medium speeds (57.43, SD = 15.47) and slow speeds (52.10, SD = 16.32). A summary of these findings can be seen in Figure 3.11.

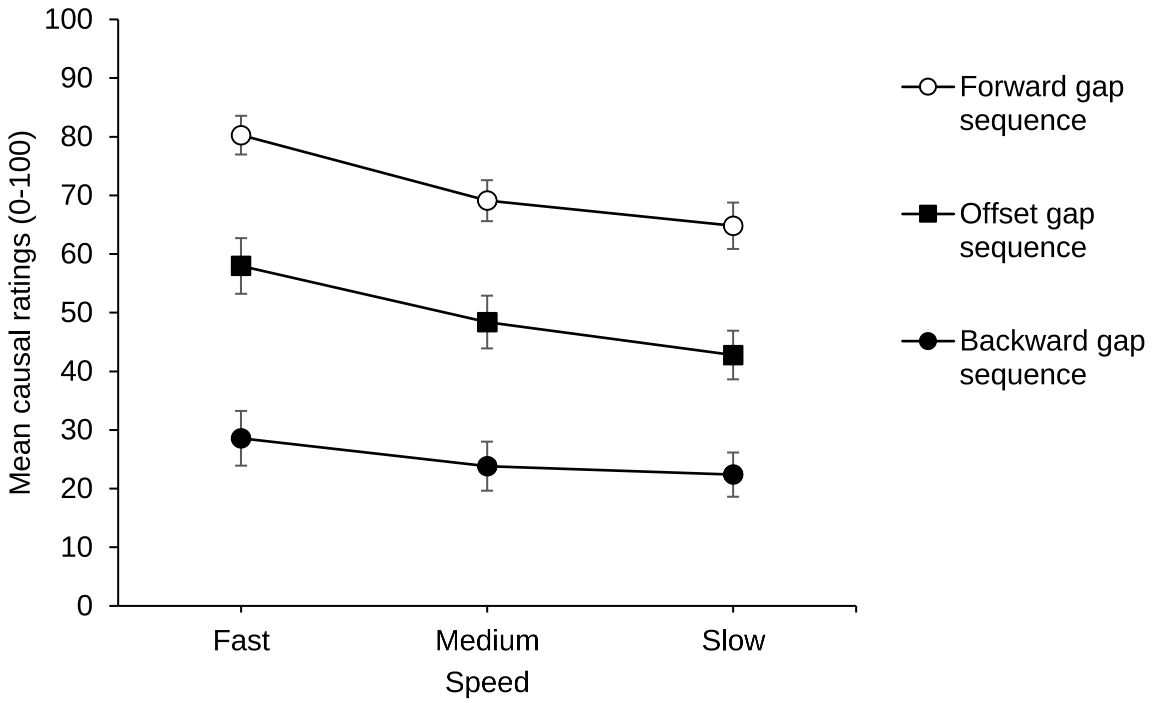

Figure 3.11. Mean causal ratings by gap sequence type and animation speed. Error bars represent 95% confidence intervals.

A two-way, repeated-measures ANOVA was carried out on the mean causal ratings. As the assumption of sphericity, as tested with Mauchly's test of sphericity, was not met for the main effects of gap sequence type ($\chi^2(2) = 7.63$, $p = .02$) and speed ($\chi^2(2) = 12.34$, $p = .002$), the degrees of freedom reported here have been corrected using Greenhouse-Geisser correction ($\varepsilon = .79$ and $\varepsilon = .71$, respectively). The assumption of sphericity was not violated for the interaction between gap sequence type and animation speed ($\chi^2(9) = 11.39$, $p = .25$).

Significant main effects of gap sequence type ($F(1.57, 39.30) = 36.32$, $p < .001$, $\eta_p^2 = .59$) and animation speed ($F(1.43, 35.67) = 40.81$, $p < .001$, $\eta_p^2 = .62$) were found. Additionally, a significant interaction was found between gap sequence type and animation speed ($F(4, 100) = 3.10$, $p = .02$, $\eta_p^2 = .11$).

Planned contrasts were carried out according to the expected causal ratings. The forward gap sequence was compared with the offset gap sequence, and offset gap sequence was compared with the backward gap sequence. Significant differences were found in both comparisons ($F(1, 25) = 34.38$, $p < .001$, $\eta_p^2 = .58$ and $F(1, 25) = 26.55$, $p < .001$, $\eta_p^2 = .51$, respectively). Based on the previous two studies, the medium animation speed was expected to result in lower causal ratings than the fast animation speed, and higher ratings than the slow animation speed. Significant differences were found between the medium animation speed and the slow animation speed ($F(1, 25) = 19.47$, $p < .001$, $\eta_p^2 = .44$) and the fast animation speed ($F(1, 25) = 26.55$, $p = .02$, $\eta_p^2 = .52$).

The interaction effect appeared to be driven by a difference in the effect of animation speed (slow and medium) between the forward and offset gap sequence types. As in experiment 2, the magnitude of this difference was small relative to the measurement scale (3.23 on a scale of 0-100) and would not be expected to result in a significant effect on perceived interval lengths. The analysis of this interaction can be seen in Appendix B.

Overall, these findings replicate those of experiment 2, with higher causal ratings for the forward gap sequence type compared with offset gap sequences, and higher causal ratings for offset compared with backward gap sequence types. Faster animation speeds led to higher causal ratings overall, although the differences found here were of a smaller magnitude than those found between different animation types.

3.5.2.3. Interval reproduction errors

The mean reproduced interval per condition was calculated for each participant. For the analysis, the actual gap intervals were subtracted from the mean reproduced interval to produce the estimate errors (see Figure 3.12 for the mean estimate errors). All reproduction errors are reported in milliseconds. Overall, the mean reproduction errors were lowest for the forward gap sequences (352.76, SD = 242.11), followed by the offset and backward gap sequences (371.83, SD = 249.63 and 379.29, SD = 276.17, respectively). Participants showed a tendency to overestimate gap intervals of all lengths, as reflected by positive reproduction errors, with intervals at medium speeds (400 millisecond gap) being the most over-estimated (398.22, SD = 258.47), followed by the slow (364.52, SD = 278.08) and fast (341.14, SD = 241.06) speeds.

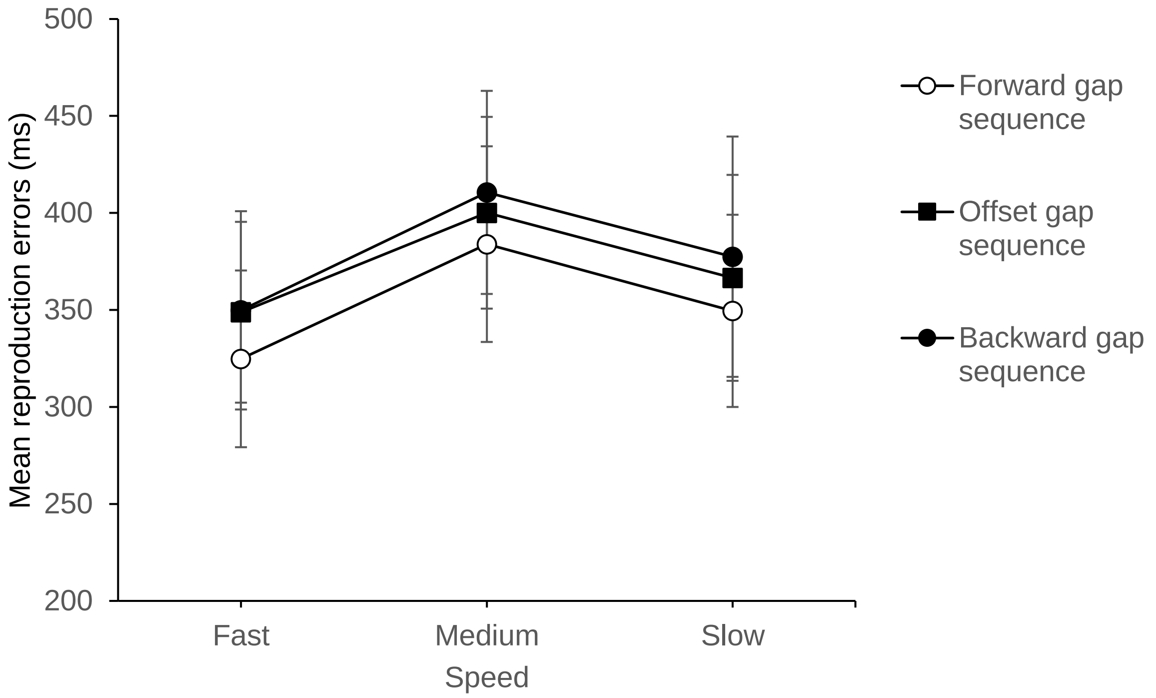

Figure 3.12. Mean reproduction errors by gap sequence type and speed. Error bars represent 95% confidence intervals.

A two-way, repeated-measures ANOVA was carried out on the mean reproduction errors. As Mauchly's test of sphericity found a significant violation of sphericity in the animation speed main effect analysis ($\chi^2(2) = 12.34$, $p < .001$), this analysis has been corrected using the Greenhouse-Geisser correction ($\varepsilon = .62$). The assumption of sphericity was met for the animation type main effect ($\chi^2(2) = 5.83$, $p = .054$) and the interaction ($\chi^2(9) = 10.10$, $p = .34$).

A significant main effect of animation speed was found ($F(1.25, 31.18) = 4.01$, $p = .046$, $\eta_p^2 = .14$). There was no significant effect of gap sequence type ($F(2, 50) = 1.73$, $p = .19$, $\eta_p^2 = .07$) or a significant interaction ($F(4,100) = .05$, $p = .995$, $\eta_p^2 < .001$). Planned contrasts found significant differences between the medium and fast speeds ($F(1, 25) = 7.83$, $p = .01$, $\eta_p^2 = .24$) and between the medium and slow speeds ($F(1, 25) = 8.64$, $p = 007$, $\eta_p^2 = .26$).

3.5.3. Discussion

Experiment 3 found largely similar results to experiment 2. Causal ratings were consistent with those found in experiments 1 and 2. Although participants showed an overall tendency to overestimate when reproducing delay intervals, this is not a concern for temporal binding research as the relative differences between estimates are of interest, rather than the overall accuracy of estimates. This effect is likely due to differences in the perceived duration of intervals between visual and tactile stimuli, as visual events have been found to be perceived as longer than tactile events (e.g. Tomassini, Gori, Burr, Sandini & Morrone, 2011). Despite larger over-estimation at medium speeds, participants consistently judged shorter intervals as shorter than longer intervals (with the exception of the four participants excluded from the analysis for failure to do so). Overall, although there is no evidence of interval reproduction being an inappropriate measure of interval estimation, experiment 3 found no evidence for temporal binding, in line with the findings of experiment 2.

3.6. Interim discussion: Experiments 1-3

The three studies described above made use of stimuli novel to temporal binding research to investigate temporal binding in phenomenal causality. No evidence for temporal binding was found. Specifically, no effect of gap sequence type on interval estimate errors was found in any of the three experiments, despite clear effects of gap sequence types on causal impressions. Causal judgements suggest that the stimuli were effective in producing distinct causal impressions, which were reliable across participants and experiments. While participants tended to overestimate interval durations in both direct interval estimation and interval reproduction tasks, this does not detract from any effects or lack thereof reported here. As these are subjective measures of time perception, it is the relative differences between conditions that are of interest, rather than a comparison between estimated intervals and the objective duration of those intervals. As both measures had been used in the past to replicate the temporal binding effect (e.g. Humphreys & Buehner, 2009;

Humphreys & Buehner, 2010; Moore, Wegner & Haggard, 2009), it is unlikely the absence of temporal binding here can be explained by the use of either measure.

It cannot be entirely ruled out that the findings may have been affected by the physical features of the stimuli used in these studies. While care was taken to ensure that all animation types are as perceptually similar as possible, some differences could not be eliminated. The differences in the direction of the colour change sequence between the forward and backward gap sequence animations may have led to differences in eye movement between these conditions. In the backward colour change sequences attention is drawn first to the left side of the screen, followed by a shift to the right side of the screen at the beginning of the colour change sequence, and finally from the left side of the screen where the sequence ends to the right side of the screen, where the launch of the target object occurs. In contrast, in forward gap sequence animations the motion of the launching objects and the colour change sequence take place consistently from left to right. Saccadic eye movement has been shown to reduce the perceived duration of time intervals (Morrone, Ross & Burr, 2005). Although participants were instructed to fixate their gaze on the fixation cross, it cannot be guaranteed that all participants followed these instructions. However, this does not appear to be a likely explanation of the absence of temporal binding. The effect of differences in eye movement between conditions would have to have offset temporal binding in all three conditions in experiments 2 and 3 to account for the absence of an effect of animation type on perceived intervals.

Another possibility is that the predictability of the time of the launch may have eliminated a temporal binding effect which would otherwise occur, as participants had a visual cue to the onset of the launch. In experiment 1 participants showed a greater overestimation of delay durations when the gap was entirely empty, suggesting that the presence of the colour change sequences affected time perception, regardless of causal impressions. However, research on the effect of predictability on temporal binding, although inconclusive, suggests that the increased predictability of the delay interval leads to a greater temporal binding effect, rather than a reduction in temporal binding (Cravo et al., 2011).

The possibility that perceptual influences other than causal impressions may have eliminated a temporal binding effect which otherwise have occurred cannot be ruled out based on experiments 1-3.The possible theoretical implications of the lack of evidence for causal binding in experiments 1-3 must be considered in the context of external variables which may have affected the findings. The possible influence of non-causal variables on the findings, discussed above, cannot be ruled out on the basis of experiments 1-3. As such, experiments 4 and 5 were carried out in order to test for effects of the physical features of the colour change sequences and the visual differences between the forward and backward gap sequence types.

3.7. Experiment 4

In experiment 4 participants viewed launching animations in which the gap sequences were hidden behind an occluder, in addition to the forward and backward gap sequence types used in experiments 1-3. This was done to remove the possible influence of perceptual differences between the animation types on time perception; in occluded animations the launching objects remained visible throughout, while the gap objects were hidden. As such, occluded animations were visually identical. Any differences in eye movement or non-causal effects of the colour change sequences could not explain differences in the perceived length of delay intervals, provided causal impressions differ between occluded animations.

Previous research has found that people can perceive a launching effect as taking place behind an occluder (e.g. Kiritani, 1999), when presented with an object moving behind an occluder, followed by a different object emerging from the other side after an appropriate delay. It was hypothesised, therefore, that participants may similarly perceive a gap sequence as occurring behind an occluder. To encourage participants to perceive the gap sequence as taking place behind the occluder, each such trial began with the gap objects and target object visible on the screen, as in visible-gap trials. Shortly after the occluding object descended, covering the gap objects. The occluding objects further covered half of the target object and the final position of the launcher to prevent any impression of the launcher object colliding with the occluder. Figure 3.13 shows the sequence of events seen in occluded animations.

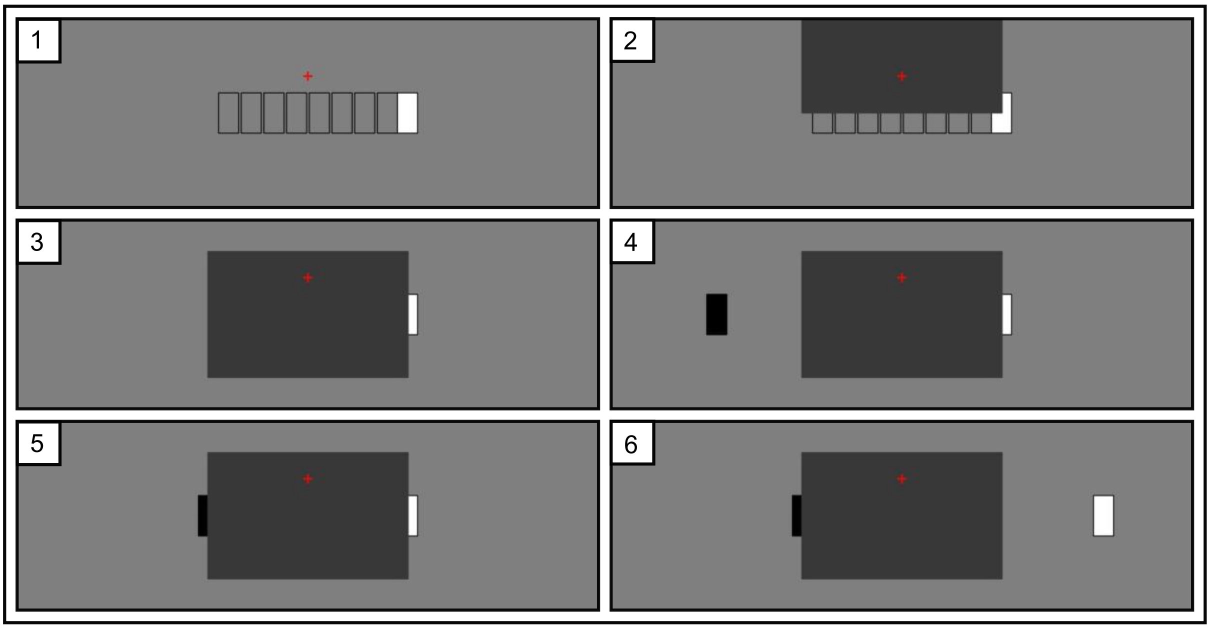

Figure 3.13. Screen shots (cropped) of the occluded animations used in experiment 4. At the beginning of each animation the gap objects and target were visible (1), followed by the occluded descending and covering the gap objects (2-3). The launching objects moved in the manner as in the stimuli used in experiments 1-3 (4-6).

Forward and backward gap sequences were presented in separate blocks to ensure participants would only perceive the intended gap sequence as taking place when the gap was occluded. This was done as the occluded animations were identical and differed only in the context in which they were presented. In each block half the trials were occluded and occluded- and visible-gap stimuli were presented at a random order. Causal rating and interval reproduction trials were interleaved to ensure that participants continue to attend to causal impressions throughout.

Experiment 4 made some further alterations to the designs used in experiments 1-3. The number of gap sequence types was reduced to two (forward and backward), as was the number of animation speeds (slow and fast). This was done to allow for the inclusion of the visibility variable and the increase of the number of interval reproduction trials to 40 per condition. The number of trials was increased to reduce the variability of reproduced intervals due to measurement noise. In contrast, causal ratings appeared highly reliable in experiments 1-3. This is unsurprising since, as reported by Michotte (1946/63), visual impressions of causality are stable and require no multiple exposure in order to occur. As such, the number of causal trials remained 5 per condition.

3.7.1. Methods

3.7.1.1. Participants

31 Cardiff University students were recruited using Cardiff University's experiment management system. Participants took part in exchange for course credits (5 male, age range 18-34). All participants had normal or corrected-to-normal vision. Students were not permitted to participate if they had taken part in any of experiments 1-3. All participants had normal or corrected-to-normal vision.

3.7.1.2. Apparatus and materials

The experiment programme was run using PsychoPy (Peirce et al., 2019). The experiment was run on an i-Mac 27" computer with the Apple Mac OS X 10.9.4 (Mavericks) operating system. The monitor was 59.5cm wide 33.5cm in height, with a resolution of 2,560 by 1,440 pixels. by The refresh rate was 60 frames per second. The computer mouse and keyboard were used for participant responses. All instructions were presented onscreen, at the beginning of each block of trials.

The text and visual stimuli were of the same sizes and proportions as experiments 1 and 3. The occluder was 7.39cm wide and 4.65cm in height (318 by 200 pixels) and positioned at the horizontal centre of the screen. At the beginning of occluded trials the occluded descended from its initial position of 23.24cm (1,000 pixels) above the centre of the screen to the centre of the screen at a speed of 0.23cm (10 pixels) per frame.

The launcher was initially positioned 23.24cm (1,000 pixels) to the left of the leftmost gap object in all trials. On each trial the launcher began to move to the right after the occluder was in place (in occluder trials) or a 100 frame (1666.67ms) delay (visible-gap trials). This was done to ensure identical trial lengths in both visible-gap and occluded trials. The animation speeds and delay intervals were the same as in experiments 1-3 (fast and slow speeds). In occluded trials the delay interval was identical to the duration of the gap sequences.

3.7.1.3. Design and procedure

This experiment employed a three-way factorial design. The three factors were as follows: gap sequence direction (forward/backward), occlusion (occluded/visible) and animation speed (fast/slow). There were eight conditions altogether.

These animations were presented in two main blocks, the order of which was counterbalanced between participants who were each randomly assigned to one of the two block orders. In the causal block, participants were presented with the forward gap sequence animations, with either occluded or visible gaps, and at both animation speeds. Each animation types (four in each block) was presented multiple times. The order of presentation within each block was randomised. The non-causal block was similar to the causal block, with the exception that the gap sequence direction was always backward. This was done to ensure participants could assume that the gap sequence taking place behind the occluding object was always the same within each block, while the occluded animations were visually identical, regardless the experimental block.

Causal ratings and reproduced intervals were collected for all conditions. Causal trials were identical to those used in experiments 1-3. In temporal trials participants were instructed to hold down the left mouse button for the duration of the temporal gap between the launching object stopping and the target object moving as in experiment 3. Similarly to experiment 3, causal and temporal trials were interleaved.

Participants were tested individually. After agreeing to an electronic consent form, participants were presented instructions explaining the task (the main instructions presented before the first block of trials can be seen in Appendix A).The blocks began with practice sections containing one temporal and one causal trial per condition (4 causal and 4 temporal practice trials in each block), presented in a random order. These were followed by 40 temporal and 5 causal experimental trials per condition (160 temporal trials and 20 causal trials per block). The presentation order of animation speeds and occluded and visible-gap trials was randomised in each block. At the end of the experiment, participants were debriefed.

3.7.2. Results

3.7.2.1. Exclusions

Four participants were excluded from analysis in total. Two participants were excluded from analysis for failure to follow instructions – these participants consistently failed to distinguish between long and short intervals (one participant had mean reproduced intervals of 94.50ms for short and 94.28m for long intervals; the other 140.91ms and 145.13ms for short and long intervals, respectively). Both participants reported short intervals as longer on average, on at least one of four comparisons.

One participant was excluded due to a technical error resulting in loss of data. One final participant was excluded from analysis due to outlying data. This participant’s mean reproduced intervals were between 3.31 and 4.10 standard deviations from the group mean in all conditions.

3.7.2.2. *Causal ratings*

The mean causal rating was calculated per participant, for each condition, for inclusion in the analysis. A summary of the mean causal ratings can be seen in Figure 3.14. Overall, participants reported perceiving forward gap sequence animations as appearing more causal than backward gap sequence animations, but only when the gap sequences were visible. Longer intervals led to lower causal impressions than shorter intervals. In visible-gap trials, mean causal ratings were higher for forward gap sequences (71.36, SD = 13.74) than for backward gap sequences (M = 55.58, SD = 19.72). Causal ratings appear similar in occluded trials regardless of the experimental block, and similar in magnitude to visible backward gap sequence trials of the same speed. As in experiments 1-3, causal ratings were higher for animations presented at fast speeds (M = 64.68, SD = 14.97) compared with slow animations (M = 51.28, SD = 14.85).

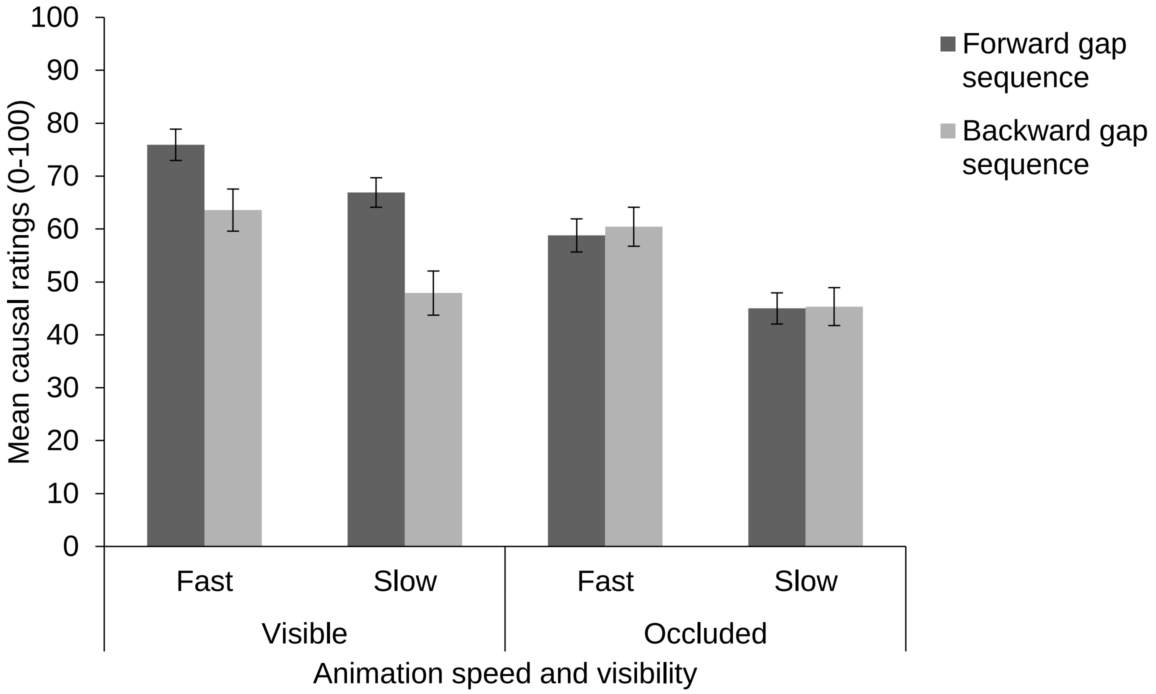

Figure 3.14. Mean causal ratings by animation type, speed and visibility. Error bars represent 95% confidence intervals.

A three-way, repeated measures ANOVA found significant main effects of gap sequence direction (F(1, 26) = 7.40, p < .001, η_p^2 = .22), visibility (F(1, 26) = 32.26, p < .001,

η_p^2 = .55) and animation speed (F(1, 26) = 36.41, p < .001, η_p^2 = .58). Additionally, significant interactions were found between gap sequence direction and visibility (F(1, 26) = 35.65, p < .001, η_p^2 = .58), and gap sequence direction and speed (F(1, 26) = 4.47, p = .04, η_p^2 = .15). Both the interaction effect between visibility and animation speed (F(1, 26) = 1.29, p = .27, η_p^2 = .05) and the three-way interaction (F(1, 26) = 1.19, p = .29, η_p^2 = .04) were not statistically significant.

A simple effects analysis found a significant difference between gap sequence directions when the gap objects were visible (F(1, 26) = 19.46, p < .001, η_p^2 = .43), but not occluded (F(1, 26) = 0.17, p = .68, η_p^2 = .007). Additionally, a significant difference between gap sequence directions was found only for slow animations (F(1, 26) = 3.11, p = .002 η_p^2 = .32), but not fast animations (F(1, 26) = 3.11, p = .09, η_p^2 = .11).

3.7.2.3. Interval reproduction errors

To produce the temporal reproduction errors, the actual gap intervals were subtracted from each participant’s mean reproduced intervals. A summary of these findings can be seen in Figure 3.15. Overall, participants overestimated all time intervals, as in experiment 3. Participants overestimated the duration of intervals in occluded animations (M = 242.15ms, SD = 209.61ms) to a greater extent than in animations in which the gap sequence was visible (M = 161.38ms, SD = 192.04ms). There appears to be possible evidence of a temporal binding effect, but only in visible, slow animations. In slow visible animations mean reproduction errors were 63.07ms shorter for forward gap sequences, whereas they were only 12.59ms shorter in fast visible animations.

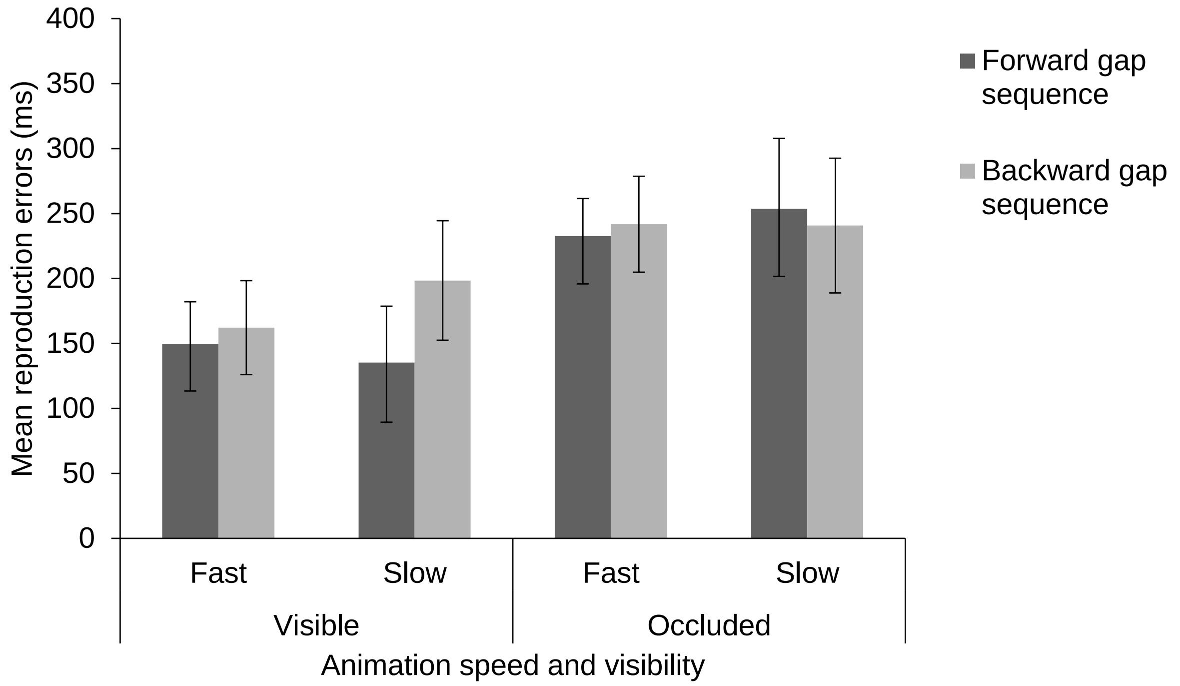

Figure 3.15. Mean reproduction errors by animation type. Error bars represent 95% confidence intervals.

A three-way, repeated measures ANOVA found a significant main effect of visibility on reproduction errors ($F(1, 26) = 36.85$, $p < .001$, $\eta_p^2 = .59$). Additionally, a significant interaction between gap sequence direction and visibility ($F(1, 26) = 7.46$, $p = .01$, $\eta_p^2 = .22$), and a three-way interaction ($F(1, 26) = 11.29$, $p = .002$, $\eta_p^2 = .30$) were found. No significant main effects of gap sequence direction ($F(1, 26) = 1.02$, $p = .32$, $\eta_p^2 = .04$) or animation speed ($F(1, 26) = .14$, $p = .71$, $\eta_p^2 = .01$) were found, or significant interactions between gap sequence direction and animation speed ($F(1, 26) = .33$, $p = .58$, $\eta_p^2 = .30$) or visibility and speed ($F(1, 26) =.01$, $p = .94$, $\eta_p^2 < .001$).

A simple effects analysis found a significant difference in temporal reproduction errors between forward and backward gap sequences, but only in visible, slow animations ($F(1, 26) = -5.44$, $p = .03$, $\eta_p^2 = .17$). No significant difference between the forward and backward gap sequence types was found in fast, visible gap sequences ($F(1, 26) = .47$, $p = .50$, $\eta_p^2 = .02$) or in occluded, fast gap sequences ($F(1, 26) = .24$, $p = .63$, $\eta_p^2 = .009$) or occluded, slow gap sequences ($F(1, 26) = .19$, $p = .67$, $\eta_p^2 = .007$).

3.7.3. Discussion

Experiment 4 used occluding objects to eliminate the possible influence of perceptual differences between the forward and gap sequence animations on time perception. Occluded animations were placed among forward or backward gap sequence animations in separate blocks to create the assumption that these gap sequences were taking place behind the occluder. This manipulation failed to yield the expected causal impressions, however. Participants reported similar causal impressions for occluded animations regardless of which block they were in. These causal impressions were comparable to the visible backward gap sequence animations, indicating no moderating of the effects of the temporal and spatial gaps between launching objects on causal impressions. Although this experiment failed to eliminate the possible role of perceptual differences (other than causal impressions) between the forward and backward sequence types, it provided some, albeit inconclusive, evidence for temporal binding in phenomenal causality.

The findings from this study diverged from those of experiments 1-3 in a number of ways. Firstly, causal impressions for backward gap sequence animations were less distinct than those reported for forward gap sequence animations. This is indicated by a lack of a significant effect of gap sequence direction on causal ratings in fast animations. This may have been caused by context effects, due to the presentation of forward and backward gap sequences in separate blocks. Presenting the gap sequence types in separate blocks may have decreased the contrast between them, thereby making the backward gap sequence animations appear more causal than they otherwise would, in the absence of a direct comparison between the two. Several studies have found context effect in phenomenal causality, where causal impressions of are affected by exposure to other stimuli, less or more visually causal (e.g. Brown & Miles, 1968; Powesland, 1959; Young et al., 2005). However, in occluded causal ratings did not significantly differ between those presented alongside visible forward gap sequences and visible backward gap sequences. If context effects had affected causal impressions, significantly lower causal ratings would be expected when occluded animations were presented alongside the more causal-appearing forward gap sequences.

Nevertheless, in contrast with experiments 1-3, the analysis suggests temporal binding in visible animations. As a statistically significant difference in causal ratings was found only in visible, slow animations, a significant difference in reproduced intervals would only be expected in those animations. Such an interaction effect was found, as reproduction errors were significantly lower in forward animations than backward animations, when these were visible and at slow speed. No significant differences were found between the gap sequence types in occluded animations or fast speeds. In other words, temporal estimate errors appeared to mirror causal ratings, with shorter reproduced intervals reported for conditions with higher causal ratings. As such, experiment 4 provided the first evidence for temporal binding in phenomenal causality in the absence of intentionality in the stimuli studied here. However, interpretation of this evidence should remain tentative, as no such effect was found in experiments 1-3. The findings can be interpreted either as emerging evidence for temporal binding in phenomenal causality or as a chance finding. Further, it should be noted these statistical tests do not necessarily indicate a direct relationship between causal impressions and interval perception at the level of the individual.

The effect of occlusion on reproduced intervals is similar to the effect of empty temporal and spatial gaps in experiment 1. The absence of a visible colour change sequence filling the temporal and spatial gap led to significantly greater overestimations than visible gaps. This suggests that the presence of a regular colour change sequences had altered interval perception in experiments 1-4. If this had affected all gap sequence conditions equally, evidence for temporal binding or lack thereof can still be detected in the analysis. The possibility that the colour change sequences had altered time perception in a way that eliminated temporal binding in experiments 1-3 can not be ruled out, however, although it is not known how temporal binding interacts with other temporal illusions.

3.8. Experiment 5

As experiment four failed to elicit differing causal impressions for visually identical stimuli, experiment 5 aimed to specifically investigate the potential effect of differences in eye movement on the findings of experiments 1-4. On some trials the launching events were replaced by “signals”, whereby the launching objects remained stationary throughout and disappeared in sequence. The object to the left of the gap objects disappeared first, followed by the gap sequence. The right object disappeared at the end of the colour change sequence. This was intended to provide non-causal stimuli in which the observer’s attention is drawn to the same parts of the screen as in launching animations.

Figure 3.16 shows screen shots of a signal animation with a forward gap sequence. In all signal animations all objects were visible at the start of each trial. The leftmost object (black) disappeared at the start of the trial, followed by either a forward or a backward gap sequence. At the end of the gap sequence, in place of a launch, the second signal object disappeared. The signal objects were taller than the gap objects and offset horizontally to ensure participants can distinguish between the signal and gap objects. It was hypothesised that signal animations may lead to lower causal ratings, as no “collision” event took place between the first signal object and the gap sequences, unlike in launching sequences.

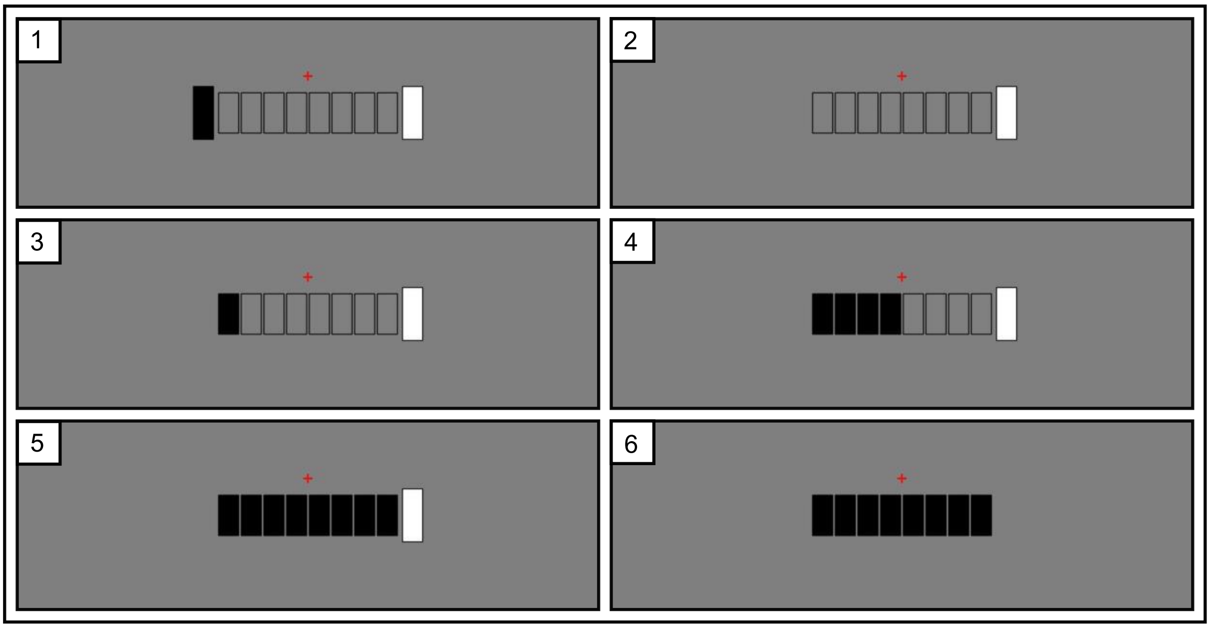

Figure 3.16. Screenshots (cropped) of a signal animation with a forward gap sequence. All objects were visible at the beginning of each trial (1). This was followed by the disappearance of the first signal object (2) and a gap sequence (3-5). After the end of the gap sequence the second signal object disappeared (6).

Signal and launching animations were presented in separate blocks, each containing all combinations of the gap sequence direction and speed factors. The same causal and temporal trials were used as in experiment 4, along with similar instructions. As experiment 4 found no significant difference in causal ratings at fast speeds, both speeds were made slower to ensure causal impressions were distinct.

3.8.1. Methods

3.8.1.1. Participants

32 Cardiff University students (3 male, one not reported, age range 18-21) participated in exchange for course credits. Participants were recruited via the School of Psychology's experiment management system. Participants who took part in experiments 1-5 were excluded from participation in this study. All participants had normal or corrected-to-normal vision.

3.8.1.2. *Apparatus and materials*

The experiment programme was run using PsychoPy (Peirce et al., 2019). The experiment was run on a Mac Mini computer running the MacOS High Sierra 10.13.6 operating system. Stimuli were presented on a 47cm by 30cm monitor at a resolution of 1,680 by 1,050 pixels and a refresh rate of 60 frames per second. All instructions were presented on the screen at a height 0.56cm (20 pixles). Participant responses were recorded using the computer mouse and keyboard.

The absolute sizes and positions of all gap sequence objects, the visual aperture and the fixation cross were identical to those used in experiment 2. Some alterations were made to the stimuli, however. This study made use of the forward and backward gap sequence stimuli, as used in previous experiments. In addition, novel animations were used, in which the black and white objects did not “launch” as in the previous experiments, but disappeared in sequence. The disappearing objects had a height of 2.35cm (84 pixels), in order to differentiate them from the gap objects, which had a height of 1.79cm (64 pixels). Signal objects were horizontally offset from the gap objects by 0.22cm (8 pixels). The aperture, fixation cross and text sizes as well as the width of the signal/launching and gap objects were retained from previous experiments. In launching animations, the launching objects had a height of 2.35cm, as in signal animations.

In launching animations, the launching objects moved at 0.17cm (6 pixels) per frame at slow speeds and 0.34cm (12 pixels) per frame at fast speeds. In both signal and launching animations, the gap sequences were 800ms or 400ms in duration at slow and fast speeds, respectively. In launching trials the launcher object was positioned 16.79cm (600 pixels) to the left of the centre of the screen at the beginning of each launching trial. In signal animations the first signal (the first signal object disappearing) occurred one second after the beginning of the trial. Trials ended 2.4 seconds or 2.8 seconds after the end of the gap sequence in fast and slow trials, respectively.

3.8.1.3. Design and procedure

The study used a repeated-measures, 3-way factorial design. Each factor contained two levels. The animation type was either a “launch” or “signal” (see apparatus and materials). The colour change sequences took place either from left to right (forward, i.e. from the direction of the first object to move/disappear in the direction of the second object to move/disappear) or from right to left (backward). All animations were presented at either a fast or slow speed.

Causal ratings and reproduced intervals were collected for each condition. For launching trials this was the same as the procedure used in experiment 4. In causal trials following signal animations the instructions changed to “Did you have the impression that the black rectangle made the white rectangle disappear?”. The same interval reproduction task as used in experiment 4 was used in temporal trials. Again, the instructions were altered for signal animations to “Please hold the left mouse button for the duration of time between then the left (black) rectangle disappeared and when the right (white) rectangle disappeared”.

Signal and movement animations were presented to all participants, in separate blocks. Each of the two possible block orders (signal followed by movement, or movement followed by signal) was presented to 16 of the 32 participants, who were randomly assigned to one of the two possible orders. The animations were presented in both speeds and with both types of gap sequences. The order of trials within each block was randomised.

Each block contained three sections. The first section was the causal section, consisting of the causal ratings task. This section which contained 10 trials per condition (40 overall). This was followed by practice temporal trials (2 per condition, 8 overall), during which reproduced intervals were produced by the participants, but not recorded. This was followed by the experimental temporal block, which contained 40 trials per condition (360 overall).

Participants were tested individually. At the beginning of the experiment participants were presented with an electronic consent form. After consent was granted, detailed instructions were provided for each part of the experiment (see Appendix A). At the end of the experiment participants were debriefed.

3.8.2. Results

3.8.2.1. Exclusions and transformation

Four participants were excluded from analysis based on pre-determined exclusion criteria. Participants were to be excluded if they consistently failed to distinguish between 400ms and 800ms intervals. Specifically, exclusions were made if participants reported a higher mean interval for the 400ms intervals than for the 800ms overall, or if they did so on two or more of the four conditions (animation type x gap sequence type). Both of the participants excluded from analysis failed to distinguish between long and short intervals on average across all trials, as well as in three of the four conditions. It should be noted that this test is conservative, in that it only excludes participants who performed at or below chance level. Two further participants were excluded due to outlying data (3+ standard deviation from the mean). One participant showed outlying data in four of the eight conditions, and the other in three of the eight.

The temporal data was found to be consistently non-normally distributed, as tested using the Shapiro-Wilk test (p < .05 in 5 of the 8 conditions). As a positive skew was observed in all conditions, the data was transformed using a log transformation (base 10). The transformation was made on the raw scores rather than estimate errors to avoid adjusting negative values with an arbitrarily chosen constant, as the choice of constant has been shown to affect p values (Feng et al., 2014). The transformed data meets the assumption of normality in all conditions, as tested using the Shapiro-Wilk test of normality (p > .05). The data before and after transformation can be seen in Figures 3.18 and 3.19, in section 3.8.2.3.

3.8.2.2. *Causal ratings*

The mean average causal rating for each condition, per participant, was included in the analysis. See Figure 3.17 for a summary of these findings. Participants reported higher mean causal ratings in animations with forward gap sequences (M = 73.61, SD = 16.98) than animations with backward gap sequences (M = 28.73, SD = 20.09). Mean causal ratings also appear higher for fast speeds (M = 43.83, SD = 12.47) compared with slow speeds (M = 38.03, SD = 12.74). Causal ratings for signal animations were higher than expected and appear to be similar to those reported for launching animations.

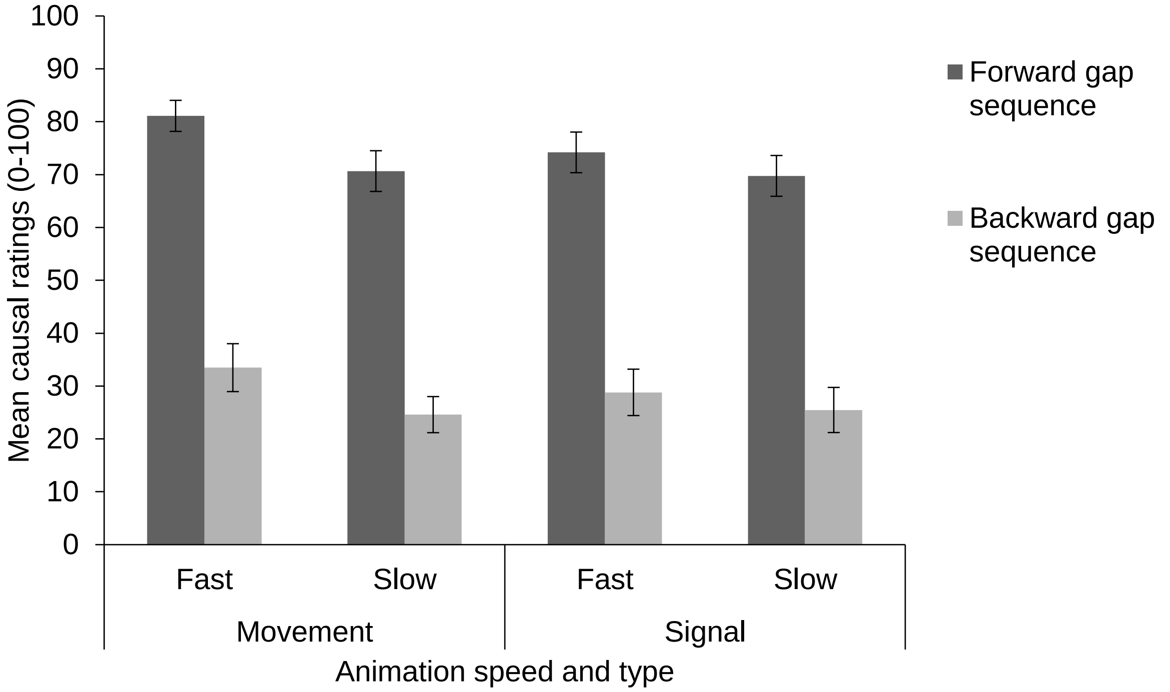

Figure 3.17. Mean causal ratings by gap sequence type, movement speed and animation type. Error bars represent 95% confidence intervals.

A repeated-measures, three-way ANOVA found significant main effects of gap sequence direction ($F(1, 27) = 136.86$, $p < .001$, $\eta_p^2 = .84$) and speed ($F(1, 27) = 25.27$, $p < .001$, $\eta_p^2 = .48$). Additionally, a significant interaction was found between animation type and speed ($F(1, 27) = 10.57$, $p = .003$, $\eta_p^2 = .28$). No significant main effect of animation type was found ($F(1, 27) = 1.98$, $p = .17$, $\eta_p^2 = .07$). Likewise, no significant interactions were found between animation type and gap sequence direction ($F(1, 27) = .23$, $p = .23$, $\eta_p^2 = .009$), gap sequence direction and speed (F(1, 27) = .89, p = .35, η_p^2 = .03) or the three-way interaction (F(1, 27) = .23, p = .64, η_p^2 = .008).

A simple effects analysis found a significant difference between the movement and signal animation types at fast speeds *(F*(1, 27) = 6.09, p = .02, η_p^2 = .18*),* but not at slow speeds (F(1, 27) = .05, p = .72, η_p^2 = .005). At a descriptive level, however, this difference was of a much smaller magnitude than the difference between gap sequence types and may not be large enough to lead to a detectable effect on interval perception.

3.8.2.3. Reproduced intervals

The mean reproduced intervals and log-transformed (base 10) intervals are presented in Figures 3.18 and 3.19 respectively. Overall reproduced intervals were consistently higher for slow animation speeds (M = 742.22ms, SD = 254.30ms) than fast animation speeds (M = 957.47ms, SD = 282.13ms). Surprisingly, mean reproduced intervals were lower for backward gap sequences (843.06ms, SD = 265.38ms) than for forward gap sequences (857.63ms, SD = 262.05ms), unlike in previous experiments. The reproduced intervals were shorter for launching animations than for signal animations in all conditions with the exception of slow backward gap sequences, with a mean reproduced interval of 837.59ms (SD = 238.12ms) for launching animations and 836.10ms (SD = 299.12ms) for signal animations.

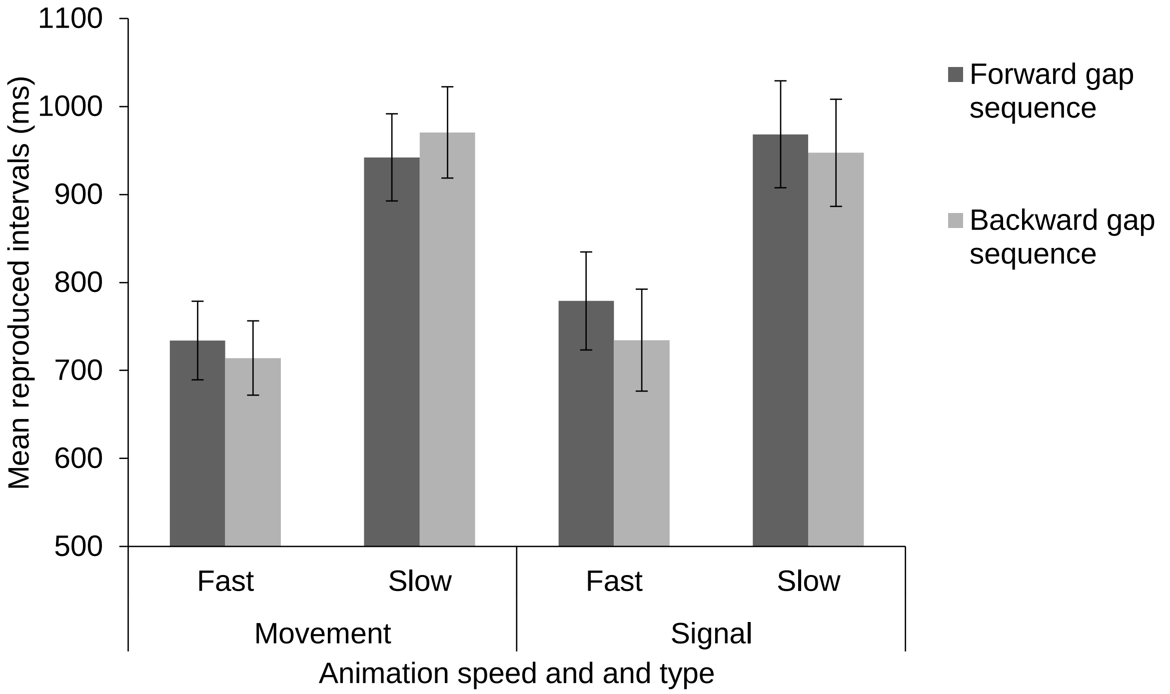

Figure 3.18. Mean reproduced intervals by gap sequence type, movement speed and animation type. Error bars represent 95% confidence intervals.

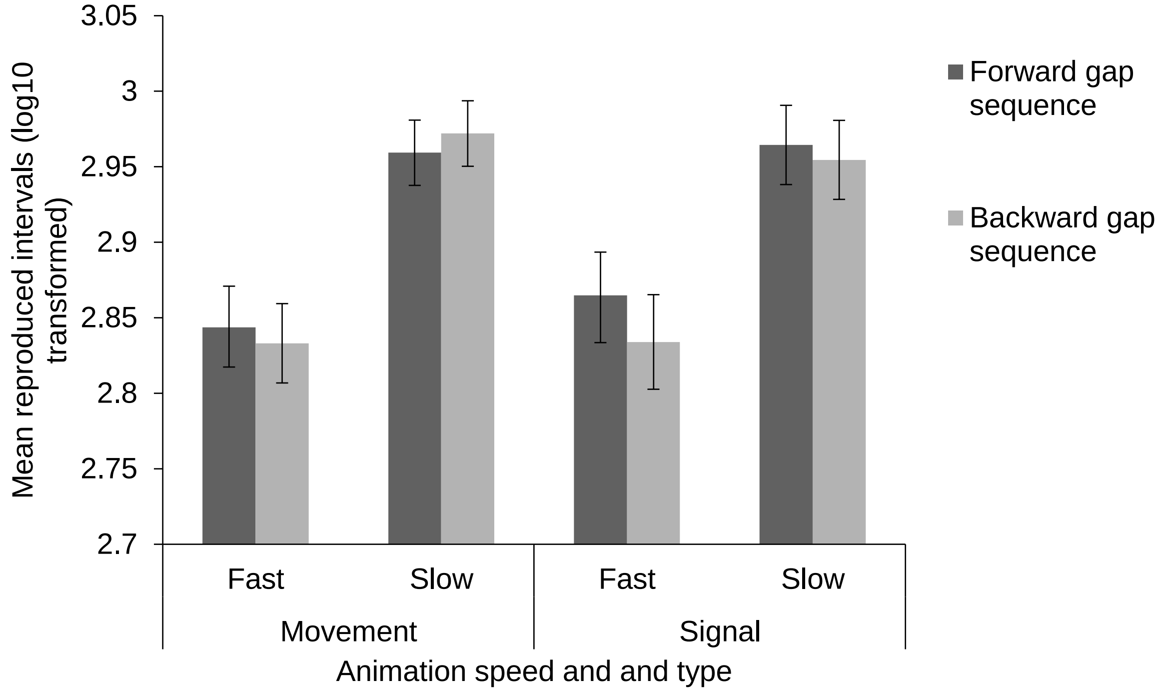

Figure 3.19. Mean reproduced intervals (log10 transformed) by gap sequence type, movement speed and animation type. Error bars represent 95% confidence intervals.

All analyses made use of the transformed data. A three-way, repeated-measures ANOVA found significant main effects of gap sequence direction ($F(1, 27) = 9.43$, $p = .005$,

$\eta_p^2 = .26$) and animation speed ($F(1, 27) = 114.70$, $p < .001$, $\eta_p^2 = .81$). Additionally, significant interactions were found between gap sequence direction and animation type ($F(1, 27) = 14.40$, $p = .001$, $\eta_p^2 = .35$), and gap sequence direction and speed ($F(1, 27) = 10.01$, $p = .004$, $\eta_p^2 = .27$). No significant main effect of animation type was found ($F(1, 27) = .31$, $p = .58$, $\eta_p^2 = .01$). No significant interaction between animation type and speed ($F(1, 27) = 2.61$, $p = .12$, $\eta_p^2 = .09$) or a significant three-way interaction ($F(1, 27) < .001$, $p = .99$, $\eta_p^2 < .001$) were found.

A simple effects analysis found significant differences between the forward and backward gap sequence types, but only at fast speeds ($F(1, 27) = 16.72$, $p < .001$, $\eta_p^2 = .38$) or signal animations ($F(1, 27) = 19.51$, $p < .001$, $\eta_p^2 = .42$). The same comparison was not statistically significant at slow speeds ($F(1, 27) = .18$ $p = .68$, $\eta_p^2 = .007$) or for movement animations ($F(1, 27) = .08$, $p = .78$, $\eta_p^2 = .003$). Therefore, the main effect of gap sequence direction does not appear to reflect a reversal of the expected temporal binding effect, as such an effect would be expected to take place across both animation types based on the causal ratings.

3.8.3. Discussion

The signal animations in experiment 5 failed to create non-causal perceptually matched controls for the launching animations. Surprisingly, the lack of movement and collision between the “launcher”, and the spatial gap between the disappearing objects and the gap objects did not significantly decrease causal ratings for the most part. The exception to this is the fast, forward gap sequence animations in which a significant difference was observe, although causal ratings were still higher for forward gap sequence, signal animations, compared with backward gap sequence, signal animations. As such, this experiment cannot confirm or disconfirm an effect of shifts in eye movements between the right and left sides of the screen on time perception.

Nevertheless, experiment 5 yielded some interesting findings. The perceived effect of the leftmost object on the rightmost object, regardless of whether they “launched” or disappeared in sequence indicates that the gap sequence does not merely preserve launching impressions when temporal and spatial delays are present. Here, forward gap sequences appeared to lead to the perception of a causal link between the disappearing objects where none would be expected in the absence of such a gap sequence, regardless of the temporal and spatial gap between them. This effect was as consistent as that seen for launching animations. The signal animations provide a replication of the findings of experiments 1-3 in a novel set of causal and non-causal animations.

3.9. Experiment 6

Along with experiments 1-3, experiment 5 found further evidence for a lack of temporal binding in phenomenal causality due to perceived causality alone, while the findings of experiment 4 found possible evidence of temporal binding. However, a possible interaction between causality and intentionality has not been explored up to this point. Agency accounts of temporal binding predict that temporal binding will only take place when agency is present in addition to a perceived or inferred causal relationship between an action and its outcome. As discussed previously, Cravo et al. (2009) found evidence for this using launching animations with delays between the stopping of the launcher and the movement of the target object. In their findings a clear interaction between perceived causality and intentionality can be seen. Temporal estimates were significantly lower when participants triggered a “launching” effect animation, compared with non-causal animations in which the launching objects moved apart in sequence. However, this only occurred when the launcher was moved by the participant and not when the animation was observed passively. The authors concluded that these findings show that both causality and agency are necessary for temporal binding to occur.

It can be argued, however, that this design contains a mismatch between the visual causal impression caused by launching animations and the actual causal mechanism present in the task. Participants were effectively able only to play or pause the animation by holding down a mouse button; the speed and trajectory of the launcher were set and not controlled by the participant. The remainder of the animation was equally contingent upon the mouse button being held down for a certain amount of time regardless of whether it appeared causal. The design and conclusion of this study make no distinction between visual impressions of causality and inferred causality and therefore make interpretation difficult. Accounts which hypothesise that temporal binding results from or contributes to motor planning processes (such as the account put forward by Haggard et al., 2002) would predict that temporal binding should occur between an action and its intended consequence, rather than by concurrent impressions of causality and agency. It can therefore be predicted, based on such accounts, that temporal binding should occur between the participants' actions and subsequent visual events, regardless of visual causal impressions. As such, studying the combined effects of agency and phenomenal causality on interval perception may contribute to the understanding of how agency and causality interact to produce temporal binding.

Experiment 6 was in part a replication of Cravo et al.'s (2009) study, using the forward and backward gap sequence animations used in experiments 1-5. These animations were used, as in experiments 1-5, to ensure that animations are as perceptually similar as possible and that the animations are distinctly "causal" or "non-causal" in appearance more reliably than empty temporal gaps. In active conditions, participants were in full control of the launcher, rather than merely initiating its movement. The launcher was controlled by the mouse and moved in the same speed and direction as the mouse movements. Participants could move the launcher freely in each trial, but the gap sequence only began when the launcher collided with the leftmost gap object from the left, at an equal or higher speed to that of the colour change sequence. This was done to ensure that participants were aware their own actions were responsible for the following events.

For comparison, in other conditions participants viewed linear gap object movements, as in experiments 1-5, or simulated agent movement. In the simulated movement condition the movements of the launcher from a pilot session of the self-causal condition were played back to participants. This condition was used as a control condition, to ensure that the differences in the pattern of movement of the launcher between the linear animations and the self-causal animations did not affect the findings. The causal and temporal tasks were retained from experiments 4 and 5.

If causal accounts of temporal binding are correct, no effect of movement type on reproduced interval should be found. In particular, significantly lower reproduced intervals in forward compared with backward gap sequences would not be expected, in line with the findings of experiments 1-5. Under agency accounts, if phenomenal causality does not contribute to temporal binding, temporal binding should be observed in self-causal conditions regardless of the gap sequence direction, as the “launch” events were caused by the participants’ actions regardless of the gap sequence. If phenomenal causality contributes to temporal binding, however, the effect would only be predicted in self-causal forward gap sequence animations by agency accounts of temporal binding.

3.9.1. Methods

3.9.1.1. Participants

The sample consisted of 30 Cardiff University undergraduate participants participating for course credit. The age range was 18-23 (4 not reported) and the gender ratio was 2 males and 22 females (6 not reported). Participants were precluded from taking part if they had participated in the previous experiments reported here. All participants had normal or corrected-to-normal vision.

3.9.1.2. Apparatus and materials

The experiment programme was run using PsychoPy (Peirce, et al., 2019). The experiment was run on Stone EcoSaver80+ computer running Windows 10. The monitor was 48cm wide by 27cm in height, with stimuli presented at a resolution of 1,600 by 900 pixels and a refresh rate of 60 frames per second. The computer mouse and keyboard were used for participant responses.

This study made use of the forward and backward gap sequence stimuli, as used in previous experiments. Due to the difference in the display size and resolution, the absolute sizes of the stimuli differed while the relative sizes between the objects remained the same. The launching and gap objects were 1.92cm tall and 0.96cm in width. Gap objects were separated by 0.12cm gaps. The fixation cross was 0.45cm tall by 0.45cm in width and presented 1.80cm (60 pixels) above the centre of the screen. All text was set to a line height of 0.60cm. In all animations the launcher was placed 18cm (600 pixels) left of the centre of the screen at the start of each trial. Launcher objects moved at a speed of 0.36cm (12 pixels) per frame in all linear animations, while target objects moved at 0.36cm per frame at fast speeds and 0.18cm (6 pixels) per frame at slow speeds. As in experiment 5, the delay intervals were 400ms (3 frames per gap object) in fast conditions and 800ms (6 frames per gap object) in slow conditions. Following the launch of the target object, each trial lasted for a further 2 seconds (120 frames) during which the target object moved out of the visible screen.

In this experiment, animations were not presented within an aperture to allow participants to move the launcher freely on self-causal trials. Unlike in previous experiments, in linear movement trials the launcher object moved at the same speed regardless of the length of the gap or the movement speed of the target object. This was done to ensure that the target object always moved at the same speed or slower than the launcher object, while still leading to the same causal impressions. This was necessary because on self-causal trials participants moved the launcher before knowing the speed of the gap stimulus. This additionally prevented the gap sequence and the movement of the target from being faster than the movement of the launcher.

In linear movement animations the launching objects moved horizontally at a constant speed. In self-causal movement animations participants controlled the movement of the launcher object using the computer mouse, whereas the launched object moved in a linear path. In self-causal trials the launcher object's position was set by the position of the mouse. The "launch" would only occur in these animations if the launcher collided with the leftmost gap object from the left, at a speed equal to or faster than that of the faster of the two launching speeds. This was done to ensure that the gap sequence was never faster than the movement of the launcher. Participants were able to move the launcher freely, although it would stop upon any contact with the target or gap stimuli. The gap sequence only initiated when the launcher made contact with the rightmost gap object, moving rightward.

In simulated self-movement animations participants were shown launcher movements using mouse positions captured from the self-causal condition, as performed by a pilot participant. 40 trials were selected in which the launcher object did not move past the gap objects or collide too slowly to initiate the gap sequence. These were used in every simulated self-movement condition in a randomised order, such that each of the replayed launcher animations were used once for each combination of animation type, animation speed and movement type.

3.9.1.3. Design and procedure

The study used a repeated-measures, 3-way factorial design. The movement of the launcher object was either generated by the participant (self-causal), replayed launched object movement from a pilot participant (simulated self-movement), or horizontal and at a constant speed (linear movement). The colour change sequences took place either from left to right (forward, i.e. from the direction of the first object to move in the direction of the second object to launch) or from right to left (backward). Animations were presented at either a fast or slow speed.

There were 12 animation types altogether. The three movement types were presented in separate blocks, while the gap sequence direction and speed were interleaved (i.e. 4 animation types in each block). Each block contained 3 sections, presented in the following order: causal trials, practice temporal trials and experimental temporal trials. In each block there were 10 causal trials per condition (40 in total), 2 practice temporal trials per condition (8 in total) and 40 experimental temporal trials per condition (160 in total). The self-causal block began with an additional practice session in which participants practiced moving the launcher (twice per condition) being asked for causal ratings or reproduced intervals. Within each section the trials, varying in speed and gap sequence direction, were presented in a randomised order. For simulated self-movement conditions, each of the 40 pre-recorded launcher movement patterns was played once in each condition in the experimental temporal trials, and randomly selected movement patterns were played in the causal and practice temporal sections. The order of the three main blocks was counterbalanced between participants, which each participant randomly assigned to one of the six possible block orders (each possible order was presented to five different participants).

Causal trials were identical to those used in previous experiments. Participants were only asked whether they had a visual impression of the launcher causing the target to move. Temporal trials used the interval reproduction task from experiments 4 and 5.

At the beginning of the experiment participants were presented with a consent form. Following this, detailed instructions were provided for each block and section of the experiment (see Appendix A for the main instructions presented at the beginning of each block). Participants were tested in groups of up to 10 at a time in separate cubicles. All participants were debriefed at the end of the experiment.

3.9.2. Results

3.9.2.1. Exclusions

Four participants in total were excluded from the analysis. One participant was excluded from analysis based on pre-determined exclusion criteria. Participants were to be excluded if they consistently failed to distinguish between 400ms and 800ms intervals. Specifically, exclusions were made if participants reported a higher mean interval for the 400ms intervals than for the 800ms overall, or if they did so on two or more of the four conditions (movement type x gap sequence type). Two participants' data was missing due to technical errors. One participant was excluded due to outlying data (over 3 standard deviations from the mean in 7 of the 12 conditions).

3.9.2.2. Causal ratings

The mean causal ratings per participant, per condition, were included in the analysis. See Figure 3.19 for a summary of these findings. As in previous experiments, causal ratings were higher for forward gap sequences (M = 71.21, SD = 19.88) than backward gap sequences (M = 38.05, SD = 27.89), and higher for shorter gap intervals (M = 58.80, SD = 19.24) than for longer gap intervals (M = 50.45, SD = 22.57). Additionally, causal ratings appear to be more extreme for linear animations, with higher causal ratings for forward gap sequences and lower causal ratings for the backward gap sequences, compared with the other movement conditions.

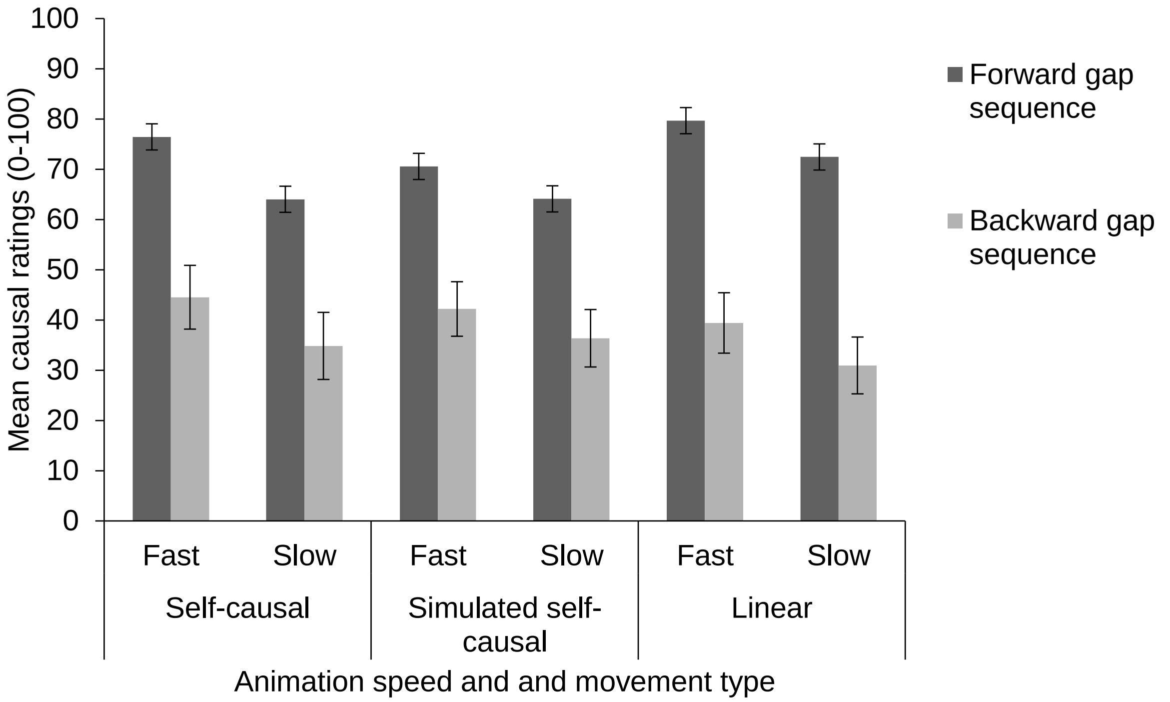

Figure 3.20. mean causal ratings by gap sequence type, movement speed and movement type. Error bars represent 95% confidence intervals.

The assumption of sphericity, as tested by Mauchly's test of sphericity, was met for the movement type main effect ($\chi^2(2) = 3.47$, $p = .18$), its interactions with gap sequence direction ($\chi^2(2) = 1.39$, $p = .50$) and animation speed ($\chi^2(2) = 2.12$, $p = .35$) and the three-way interaction ($\chi^2(2) = 2.28$, $p = .32$). A three-way, repeated-measures ANOVA found significant main effects of animation speed ($F(1, 25) = 17.27$, $p < .001$, $\eta_p^2 = .41$) and gap sequence direction ($F(1, 25) = 41.29$, $p < .001$, $\eta_p^2 = .62$) on causal ratings. Additionally, a significant interaction between movement type and gap sequence direction was found ($F(2, 50) = 4.62$, $p = .01$, $\eta_p^2 = .16$).

Planned contrasts were used to explore the interaction between movement type and gap sequence direction. As The simulated self-movement condition was visually similar to the self-causal condition, but contained no intentional action, it would be expected that causal ratings in the simulated self-movement condition would fall between those found for the other two movement types. Repeated contrasts were used, therefore, with each of the other movement types compared with the simulated self-movement condition. A significant interaction between movement type and gap sequence direction was found only when the simulated self-movement condition was compared with the linear movement condition (F(1, 25) = 8.00, p = .009, η_p^2 = .24), but not when comparing simulated-self movement with self-movement (F(1, 25) = .40, p = .53, η_p^2 = .02). These findings suggest that causal ratings were significantly more extreme overall (higher for forward gap sequences and lower for backward gap sequences) in linear self-movement animations than for self-movement or simulated self-movement animations. However, forward gap sequences appeared to result in much higher causal ratings than backward gap sequences in all movement types and as such significant differences in reproduced intervals would be expected if temporal binding had taken place.

3.9.2.3. Interval reproduction errors

The mean reproduced interval was calculated per participant, per condition. To calculate the reproduction errors, the actual gap length (400ms in the fast animations, 800ms in the slow animations) was subtracted from each mean reproduced interval. See Figure 3.20 for the mean estimate errors. Participants made greater overestimations of short delay intervals (M = 307.70ms, SD = 293.54ms) than long delay intervals (M = 95.50ms, SD = 399.77ms). Participants appear to have consistently made greater over-estimations for self-causal animations (M = 221.94ms, SD = 344.54ms), followed by the simulated (M = 201.55ms, SD = 388.84ms) and linear animations (M = 181.32ms, SD = 383.11ms). The findings do not show evidence for temporal binding, as reproduction errors were greater for forward gap sequences (M = 205.78ms, SD = 344.54ms) than for backward gap sequences (M = 197.42ms, SD = 333.61ms).

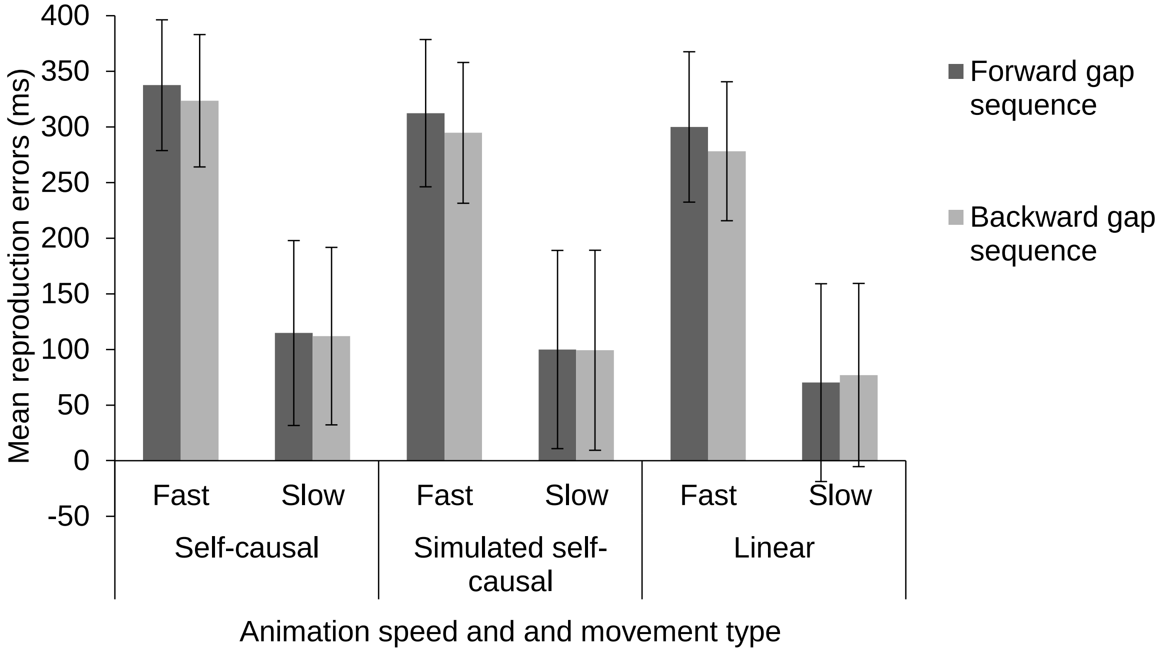

Figure 3.21. Mean reproduction errors by gap sequence type, movement speed and movement type. Error bars represent 95% confidence intervals.

The mean estimate errors per participant, per condition, were used in the main analysis. The assumption of sphericity, as tested by Mauchly's test of sphericity was violated in the main effect of movement type ($\chi^2(2) = 10.15$, $p = .006$, $\varepsilon = .74$) and the three-way interaction ($\chi^2(2) = 6.88$, $p = .03$, $\varepsilon = .80$); adjusted degrees of freedom are reported where appropriate. The assumption of sphericity was not violated in the interaction between movement type and gap sequence direction ($\chi^2(2) = 3.14$, $p = .21$) and movement type and speed ($\chi^2(2) = .15$, $p = .93$).

A three-way, repeated-measures ANOVA found significant main effects of animation speed ($F(1, 25) = 36.33$, $p < .001$, $\eta_p^2 = .59$). No significant main effects of movement type ($F(1.49, 37.18) = .27$, $p = .70$, $\eta_p^2 = .01$) or gap sequence direction ($F(1, 25) = 3.38$, $p = .08$, $\eta_p^2 = .12$) were found. Similarly, no significant interactions were found between movement type and gap sequence direction ($F(2, 50) = .01$, $p = .99$, $\eta_p^2 < .001$), movement type and speed ($F(2, 50) = .09$, $p = .91$, $\eta_p^2 = .004$) or gap sequence direction and speed ($F(1, 25) =$

3.00, p = .10, $\eta_p^2 = .11$). No significant three-way interaction was found (F(1.60, 40.02) = .25, p = .73, $\eta_p^2 = .01$).

These findings show significant overestimation of shorter intervals compared with longer intervals. Estimate errors were lower for backward gap sequences (non-causal animations), the opposite of what would be expected due to causal or intentional binding.

3.9.3. Discussion

Experiment 6 did not find evidence for temporal binding, regardless of agency. Causal ratings were similar to those observed in experiments 1-5, while no effect of agency on reproduced intervals was found. As in experiment 5, reproduced intervals were found to be significantly shorter for backward gap sequence animations compared with forward gap sequence animations.

Causal ratings were similar regardless of the type of movement (self-causal, simulated self-causal or linear), indicating the results cannot be explained by differences in phenomenal causality between the three types of movement. Likewise, the differences in the launcher movement between conditions is unlikely to have affected the findings, as no significant difference was found between the simulated self-movement control condition and the self-movement condition. This does not rule out the possibility that interval perception might have been affected by the participant's hand movements in the self-causal condition, however. This explanation nevertheless appears unlikely, as an effect of hand movement unrelated to agency would be expected to affect reproduced intervals in both forward and backward gap sequence animations, and no such effect was found.

The findings are therefore consistent with previous findings in suggesting that either phenomenal causality does not contribute to temporal binding, or that certain features of the stimuli used have eliminated the temporal binding effect. As this study was not a direct replication of Cravo et al.'s (2009) research, it is not possible to determine the reliability of Cravo et al.'s findings based on the current findings. It is evident, however, that the combined presence of perceived causality and intentionality do not necessarily lead to temporal binding.

3.10. Slope analyses

The findings of experiments 1-6 suggest lack of temporal binding in phenomenal causality, both when agency is absent and present, in the stimuli used here. Temporal binding experiment designs typically assume that temporal estimates should mirror causal impressions/judgements: the higher the perceived causality, the lower the estimated interval should be. Such findings may not necessarily reflect individual differences, however. In most temporal binding research causal ratings are not collected and, therefore, a direct comparison between causal impressions and interval estimates would not be possible. Here, however, both measurements had been collected for each participant.

This comparison was carried out by regressing mean temporal estimates over mean causal ratings per participants and analysing the mean slopes in each experiment. A comparison of the relationship between causal ratings and temporal estimates better accounts for the range of differences between causal ratings among participants. Unlike the ANOVA analyses, the slope coefficient of each participant is affected by the relative distance between causal ratings. Furthermore, the regression slopes are calculated based on the causal ratings reported by the participant, rather than assumptions based on the group mean for each condition.

3.10.1. Analysis

The mean causal ratings and temporal estimate errors per condition were computed for each participant in each experiment. Each condition, i.e. all combinations of gap sequence type, speed and other factors, contained two data points – one causal and one temporal mean. Based on this, regressions lines were calculated for each participant between the mean causal ratings and mean temporal estimates. If causal binding had occurred, a significantly negative slope was expected, reflecting that higher causal impressions had led to lower estimated interval errors and vice versa.

Participants who were excluded from other analyses were not included in the slope analyses. One further participant was excluded from the analysis of experiment 4, as this participant reported identical mean causal ratings in all conditions included in the analysis and a regression line could therefore not be calculated. Conditions in which no gap colour change sequence was visible were further removed from the analysis. This was done as temporal estimate errors in experiments 1 and 4 found large overestimation of delay intervals when no gap sequence was visible. As causal ratings did not significantly differ between backward gap sequences and empty or occluded gap sequences, causal impressions cannot account for the overestimation of delay intervals in the absence of a gap sequence. This effect, therefore, likely occurred due to perceptual differences, unrelated to causal impressions. These conditions would introduce a confound into the analyses, therefore, whereby empty and occluded gap sequences artificially inflate a negative relationship between causal impressions and temporal estimate errors. A comparison of the differences between the mean slopes in experiments 1 and 4, with and without empty/occluded gap conditions, can be seen in Table 3.1.

Table 3.1.

Mean slope coefficients between the mean causal rating and mean temporal estimate/reproduction errors, per condition, for experiments 1 and 4. Columns show the mean slope coefficients for the data included in the analysis, and the data including all conditions.

	Mean slope coefficient (analysis data)	Mean slope coefficient (all conditions included)
Experiment 1	-1.14 (2.52)	-2.20 (3.67)
Experiment 4	-0.43 (6.73)	-1.55 (5.39)

Note: standard deviations appear in parentheses.

The mean slope coefficients per experiment, in addition to the mean slope coefficient across experiments 1-6 can be seen in Figure 3.21, below. Negative mean slope coefficients were found in experiments 1-4, whereas positive slope coefficients were found in experiments 5 and 6.

As negative slopes are of theoretical interest here, one sample tests were carried out on the slope coefficients of each experiment to test for significant deviations from 0. In six of the seven comparisons the assumption of normality, as tested by the Shapiro-Wilk test of normality, was not met ($p < .05$). As such, Wilcoxon signed-rank tests were used for all comparisons. Significant negative slopes were found in experiment 1 (Z = 111, $p = .006$) and experiment 2 (Z = 131, $p = .03$). However, no significant negative slopes were found in experiments 3 (Z = 154, $p = .60$), 4 (Z = 159, $p = .35$), 5 (Z = 369, $p > .99$) or 6 (Z = 369, $p = .97$). Likewise, a test including all experiments found no significant negative slope (Z = 6,916, $p = .55$).

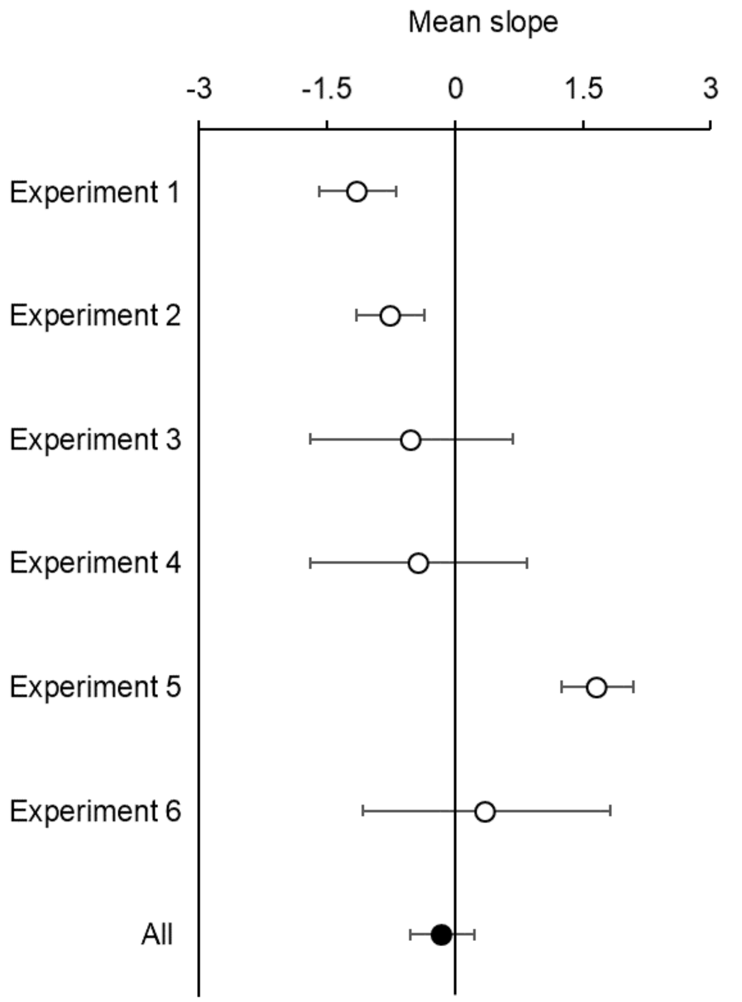

Figure 3.22. Mean slope coefficients by experiment. Error bars represent 95% confidence intervals.

Although significant negative slope coefficients were found in experiments 1 and 2, no such effects were found in experiments 3-6 or in the analysis of the combined data from all experiments. As was the case in other analyses of these experiments, no clear evidence for temporal binding was found. It should be noted that while this analysis better accounts for individual differences in visual impressions of causality, it does not take into account the multiple factors included in the ANOVA analyses. Nevertheless, in combination, both sets of findings suggest no clear effect of causal impressions of interval estimation errors, contrary to the predictions of causal accounts of temporal binding if temporal binding occurs in phenomenal causality. Agency accounts of temporal binding would predict neither a negative nor positive slope in experiment 1-5, as no relationship between perceived causality and reproduced intervals would be expected in the absence of intentional actions. However, in experiment 6, due to inclusion of intentional action conditions in three of the nine conditions, a relationship between perceived causality and reproduced intervals would be expected with no relationship in the other six conditions. Overall, therefore, this would result in a negative slope. Contrary to this prediction, a positive mean slope coefficient was found in experiment 6. Overall, based on the predictions of both causal and agency accounts of temporal binding, no evidence of temporal binding in phenomenal causality was found.

3.11. Experiments 1-6: discussion

Experiments 1-6 made use of a variety of two-dimensional animations in order to investigate temporal binding in phenomenal causality. Several animation types, including novel stimuli, were identified as eliciting reliably high, middling or low causal impressions. The first three experiments did not find evidence of temporal binding occurring due to visual impressions of causality. Following experiments 1-3, several amendments were made to the design including the increase of the number of temporal reproduction trials to 40 per condition. Although experiment 4 found some evidence for temporal binding, this was not replicated in experiments 5 and 6. Overall, the findings of experiments 1-6 point to a lack of temporal binding in the stimuli used here.

One possibility, that these findings were due to an absence of intentionality or agency, was investigated in experiment 6. No evidence for temporal binding was found, regardless of agency and perceived causality, suggesting that a lack of perceived agency did not lead to the lack of temporal binding in experiments 1-5. The inclusion of a simulated self-action condition ruled out perceptual differences between human movements and computer-generated movements as causing the lack of temporal binding.

If a lack of perceived agency cannot account for the absence of temporal binding in experiments 1-6, two possibilities remain: that these findings indicate that temporal binding is not affected by visual impressions of causality, or that the findings came about due to the stimuli used. In experiment 4 the gap stimuli were obscured to eliminate any effects of the gap stimuli other than visual causal impressions. Although the two gap sequence directions were presented in separate blocks, and gap objects were visible at the beginning of each trial before being occluded, participants reported causal impressions comparable to those of backward gap sequences. As causal ratings did not significantly vary between occluded gap sequences, it cannot be determined whether the visual features of the gap sequences had eliminated a possible temporal binding effect. Nevertheless, experiment 4 yielded other noteworthy findings. It appears that the visual cue to generative transmission must be seen in order to preserve causal impressions across temporal and spatial gaps. As in experiment 1, the lack of a gap sequence resulted in higher temporal estimates, indicating that the presence of the colour change sequence affected the perceived interval length, regardless of whether it affected causal impressions. It cannot be ruled out that this could have eliminated a temporal binding effect that would otherwise have been observed, although it is unclear why this might be the case.

Experiment 5 employed a different manipulation whereby on some trials the “collision” and “launch” were replaced the disappearing of the two launching objects in sequence, on either side of the spatial gap (signal animations). These animations were intended to lead to low causal impressions and therefore be able to rule out an effect of differences in eye movement or between forward and backward gap sequence animations. Surprisingly, participants reported high causal ratings when signal animations contained forward gap sequences. While the experiment did not rule out any effects of difference in shifts in attention, it provided further evidence for a lack of temporal binding in phenomenal causality.

The results of experiments 4-6 found additional evidence for a lack of temporal binding in phenomenal causality, while providing evidence against a lack of agency as a possible explanation. Experiment 4 demonstrated a clear effect of a gap sequence on reproduced intervals, suggesting the most credible alternative explanation for the findings is that temporal binding had been eliminated due to the regular sequence of colour changes. The onset of both the cause and outcome events were entirely predictable by external visual cues, in contrast with other temporal binding studies in which the exact onset of the outcome may not be predicted as precisely. In the majority of temporal binding studies, for instance in Haggard et al.'s 2002 studies, the outcome may be predicted by the time of the action. However, this takes place following delay and therefore, although a fixed delay is objectively very predictable, participants are reliant on their subjective perception of time and predictions of the delay duration based on prior trials. Due to the statistical noise present in time and event perception, as discussed previously, it is possible that perceptual processes are affected by additional prior assumptions such as temporal contiguity in causality, as suggested by Eagleman and Holcombe (2002). In the majority of animations in experiment 6, however, external visual cues to the onset of the "launch" are present in the form of the colour change sequences. Because the colour change sequences took place at a constant speed, they were also a reliable cue to the portion of the delay which had already elapse. This means that the delay duration need not necessarily have to be predicted in advance in order to accurately predict the onset of the "launch". This may have led to a lesser reliance on other cues, such as causality and agency, in judgements of interval length.

It is also possible that, despite instructions to fixate on the fixation cross, participants made saccadic eye movements in backward gap sequence trials, leading to a compression of the perceived interval length on those trials. The experiments reported in Chapter 4 investigated the possible role of other factors, not tested in experiments 1-6. Experiment 7 was an investigation of temporal binding due to visual events, using similar measures to those used in experiments 1-6. In addition, the effect of external visual cues to the onset of the outcome event and the duration of the gap interval was tested in experiment 8.

4. Chapter 4: Investigations of the Roles of Causality and Predictability in Temporal Binding

As discussed in chapter 3, the lack of temporal binding in experiments 1-6 does not necessarily reflect an absence of temporal binding in phenomenal causality. Experiments 7 and 8 differed from the other studies described in this thesis in being investigations of temporal binding in inferred causality. These were run in order to test some alternative hypotheses which may account for the findings of previous experiments. By process of elimination, the findings of experiments 7 and 8 provide a better understanding of which explanations best account for the findings of experiments 1-6. In order to do this, two temporal binding investigations were carried out using inferred causality in order to test whether the introduction of features such as visual stimuli or manipulations of predictability affect the findings.

Experiment 7 made use of self-causal, machine-causal (causal but not intentional) and non-causal sequences of events to further investigate whether temporal binding may occur in the absence of agency. Novel apparatus were designed to ensure that key features of experiments 1-6 were present. The experiment made use of interval reproduction as a measure of perceived interval length. Participants had only visual cues to the onset of both judged events regardless of causality and agency, as in launching animations, including when intentional actions were performed. In contrast with the majority of temporal binding studies, intentional actions did not result in tactile, proprioceptive or auditory feedback at the moment of the first judged event. Similarly, the predictability of the first event in each judged sequence was equal in both causal conditions regardless of agency. The onset of the first event in non-causal sequences was controlled to be as predictable as possible, although it does from causal conditions in that, despite being objectively as predictable, there were no external visual cues to the onset of the first judged event. Instead, the sequence was continuous, such that the first event in each sequence occurred after a fixed delay following the preceding event. Here, therefore, the predictability of the first event in sequence has been increased in non-agency conditions compared with previous studies comparing agency and non-agency conditions. If either predictability of the first event in each sequence or the absence of non-visual sensory feedback had affected the findings of experiments 1-6, therefore, the same lack of temporal binding would be observed in the results of experiment 7. A second aim of experiment 7 was to further investigate whether temporal binding can occur in the absence of agency. To date, this question had not been resolved and has received little attention (see Chapter 2, Section 2.3).

Experiment 8 investigated the effect of external visual cues to the onset of the outcome on temporal binding. Using a more typical temporal binding task, the predictability of the onset of the tone was manipulated between conditions to investigate its effect on outcome binding. Here, a continuous visual cue to the onset of the tone was used as an analogue to the colour change sequences used in previous experiments. While experiments 1 and 4 showed that the presence of a gap sequence shortened the perceived durations of delay intervals compared with empty temporal gaps, it is not clear the presence of a gap sequence had additionally eliminated temporal binding. Experiment 8 focussed on the effect of predictability on outcome binding specifically, as it is the predictability of the outcome and the overall duration of the delay interval that are affected by the gap sequences in experiments 1-6, in contrast with other temporal binding studies.

In summary, experiments 7 and 8 were designed to be able to eliminate several possible accounts of the previous findings between them, namely the predictability of the time of the cause and outcome, the use of the visual modality alone and the absence of inferred causality and intentionality (with the exception of experiment 6). These were investigated as they were potential explanations for the findings of experiments 1-6 not ruled out by previous findings.

4.1. Experiment 7

Experiment 7 was carried out in part as a direct comparison of the predictions made by causal and agency accounts of temporal binding. Some studies have found direct evidence for temporal binding due to causality alone (e.g. Buehner, 2012; 2015), while there is a general consensus that causality must be present for the effect to occur (see Chapter 2, Section 2.3 for a discussion of the role of causality in temporal binding). However, at the time of writing there is a notable scarcity of research directly investigating the roles of intentionality and causality in temporal binding - whether either or both are necessary or sufficient for the effect to occur. This question is central to the research previously described in this thesis. Whether temporal binding results from causality alone determines the interpretation of the findings of experiments 1-6. The reasons for temporal binding not occurring in phenomenal causality may differ, depending on which theoretical position best accounts for temporal binding.

Experiment 7 further eliminated possible confounding variables, allowing a better comparison with experiments 1-6. All events presented to participants were visual in nature, with no other sensory feedback corresponding to their onset. As in launching animations, the onset of the first event in each sequence was equally predictable regardless of causality and intentionality. As such, an absence of temporal binding would suggest that the use of the visual modality, or the predictability of events may have resulted in the absence of temporal binding in phenomenal causality. It should be noted that some temporal binding experiments had been carried out using visual outcomes (e.g. Ruess, et al., 2017; 2018). However, Ruess et al. (2017) found evidence for a decrease in temporal binding when the outcome event was visual. It is possible that a similar effect would be found when the preceding event is visual in addition to the outcome.

Novel apparatus were designed in which the typical keypress-tone sequence had been replaced with a light sensor which responded to a laser beam pointed in its direction by switching on an LED bulb (see the materials section for more details). In self-causal trials participants allowed the laser to pass through to the light sensor using a perforated wooden paddle. Participants moved the paddle in the path of the laser beam on each trial, allowing the beam to briefly pass through the perforation. In machine-causal trials, a wheel with a perforation equal in size to the perforation in the paddle rotated continuously, allowing the laser beam to pass briefly through once per rotation. In non-causal trials, the laser was replaced with a small red LED bulb, which switched off once per trial. In all trials, a light mounted on top of the light sensor switched on following a random short delay after the laser beam reached the light sensor (causal conditions) or following the switching off of the red LED (non-causal condition.

Experiment 7 made several improvements on previous studies of the role of causality in temporal binding. Two key drawbacks of past research were discussed in Chapter 2: the lack of non-causal control stimuli and the lack of measurement of causal judgements. The first issue is present in the majority of studies reporting diminished or absent temporal binding in the absence of agency. The absence of non-causal, non-intentional control conditions results in ambiguous findings, whereby a reduction in or complete absence of temporal binding cannot be distinguished by differences in temporal estimates. A non-causal control condition is crucial in ruling out the possibility of an “intentional boost” to temporal binding, as discussed previously. The absence of causal judgement data further muddies the waters, as the possibility that causal inferences may differ between intentional and non-intentional causal events cannot be ruled out. Likewise, non-causal controls might well lead to unintended causal inferences made by participants. Past temporal binding experiments had generally been carried out under the assumption that participants will perceive the causal structures intended by the experimenter, whether these were actual or illusory.

Interval reproduction was used to collect estimates of delay durations, and a debrief questionnaire was used to collect causal ratings after the experiment had ended. The experiment was designed to test 4 hypotheses, based on current accounts of temporal binding. Under causal accounts, reproduced intervals were expected to be lower in both causal conditions compared with the non-causal control condition (self-causal = machine-causal < non-causal control). Under agency accounts, lower reproduced intervals were expected in the self-causal condition, compared with the machine causal and non-causal conditions (self-causal < machine-causal = non-causal control). Finally, based on the findings of Buehner (2012; 2015), an “intentional boost” hypothesis was tested, in which temporal binding would be increased due to intentionality, while still present when comparing the machine-causal and non-causal conditions (self-causal < machine-causal < non-causal control). This is of particular interest as Buehner (2012; 2015) found inconclusive evidence for the possibility of such an intentional boost, which has not been investigated since. The fourth was the null hypothesis, which predicted no effect. Figure 4.1 below illustrates the predictions of each model. The design and hypotheses of this experiment were intended primarily for a Bayesian analysis comparing the relative likelihood of all 4 hypotheses based on the findings.

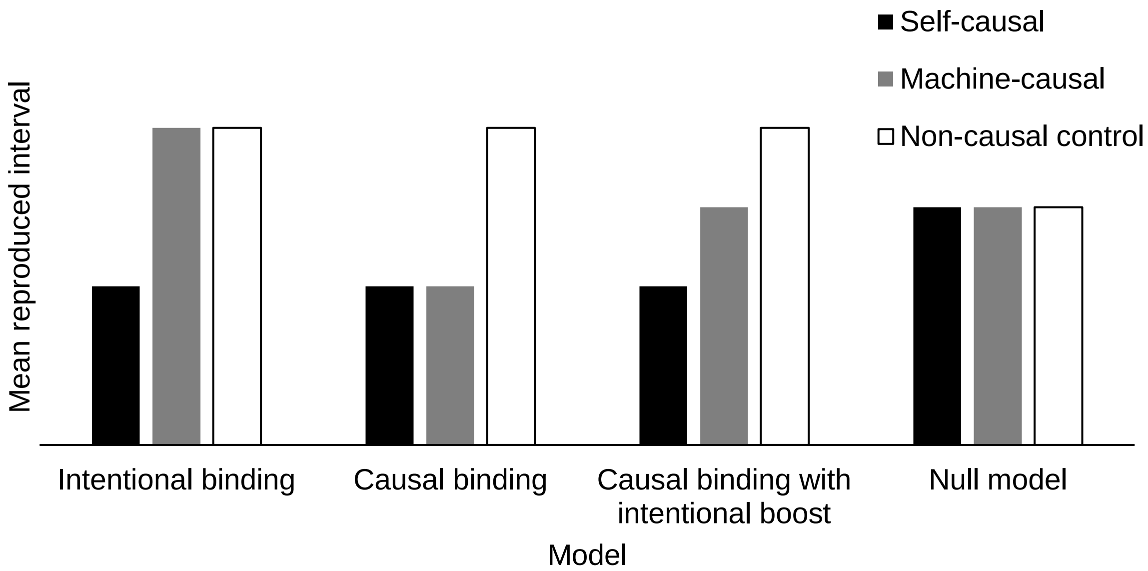

Figure 4.1. The patterns of findings predicted by each model tested for in Experiment 7 (mean reproduced interval by condition).

4.1.1. Methods

4.1.1.1. Participants

Thirty Cardiff University students and staff (2 male, age range 18-33) participated in exchange for a payment of £3 or course credits. Participants were recruited using Cardiff University's Experiment Management System.

4.1.1.2. Apparatus and materials

A schematic diagram of the apparatus can be seen in Figure 4.2. The laser and light sensor modules (see description below) were situated on a platform placed on top of a desk. The platform was 9.80cm in height, with the laser beam passing at a height of 14.50cm above the desk. The two modules were separated by a horizontal gap of 18.80cm. A wheel (21.50cm diameter), with a round 1cm diameter hole positioned in the location through which the laser beam passed, was placed between the two modules. The wheel was attached to a motor, housed within the platform, which allowed it to spin clockwise at a speed of approximately one revolution per four seconds.

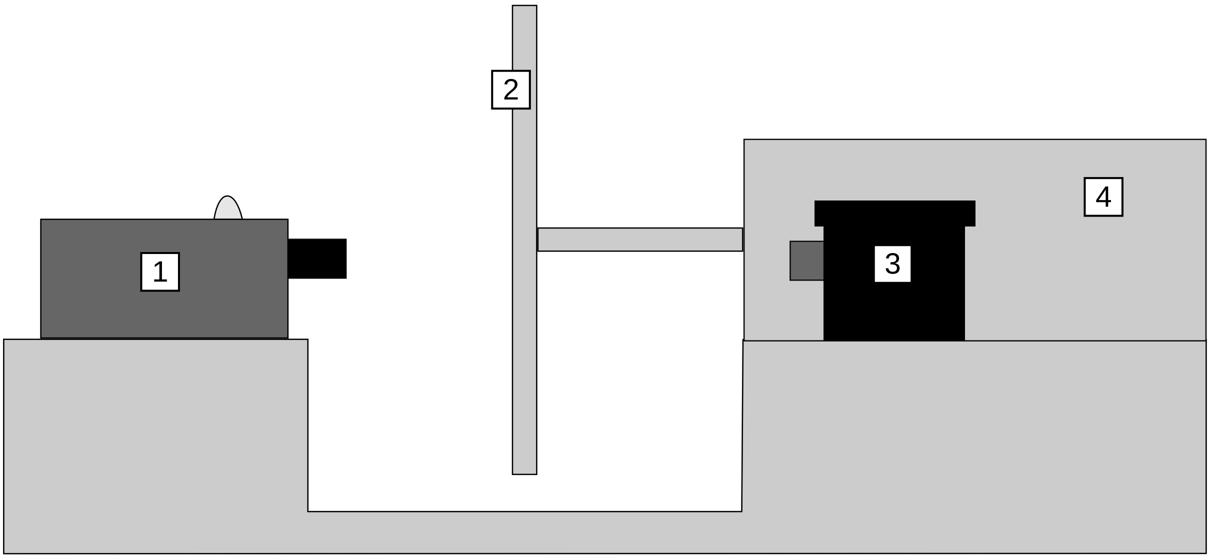

Figure 4.2. A schematic diagram of the apparatus used in experiment 7. A light sensor module (1), containing a Raspberry Pi computer and LED bulb was positioned across from the laser module (3). The rotating wheel (2) was positioned between the light sensor and laser modules and controlled by a geared motor (4), housed inside a box adjacent to the laser module.

The light sensor module consisted of a 7x7x10cm box housing a Raspberry Pi computer, with a 1cm diameter LED bulb mounted at its top and the light sensor on its front ,facing the laser module. A separate, portable, 5mm red LED bulb was also connected to the computer, but only visible to participants during the non-causal condition (see design and procedure). For the self-causal condition (see design and procedure), a rectangular wooden paddle (6cm in width and 14cm in height, with handle at its centre) with a 1cm diameter hole was used to allow the laser beam to pass to the light sensor, while the wheel position was fixed with the perforation placed in the path of the laser beam.

Participants were placed at a chin rest behind the laser module. Participant responses were recorded using a computer mouse connected to a separate computer. Finally, a debrief questionnaire was used to measure perceived causality using a 9-point Likert Scale. For each condition, participants were presented with the question "in the condition where [condition description] did it seem like [first event] was causing [second event] (1 = definitely yes, 5 = not sure, 9 = definitely no)?"

The Raspberry Pi computer placed inside the light sensor module was used to control the light sensor, both LED bulbs and the geared motor. This programme was created in Python 2.7. The data collection programme running on the second computer was programmed and run using the standalone PsychoPy application (Peirce, et al., 2019).

4.1.1.3. Design and Procedure

Participants were tested individually. After completing a consent form, participants were given safety instructions and were allowed to adjust the height of their seat. Instructions were presented verbally at the beginning of the experiment and before each block of trials. Throughout the experiment participants kept their head in the chin rest, ensuring that the light sensor, wheel and laser beam were visible. Participants were instructed to fixate their gaze on the laser point during the self-causal and mechanical-causal conditions, and on the 5mm diameter LED bulb during the non-causal control condition.

Participants took part in three conditions, in separate blocks, with order of conditions counterbalanced between participants. Each condition consisted of 40 trials, during which participants observed a critical two-event sequence lasting for an interval between 200 and 400ms (randomized, described below) and were asked to reproduce this interval by holding down the left mouse key for their perceived duration. Prior to each experimental block, participants completed as many practice trials as they needed (minimum: three, regardless of performance) to understand the task. Task comprehension was assessed by the experimenter by observing the participants performing the task to ensure that participants were performing the correct movement (if any) and reporting time intervals after each trial. Probing questions were used to ensure that participants were reporting the correct time intervals and that they did not have any further questions.

The conditions were as follows (see Figure 4.3 for a photographs of each experimental condition):

Self-causal: Participants performed an intentional action that generated a causal consequence after a short delay. The wheel was placed with the hole aligned to the laser beam and light sensor and remained stationary throughout (i.e. the laser beam could pass through to the light sensor, when allowed through by the participant). The light sensor responded to the laser beam by switching on the 10mm red LED at the top of the housing after a randomised delay of 200-400ms, and switching off after the same randomised interval once the laser beam was no longer received. Participants were told that the sensor responds to the beam after a delay, and this was demonstrated by the experimenter prior to the practice trials by using hand movements to either block the laser or allow it through. Participants were not told any additional information about these delays. Participants were instructed to place the paddle at the bottom of the apparatus, with the hole beneath the laser beam, such that the paddle blocked the beam. Participants were instructed to keep the paddle positioned adjacent to the wheel and move it upwards in front of the laser beam in each trial, such that the laser would pass through the hole. This was done to keep this condition as perceptually similar as possible to the mechanical-causal condition (see below). Participants were instructed to reproduce the time interval between the laser beam reaching the light sensor and the LED lighting up before placing the paddle back for the next trial.

Mechanical-causal: The wheel rotated continuously at a speed of approximately 4 seconds per revolution and blocked the laser beam from reaching the sensor, except when the hole came in line with it (once every 4 seconds). The light sensor was switched on and functioned in the same way as in the self-causal condition. This was demonstrated prior to the practice trials; the experimenter demonstrated that when the laser beam was blocked the light sensor did not respond at all, regardless of the position of the wheel, and that the light sensor always responded after the laser passed through the hole in the wheel. Participants were instructed to reproduce the interval between the laser reaching the sensor and the LED lighting up as in the self-causal condition. Note that in both the self-causal and mechanical-causal conditions, the critical causal event 1 (the laser reaching the light sensor) coincided with the perceptual experience of the laser spot (temporarily) being no longer visible against the paddle or wheel.

Non-causal control: Participants reproduced the interval between two sequential LED flashes. The wheel was positioned in the same way as in the self-causal condition. The laser module was switched off, and the 5mm LED was placed in the hole in the wheel. At the beginning of each trial, the 5mm LED switched on for one second before switching off, followed by the 10mm LED at the top of the housing switching on for 200-400ms. Following this, participants were asked to reproduce the time interval between the 5mm LED switching off and the 10mm LED switching on. Participants were not told any information about the causal relationship between the two lights, but only that they turned on and off in a regular sequence. In order that the switching off of the first light would be equally predictable as the laser passing through the wheel in the mechanical-causal condition participants were informed that the first light will switch off after exactly one second on each trial. It should be noted, however, that while this is objectively as predictable, here participants are reliant on internal cues – the perceived duration of time which had passed, rather than external visual cues, as in the two causal conditions. This sequence repeated automatically for the duration of the condition, with an overall trial length matching the duration of a single wheel revolution. Participants were instructed to fixate their gaze on the 5mm LED bulb throughout.

At the end of experiment participants were asked to fill in the debrief questionnaire, where they were asked to report whether they believed the first event caused in the interval they were judging caused the second event to occur, per condition. These causal ratings were taken as a manipulation check, to ensure participants correctly perceived the causal structure of the self-causal and mechanical-causal conditions (the laser beam causing the light sensor to respond) and the non-causal control condition (both lights shared a common cause). Following this, participants were debriefed as to the purpose of this experiment.

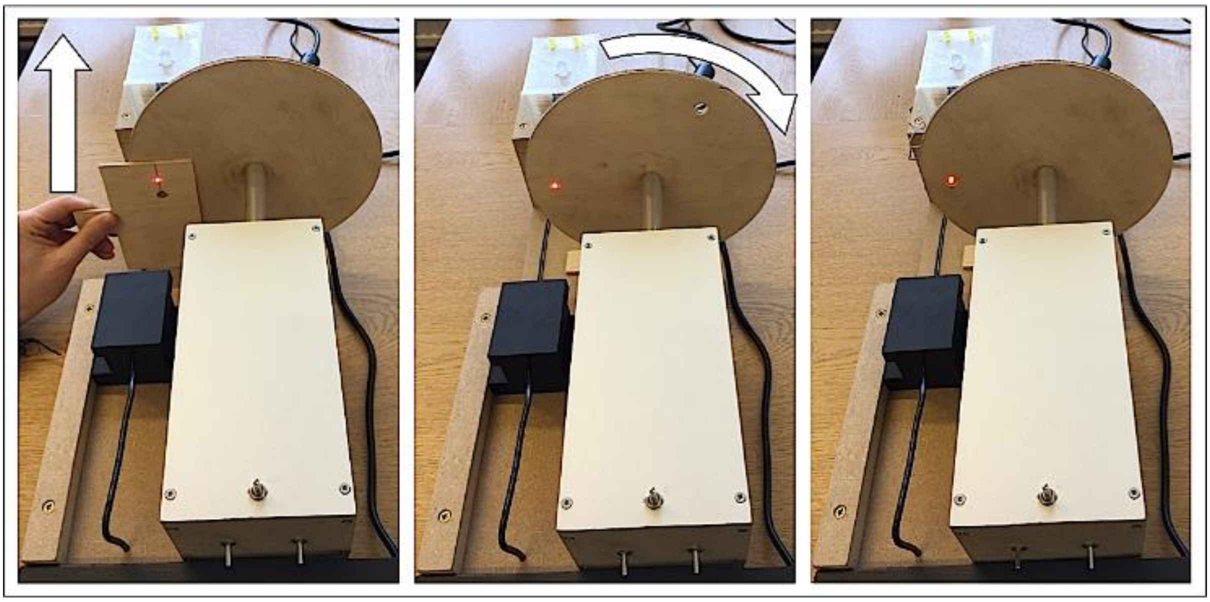

Figure 4.3. Photographs of all experimental conditions from the participants' perspective. Self-causal condition (left): the paddle is set with the hole below the laser beam at the beginning of a trial. Mechanical-causal condition (centre): the wheel is rotating clockwise and the laser beam is obstructed. Non-causal control (right): the laser beam is replaced with a red LED bulb positioned where the laser point can be seen in the other two conditions.

4.1.2. Results

4.1.2.1. Exclusions

Three participants were excluded for failing to follow instructions (consistently making multiple estimates per trial, or making estimates during, rather than between, trials). One further participant was excluded due to a technical error. For all other participants, individual trials for which there were two estimates and estimates which overlapped with the time of the event being judged were removed from analysis (8 participants with excluded trials, mean average 4.88 exclusions out of 120 trials).

4.1.2.2. Causal ratings

The distribution of causal ratings can be seen in Figure 4.4. While median causal ratings were above the mid-point of the scale in all conditions, causal ratings are were higher for the self-causal and machine-causal conditions than for the non-causal condition. A Friedman's ANOVA was used due to the ordinal nature of the causal scores. The analysis found a significant main effect of condition on causal ratings ($X^2(2) = 15.58$, $p < .001$). Post-hoc testing using Bonferroni corrections found significantly lower scores for the non-causal control condition (median = 6) compared with the self-causal condition (median = 8, Z = 24.50, $p < .001$) and the mechanical-causal condition (median = 8, Z = 37.00, $p = .002$). No significant difference was found between the self-causal and mechanical-causal conditions (Z = 87.5, $p = .95$).

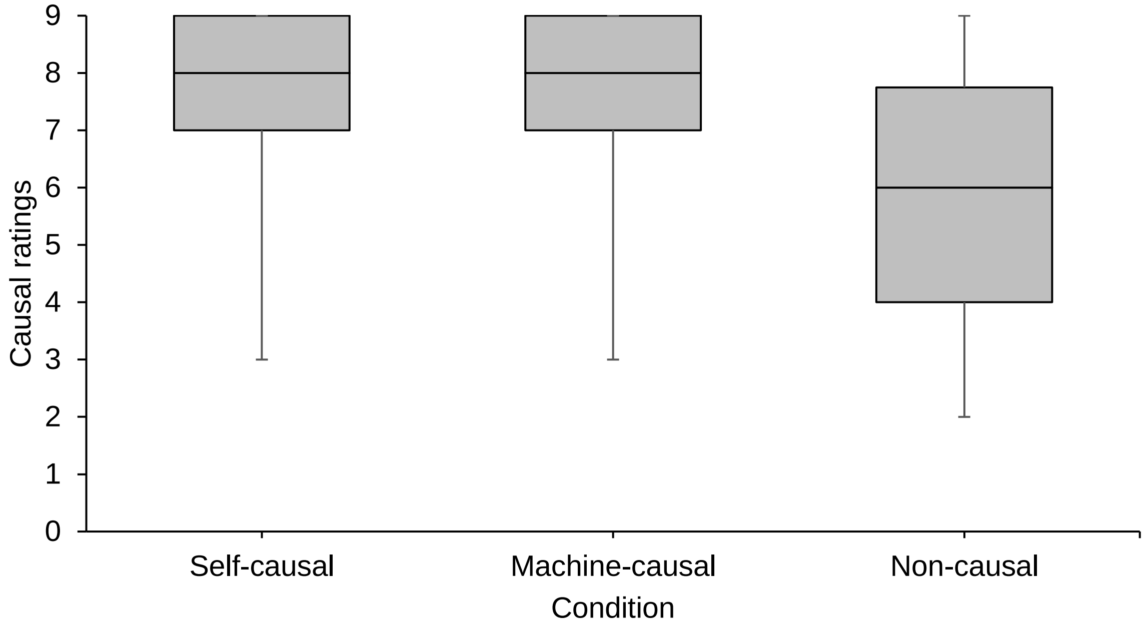

Figure 4.4. A box plot of causal ratings by experimental condition.

4.1.2.3. Temporal estimates: transformation

A preliminary analysis of the data found substantial variability in the range of interval reproductions between participants (see Table 4.1 for pre-transformation data). Additionally, a Shapiro-Wilk test found significant deviations from the normal distribution in two of the three conditions ($p < .05$). In order to reduce the influence of individual differences and reduce the positive skew of the data, temporal reproductions were converted to z-scores. To do this, each participant's grand mean reproduced interval was subtracted from each reproduction. The difference from the mean of each score was divided by the standard deviation of all estimates. The mean z-score per condition for each participant was used for the temporal reproduction analysis. Following transformation, the assumption of normality was met for all conditions ($p > .05$). The mean z-scores can be seen in Figure 4.5.

Descriptive statistics for untransformed temporal reproductions.

Table 4.1.

Descriptive statistics for untransformed temporal reproductions.

Condition	Mean (ms)	Standard deviation (ms)
Self-causal	380.26	197.49
Mechanical-causal	406.8	134.81
Non-causal Control	501.14	315.39

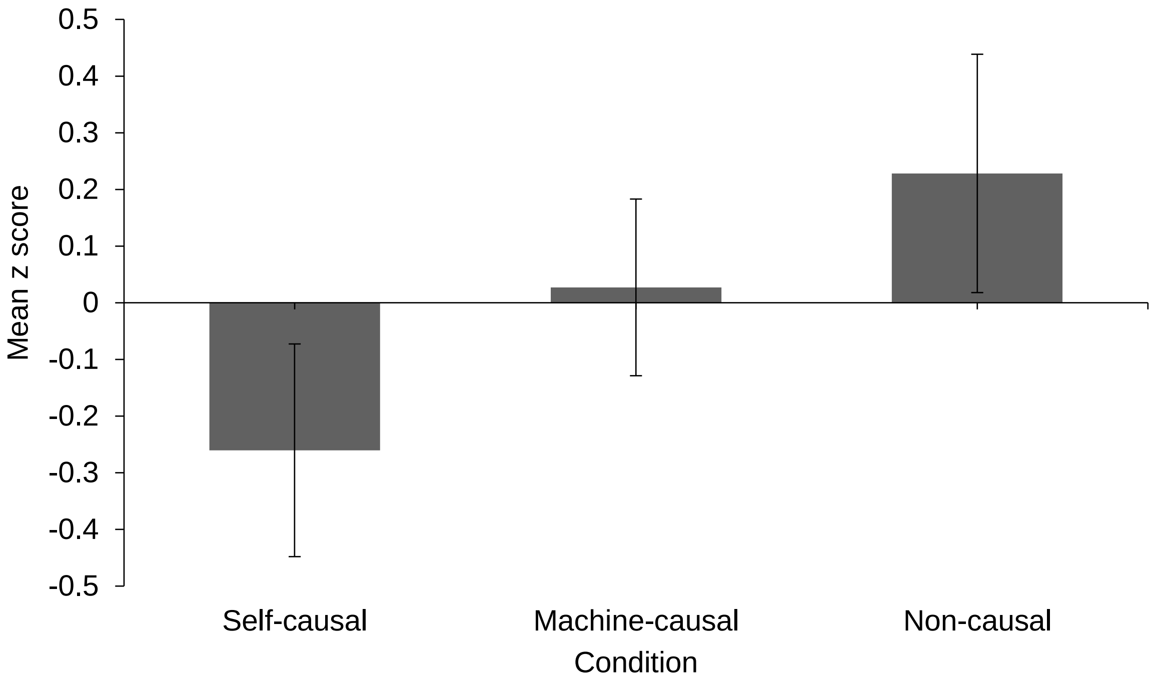

Figure 4.5. Mean z scores of temporal reproductions by condition. Error bars represent 95% confidence intervals.

4.1.2.4. Temporal estimates: analysis

A Bayesian analysis was carried out using the BayesFactor package for R statistics (Morey, Rouder, Jamil & Morey, 2015). All Bayes factors reported here used the null model as the denominator (self-causal = mechanical-causal = non-causal control). A Bayesian repeated-measures ANOVA (see Rouder, Morey, Speckman & Province, 2012 for details) found a Bayes factor of 17.23 for the unconstrained model (self-causal ≠ mechanical-causal ≠ non-causal control), indicating that the unconstrained model is over 17 times more likely given the data than the null model. Three further models were analysed, as predicted by the agency accounts of temporal binding (self-causal < mechanical-causal = non-causal control), the causal account of temporal binding (self-causal = mechanical-causal < non-causal control) and the “intentional boost” hypothesis (self-causal < mechanical-causal < non-causal control). The highest Bayes factor was found for the model predicted by the intentional boost account (BF_{10} = 91.82), and as such it is the preferred model compared with the models predicted by agency accounts (BF_{10} = 44.57) and the causal account (BF_{10} = 16.22).

A one-way frequentist ANOVA was carried out to confirm these findings. This analysis found a significant main effect of condition on the z score-transformed reproductions, $F(2,50) = 4.47$, $p = .02$, $\eta^2 = .15$. Planned simple contrasts were used to investigate the differences between the mechanical-causal condition and both other conditions. A significant difference was found between the mechanical-causal and self-causal conditions ($F(1, 25) = 4.33$, $p = .048$, $\eta^2 = .15$), but not between the mechanical-causal and non-causal control ($F(1, 25) = 1.53$, $p = .23$, $\eta^2 = .06$). The frequentist analysis, therefore, appears to favour the intentional binding account. It should be noted, however, that the non-significant difference between the mechanical-causal condition and non-causal control condition may indicate inconclusive evidence rather than evidence against the effect.

4.1.3. Discussion

Experiment 7 compared predictions made by both agency and causal accounts of temporal binding with the use of novel apparatus. In contrast with other temporal binding research, participants had access to visual feedback alone at the time of both and first and second judged events, regardless of causality or agency. Causal ratings suggest that while participants reported higher causal judgements for self-causal and machine-causal actions compared with the non-causal 2-light sequences, causal judgements for the non-causal control condition were surprisingly high, with more than half above the mid-point of the ratings scale. This occurred despite the fact that the non-causal sequence was predetermined and took place continuously, without input from the experimenter following the initiation of the block of trials, and without explanation for how the switching off of the first light caused the second light to switch on. Here, the contingency between the two events appeared to contribute to incorrect inferences of the causal mechanism controlling the two events. This further illustrates the difficulty in interpreting past findings where no causal judgements were recorded, as participants' inferences of causal mechanisms may differ substantially from those intended.

Nevertheless, significantly higher causal judgements were recorded for both self- and mechanical-causal conditions compared with the non-causal control and, crucially, agency did not appear to result in an increase in causal judgements. The Bayesian analysis found the model best supported by the data to be of causal binding with an intentional boost, while the frequentist analysis was less conclusive, finding a significant difference in reproduced intervals between the mechanical- and self-causal conditions, but not between the non-causal control and mechanical-causal conditions. It should be noted that the frequentist ANOVA tested two individual hypotheses in isolation while the Bayesian analysis allowed the comparison of entire models consisting of multiple comparisons. As such, this may indicate that the frequentist findings lacked the statistical power to detect a small causal binding effect, rather than an absence of causal binding. This may have occurred due to smaller-than-expected differences in causal judgements between non-causal and causal conditions resulting in a smaller effect size.

The notion of an "intentional boost" to causal binding has received little attention to date and is difficult to account for using either causal or agency accounts of temporal binding. Experiment 7 did not find evidence for this boost occurring due to an increase in certainty of the causal mechanism (as hypothesised by Eagleman & Holcombe, 2002), as causal ratings did not significantly differ between these two conditions. It cannot be ruled out, however, that an "intentional boost" could have resulted from low-level perceptual differences between the tasks and, in particular, the presence of hand movements during intentional trials may have effected interval perception due to changes in internal clock speeds. In addition to the previously discussed effect of saccadic eye movements on time perception (Morrone et al., 2005), evidence has been found for a compression in the perceived time interval between two sensory events during hand movements (Tomassini, Gori, Baud-Bovy, Sandini & Morrone, 2014). An “intentional boost” to temporal binding may therefore be a product of motor actions made during the judged events or interval between them. This “intentional boost” may also account for the differences in the magnitude of the temporal binding effect reported in investigations of temporal binding in observed actions (see Chapter 2, Section 2.3.2.2).

Regarding temporal binding in phenomenal causality, however, these findings suggest that temporal binding in the absence of agency is indeed possible; the Bayesian analysis found greater support for the causal binding + intentional boost model, although this model was found to be only 2.06 times more likely than the next-best-supported model predicted by agency accounts of temporal binding. These findings further demonstrate that multi-sensory feedback is not necessary for temporal binding to occur. This raises the question of why similar effects were not found in experiments 1-6. In particular, experiment 6 found no evidence for either temporal binding or an intentional boost, using the same sample size and interval reproduction task used in experiment 7. Experiment 7 demonstrates that tactile non-visual feedback at the onset of the judged events is not necessary for temporal binding to take place. A key feature of experiments 1-6 not investigated in experiment 7 was the presence of visual cues to the length of the interval and the onset of the outcome events. Experiment 8 sought to address this possible explanation.

4.2. Experiment 8

As mentioned previously, one possible reason for a lack of temporal binding in the stimuli used Experiments 1-6 is that participants had been able to use gap sequences to predict both the duration of the delay interval while the delay was taking place and the onset of the “launch” event. In launching animations with a gap sequence, the onset of the second event (launch) is signalled by the gap sequence, whereas in typical temporal binding experiments the onset of the outcome is only predicted by the first event, separated by a delay. This may have led to an elimination of any temporal binding if participants relied on this external cue in estimating delay durations, rather than predictions based on causal impressions. Under the causal account of temporal binding, this would reflect an increased weighting of sensory cues over prior assumptions of temporal contiguity, resulting in temporal judgements that are less influenced by prior assumptions.

Some studies suggest, contrary to this hypothesis, predictability increases rather than diminishes temporal binding (e.g. Cravo et al., 2011, Haggard et al., 2002). Other studies, however found no such effect, both the use of the Libet clock method (Ruess, et al., 2017) and measures of interval perception (Buehner & Humphreys, 2010; Humphreys and Buehner, 2009). Although the question of whether predictability increases the temporal binding effect remains unsolved, all evidence points to there either being no effect or an increase in temporal binding.

This evidence is limited in its application to the findings of Experiments 1-6, however. In the experiments mentioned above predictability varied due to the consistency of delay intervals between trials. In contrast, in experiments 1-6 delay durations varied between trials but were entirely predictable by the rate of the colour change sequences. This was a much more reliable cue to the onset of the outcome event as it did not require participants to rely on previous estimates of duration. Instead, the gap sequence was available as a visual cue on each trial.

Experiment 8 attempted to create equally predictable outcome stimuli, while eliminating other variables which may have affected judgements of delay duration. The experiment made use of the Libet clock method and investigated estimates of the time of outcome alone. The results of experiments 5 and 7 already suggest no effect of the predictability of the onset of the first event. In experiment 5, the onset of the first judged event was less predictable in signal than launching animations but did not result in significant differences in temporal estimate errors. Because of this, and as previous studies have shown outcome binding to account for the bulk of the temporal binding effect (e.g. Haggard et al., 2002), this experiment focussed on outcome binding alone.

The study made use of the Libet clock method and keypress-tone event sequences typical to temporal binding to ensure that conditions are in place to reliably replicate the temporal binding effect. In all conditions a white circle was present in the centre of the Libet clock. In "signal" conditions, the circle began to fill with a green colour, radiating outward from the centre of the clock from the beginning of each delay interval. In each signal trial, the circle at the centre of the clock became entirely green at the moment that the outcome event occurred (tone). This was compared to no-signal conditions in which no colour change took place. This sequence was designed to differ from the colour change sequences in experiments 1-6 to ensure the equal predictability of the tone, while eliminating any other factors which may affect the findings. This experiment made use of 500ms delay intervals to allow sufficient time for the participants to make use of the visual signal.

4.2.1. Methods

4.2.1.1. Participants

40 Cardiff University students (8 male, age range 18-27) participated in exchange for course credits or a payment of £5. Participants were recruited via the School of Psychology's experiment management system. Participants who took part in Experiments 1-7 were excluded from participation in this study. The number of participants was increased to 40 to account for a smaller effect resulting from the focus on outcome binding alone, rather than the entire delay interval.

4.2.1.2. Apparatus and materials

The experiment programme was run using PsychoPy (Peirce, 2019). The experiment was run on Mac-Mini computer. Stimuli were displayed on a monitor 47cm wide and 30cm in height, at a resolution of 1,680 by 1,050 pixels and a refresh rate of 60 frames per second.

All instructions were presented on the screen with a line height of 0.56cm (20 pixels), including the consent form and markers on the Libet clock.

This experiment made use of a Libet clock (Libet et al., 1983). See Figure 4.6 for an image of the clock used in this experiment. The clock face was presented at the centre of the screen with a radius of 5.60cm (200 pixels). Sixty minor tick marks were presented along the clock face, with major markers and labels (intervals of 5 from 0 to 55 present every 5 tick marks. A clock had extended from the centre to the edge of the clock face. The clock hand rotated at a speed of 2,560ms per revolution (7.11ms per degree, or 2.34 degrees per frame).At the centre of the clock was a white circle with a black outline, with an 84cm (30 pixel) radius, and a smaller, black circle at its centre (0.14cm, or 5 pixel radius). These were presented over the clock hand.

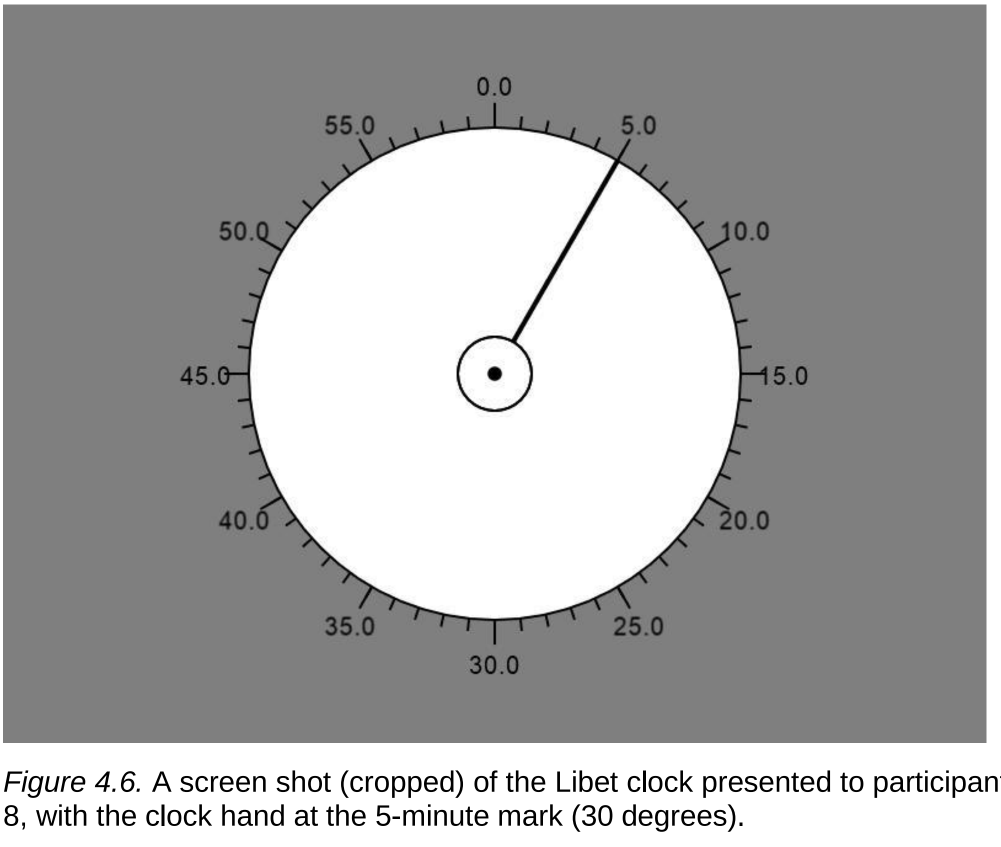

Figure 4.6. A screen shot (cropped) of the Libet clock presented to participants in experiment 8, with the clock hand at the 5-minute mark (30 degrees).

4.2.1.3. Design and procedure

The study used a repeated-measures, 2-way factorial design. Each factor contained two levels. Additionally, participants completed two baseline tasks. The Libet clock was present in all trials. The starting position of the clock hand was randomised for each trial. Participants were instructed to fixate their gaze on the centre of the clock during all trials. At the end of each trial the clock hand continued to revolve for a random interval between 1,500 and 2,500ms in order to prevent an afterimage of the clock hand position at the time of the tone, and to prevent participants from being able to infer where the clock hand was from its position at the end of each trial. All time intervals, with the exception of the length of the outcome tones, were determined in frames for optimal accuracy, so these intervals were always set in bins of 16.67ms.

In key-press (causal) trials participants were instructed to press the space key at a time of their choosing, which caused a tone (1,000hz, 50ms duration) to sound after a 500ms delay. Participants were instructed to press the key at a time of their choosing, but only after the clock had completed a full revolution. If this condition was not met, the trial would restart. This was done in order to prevent participants from attempting to press the key immediately after the trial began. Participants were also asked to press the key spontaneously, without planning in advance where the clock hand would be when it was pressed.

In two-tone (non-causal) trials participants performed no action. Instead, a tone (1,000hz, 50ms duration) sounded in place of a key-press. The first tone would sound at a random time between the time at which the clock hand completed a full revolution, and 2,566.67ms later (i.e. the end of the second revolution). The second tone would sound after a 500ms delay, as in the key-press trials.

In signal trials the centre of the clock began to change colour from white to green when the space was pressed (in key-press trials) or when the first tone sounded (two-tone trials). This colour-change radiated outward from the centre of the clock to the edge of the inner circle. This colour change took 500ms, such that the tone would sound when the inner circle changed colour completely. In no-signal trial the inner circle of the clock remained white throughout.

In addition, there were two single-tone baseline conditions. In both a tone sounded at a random time between 500ms after the clock hand completed its first full revolution and 500ms after the clock hand completed its second full revolution; this timing was identical to the time of onset of the second tone in two-tone trials. In signal baseline trials the clock hand changed colour gradually from white to green as in other signal trials (see description above). In no-signal baseline trials the inner circle of the clock remained white throughout.

There were six conditions in total: signal baseline, no-signal baseline, key-press + signal, key-press + no-signal, two-tone + signal, and two-tone + no-signal. As baseline measures were intended to be subtracted from the other conditions in the analysis, this was a two-factor design (key-press/two-tone x signal/no-signal). All participants took part in all conditions. The six conditions were presented in separate blocks of 5 practice trials and 40 experimental trials.

After each trial participants were asked to report the position of the clock hand at the time of the onset of the tone (in key-press trials) or second tone (in two-tone trials). Participants did so by entering a number between 0 and 60 (matching the interval markers on the clock) using the computer keyboard. A clock was present on the screen showing participants the clock hand position that matches their estimate.

Participants were tested individually. After completing an electronic consent form, instructions were presented on the screen prior to each block of trials. The trial types described above occurred in separate blocks of trials. The order of these blocks was randomised for each participant. At the end of the experiment participants were debriefed and given their payment.

4.2.2. Results

4.2.2.1. Exclusions

One participant was excluded from analysis due to a loss of data as result of a technical fault. Two participants were excluded from analysis due to outlying data in perceptual shifts (over 3.5 standard deviations from the group mean). No participants were excluded based on other pre-determined exclusion criteria. Participants were to be excluded if they made estimates different by over 90 degrees from the actual value on 16 or more trials per block. Participants were to be included in the analysis if they made estimates within 90 degrees of the actual value on 25 or more trials in each condition, corresponding to top 5 percentiles expected if they entered values at random.

4.2.2.2. Data cleaning

For each participant, each estimate was converted to degrees (multiplied by 6). Each estimate was subtracted from the corresponding actual angle of the clock hand to produce the estimate error. Negative values show underestimation and positive values show overestimation. As the clock is circular, but the angle of the clock hand can only be between 0 and 360 degrees, estimate errors over 180 degrees or under -180 degrees were corrected, under the assumption that participants' estimates fell within the correct half of the clock face. In estimate errors over 180 degrees 360 degrees were subtracted from the estimate error. For example, if the estimate was 354 degrees and the actual value was 6 degrees, it was assumed that the estimate error is -12, rather than 348 degrees. The reverse was done for estimates under -180 degrees.

For the purpose of analysis, estimate errors in degrees were converted to milliseconds. The values in degrees were multiplied by 7.11 to achieve this (2,560ms per rotation divided by 360 degrees, i.e. 7.11ms per degree). Both the mean standard error per participant per condition and the within-participant standard deviations were computed for analysis.

4.2.2.3. Estimate errors

The mean estimate errors and mean shifts can be seen in Table 4.2. The mean shifts represented the differences between two-event conditions and their single-event baseline equivalents. For each participant mean estimate errors from signal conditions were subtracted from the signal baseline estimate errors, and the no-signal estimate errors subtracted from the no-signal estimate errors. Positive mean shifts represent a forward shift relative to baseline and negative mean shifts represent a negative shift relative to baseline. Mean shifts were used for the main analysis.

The descriptive statistics show higher overestimation in the no-signal baseline condition than for the signal baseline. Overall there was a negative shift in both no-signal two-event conditions, and a positive shift in signal conditions. This appears to be the result of a lower baseline mean when the signal was present, as well as higher estimate errors in signal conditions. However, mean shifts were lower in causal conditions than no causal conditions across the board.

Table 4.2

Mean estimate errors and mean shifts by condition.

Condition	Mean error (ms)	Mean shift (ms)
Baseline (signal)	8.05 (79.01)	-
Baseline (no signal)	67.49 (58.88)	-
Causal (key-press) + signal	28.33 (59.46)	20.28 (52.67)
Causal (key-press) + no signal	20.88 (86.95)	-46.61 (77.48)
Non-causal (two-tone) + signal	70.57 (69.54)	62.52 (67.34)
Non-causal (two-tone) + no signal	34.93 (60.83)	-32.56 (59.02)

Note: standard deviations appear in parentheses.

The analysis included the shifts relative to baseline in all two-event conditions. These can be seen in Figure 4.7. While participants made greater overestimations overall when a signal was present, lower perceptual shifts can be seen in both causal conditions compared with non-causal conditions, with a greater difference between the two in signal trials.

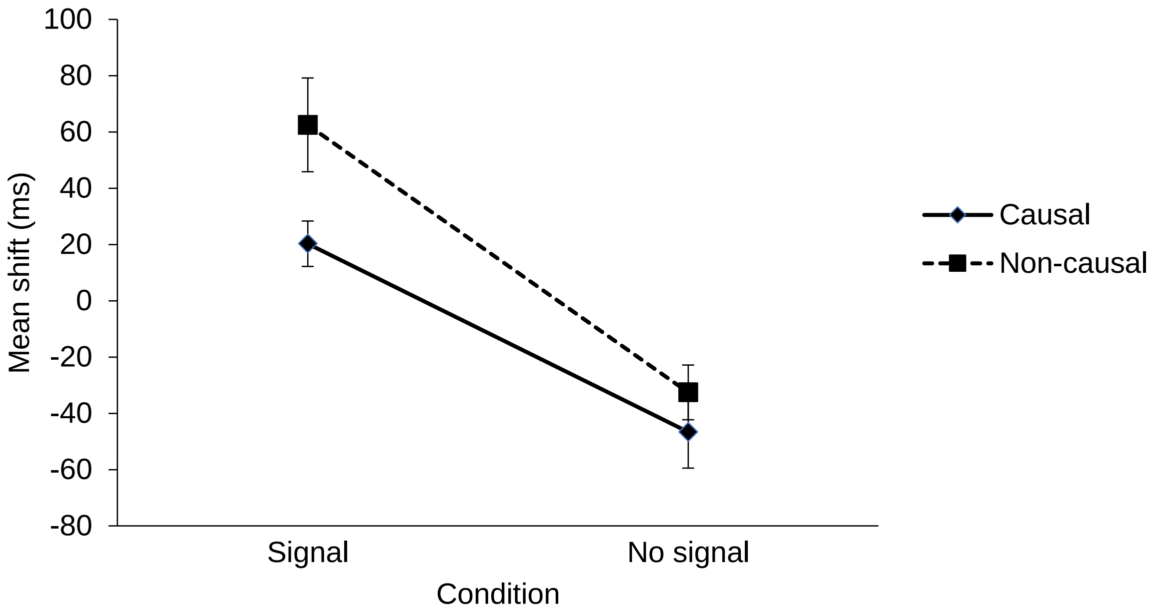

Figure 4.7. The mean perceptual shifts by causality and predictability. Error bars represent 95% confidence intervals.

A two-way, repeated-measures ANOVA found significant main effects of causality ($F(1, 36) = 15.58$, $p < .001$, $\eta_p^2 = .30$) and signal ($F(36, 1) = 27.72$, $p < .001$, $\eta_p^2 = .43$). Additionally, there was significant interaction between causality and signal ($F(1, 36) = 8.52$, $p = .006$, $\eta_p^2 = .19$). A simple effects analysis found a significant difference between the causal and non-causal conditions when a signal was present ($F(1, 36) = 22.88$, $p < .001$, $\eta_p^2 = .39$), but not in the absence of a signal ($F(1, 36) = 2.81$, $p = .10$, $\eta_p^2 = .07$).

4.2.3. Discussion

Experiment 8 made use of a temporal binding paradigm similar to that used by Haggard et al. (2002) and many temporal binding studies since. The experiment compared perceptual shifts in the perceived time of a tone either caused by the participant’s keypress or preceded by another tone, compared with single-event baseline measures. A further manipulation was included in which a signal was to the onset of the judged tone was present in half of the trials, whereby a circle at the centre of the Libet clock gradually changed colour from white to green from the end of the first event until the onset of the second event.

Results showed an overall positive shift in the perceived time of the tone in signal trials compared with no-signal trials, but also found a significant effect of causality/intentionality whereby forward shifts were significantly greater in non-causal trials compared with causal trials. This suggests temporal binding had taken place when a signal was present, despite the forward shift in all signal conditions. The lack of temporal binding in no-signal trials is unsurprising as some previous studies have found that the effects of temporal binding on event perception diminish at delay intervals longer than 250ms (e.g. Haggard et al., 2002; see Chapter 2, Section 2.2.1 for a discussion of the evidence for an effect of interval length on temporal binding), while this study made use of a 500ms interval.

While it is not clear from the analysis whether temporal binding had taken place in the absence of a signal, the presence of a signal providing a cue to the onset of the outcome (tone) appeared to strengthen, rather than diminish, the temporal binding effect. It should be noted that the use of a Libet clock limits the direct application of these findings to the previous experiments discussed in this thesis, which made use of interval estimation and reproduction methods. However, the perceptual shifts in event perception likely contribute to the overall compression in perceived delay intervals on top of differences in internal clocks. In addition - assuming that the temporal binding effect as observed in action binding, outcome binding and the compression of the perceived interval between the two results from similar processes - the effect of predictability on the weighting of prior expectations should be observed in all cases. This experiment found no evidence that the predictability of the outcome diminishes temporal binding and indeed found that the converse was true. The predictability of the outcome in experiments 1-6 is therefore not a likely explanation for the lack of temporal binding in phenomenal causality found in those experiments.

4.3. Experiments 7 and 8: discussion

Experiments 7 and 8 were conducted in order to test several hypotheses for the lack of evidence for temporal binding found in experiments 1-6. Experiment 7 tested hypotheses generated from causal and agency accounts of temporal binding with the use of visual stimuli and similar measures to those used in experiments 1-6. Experiment 8 tested whether external visual cues to the onset of the outcome event and the duration of the delay interval reduce temporal binding due to a decreased reliance on prior assumptions of causally related events as temporally contiguous.

Several methodological features of experiments 1-6 were applied to experiment 7. Visual cues alone were available to participants at the onset of causal events, outcomes and control events, including intentional actions. Delay intervals differed between trials and the interval reproduction procedure used in experiments 3-6 was employed. Strong evidence for temporal binding was found, indicating that the use of these methods cannot account for the lack of temporal binding in experiments 1-6. Causal ratings collected at the end of the experiment suggested that this evidence for temporal binding was found despite a smaller relative difference in causal ratings between conditions than was found with the use of phenomenal causality stimuli.

Experiment 7 further aimed to test agency and causal accounts of temporal binding to aid in the interpretation of the lack of temporal in phenomenal causality in experiments 1-6. Evidence was found for causal binding with an additional intentional “boost” whereby reproduced intervals were lower for the delays between causally related events compared with unrelated events, but lower still when intentional actions were performed. This outcome, although seen in other experiments (Buehner, 2012; 2015) is not entirely predicted by either agency or causal account, although it does suggest that agency is not necessary for temporal binding to occur. It cannot be ruled out, however, that this “boost” occurred due to the presence of motor actions, rather than agency per se.

Experiment 8 tested the hypothesis that external cues to the onset of the outcome may reduce temporal binding due to a decreased reliance on prior knowledge in making temporal judgements. A more typical temporal binding design was used, making use of keypress-tone event sequences and measuring outcome binding using a Libet clock. The Libet clock was chosen as the most well-established method by which temporal binding is measured, and to ensure minimal perceptual similarity between experiment 8 and experiments 1-6, with the exception of the predictability of the outcome event and the duration of the delay interval.

The shifts in the perception of the outcome events relative to the non-causal control condition were only significant when an external visual cue was present, indicating that predictability led to greater outcome binding, rather than a reduction in outcome binding. Although care must be taken when applying this inference to the effects of temporal binding on interval perception, there appears to be no general reduction in temporal binding due to external visual cues to the onset of the outcome, and, in fact, the opposite was found.

In summary, experiments 7 and 8 succeeded in eliminating several hypothetical, non-causal accounts as likely explanations for the lack of phenomenal causality found in experiments 1-6. Based on the findings of experiment 7, the design of experiments 1-6 – the number of trials, measurements, etc. – appear to be appropriate for detecting a temporal binding effect. The findings have further narrowed the range of possible accounts for the findings reported in this thesis.

5. Chapter 5: General Discussion

Temporal binding, the subjective contraction of the time interval between cause and effect, has been studied extensively since it was first reported in 2002 (Haggard et al., 2002). Although it was initially reported as the contraction between intentional actions and their outcomes, debate over the role of causality in the effect has generated some interest, with some suggesting the effect is driven entirely by the perception of causality and not intentionality or agency (Buehner & Humphreys, 2009; Eagleman & Holcombe, 2002). Perhaps surprisingly, given that the perception of causality is generally viewed as at least necessary, if not sufficient for the effect to occur (Moore & Obhi, 2012), the mechanisms by which causality contributes to temporal binding has remained under-explored. This thesis aimed to investigate whether temporal binding occurs in phenomenal causality, a subject which to date has only been investigated once (Cravo et al., 2009). Phenomenal causality refers to visual impressions of causality: these are fast and appear to be automatic, and emerge without the needed for prior experience of, or multiple exposures to, the stimuli used. Phenomenal causality has been suggested to result from separate processes from those leading to causal inferences (e.g. Schlottmann & Shanks, 1992; Scholl & Tremoulet, 2000). As such, evidence either for or against temporal binding in phenomenal causality has the potential to further our understanding of the role that causality plays in temporal binding and the mechanisms by which this occurs.

The difficulty in using phenomenal causality stimuli in temporal binding research is that stimuli leading to visual impressions of causality do not typically contain a temporal gap between causes and their effects and temporal delays between causes and their effects have been shown to reduce causal impressions (e.g. Michotte, 1946/63; Yela, 1952; Schlottmann & Shanks, 1992; White, 2014). To overcome this, Experiments 1-6 have made use of stimuli adapted from White's (2015) research, which showed that the inclusion of visual cues to generative transmission can maintain causal impressions across temporal delays. In all trials launching animations were used whereby a launcher object moved toward a target object. A second, target object began to move in the same direction as the launcher after a delay. To generate high impressions of causality, a series of objects between the stopping point of the launcher changed colour from grey to black in sequence from the direction of the launcher toward the direction of the target during the delay interval. As a control, other trials contained a reversed colour change sequence which has been found not to maintain causal impressions.

Experiments 1-6 made use of a number of variations on these stimuli to investigate whether temporal binding took place due to phenomenal causality, in the absence of agency. Participants made estimates of the delay duration using both interval estimation (experiments 1 and 2) and interval reproduction methods (experiments 3-6). Participants also provided causal ratings which were used to ensure that participants experienced the intended causal impressions. Experiments 1-6 found the causal impressions to be highly reliable. However, the prediction made by causal accounts of temporal binding – that if temporal binding had occurred, the higher the causal impressions, the smaller the perceived intervals should be – did not manifest. Analyses of the regression slope coefficients between causal ratings and interval estimate errors across experiments 1-6 did not find clear evidence for a relationship between causal impressions and interval estimation or reproduction errors.

Experiments 4-6 were carried out in part as replications of experiments 1-3 and in order to test alternative explanations for the findings. All three experiments reduced the number of gap sequence types and animation speeds to two (forward and backward gap sequences and fast and slow animation speeds, corresponding to short and long delays). The number of interval reproduction trials per condition was increased to 40 to increase the reliability of estimated intervals. Experiment 4 obscured the gap stimuli on half of the trials, with forward and backward gap sequences presented in separate blocks. These occluded stimuli were intended to yield similar causal impressions to fully visible trials, thus eliminating any visual differences between the two animation types other than causal impressions. This manipulation failed, however, as high causal impressions were only maintained when a cue to generative transmission was visible. This was indicated by the absence of a significant difference in causal ratings between occluded animations, regardless of whether they were presented alongside causal-appearing or non-causal-appearing animations. The experiment nevertheless found evidence that time perception had been affected by the presence of the gap sequences, regardless of causal impressions. Causal ratings were similarly low for all occluded animations and comparable to those found for backward gap sequences. Despite this, participants made significantly higher over-estimations of the delay intervals when the gap sequences were not visible, which could not be explained by differences in causal impressions.

Experiment 4 further found some evidence for temporal binding, as interval reproduction errors were significantly lower in forward gap sequence compared with backward gap sequences, in visible, slow animations. This mirrored the effects of gap sequence direction on causal ratings, where forward gap sequences only led to increased causal ratings in visible, slow animations on this occasion. It is not clear, however, why evidence for temporal binding was only found in experiment 4. The differences in causal ratings between gap sequence types found here, although statistically significant, were smaller than in the rest of Experiments 1-6 and would therefore be expected to result in a reduction in temporal binding. This cannot be explained by the increase in the number of trials, as Experiments 5 and 6 made use of the same number of trials per condition. One key difference between Experiment 4 and other experiments reported here is that the different gap sequence types were presented in separate blocks, rather than in a randomised order. Indeed, a separation of causal and non-causal conditions is typical in temporal binding experiments, whereas interleaving of causal and non-causal conditions is much less so. There are, nevertheless, some examples in which different causal conditions are interleaved, such as Desantis et al.'s (2011) study, in which temporal binding was found despite the presentation of different causal conditions within the same blocks of trials. A final possibility is that the significant interaction found in Experiment 4 was does not represent a true effect and was instead a false finding. The slope analysis reported in Chapter 3 did not find any significant evidence for a negative slope between causal ratings and interval reproduction errors in Experiment 4. This, along with the lack of any similar findings in Experiments 1-3, 5 or 6 suggests that the findings of experiment 4 are not replicable, although it is uncertain whether this is indeed the case.

Experiment 5 aimed to investigate the possible role of shifts in eye movement or attention on time perception in the findings of experiment 1-4. On half of the trials the launching objects were replaced with two stationary objects which disappeared in sequence (signal trials). These were intended as non-causal control stimuli which drew attention to the same parts of the screen as the events of launching sequences. Surprisingly, causal impressions were as high in signal trials as in the launching trials, despite the absence of motion. Signal animations did not act as a non-causal control for the launching animations, therefore. Because of this, an effect of differences in eye movements between gap sequence conditions in experiments 1-4 could not be ruled out based on the findings of experiment 5. Nevertheless, the absence of evidence for temporal binding was replicated here in both launching and signal animations. Further, the surprisingly high causal impressions in signal animations suggest that non-collision events can be used to create visual causal impressions, at least under specific circumstances. Similarly, a recent study by Bechlivanidis, Schlottmann and Lagnado (2019; experiment 3) found high causal impressions when participants viewed a collision which caused the target object to change colour, shape or size at the moment of collision. These causal ratings remained high even after exposure to a typical “launching” animation and as such these findings cannot be explained by context effects. Phenomenal causality appears not to be limited to “realistic” simulations of real-life physical interactions, therefore, although more research is needed to establish whether this is indeed the case. In particular, more research is needed to establish what non-kinematic features of the visual stimuli may give rise to a visual causal impression.

Experiment 6 included three types of launcher object movement in order to test for a role of intentionality. In self-causal trials the launcher was controlled by the participant using a computer mouse. Linear movement trials were identical to those used in experiments 1-5. Simulated self-causal trials reproduced visually similar patterns of motion, captured during a pilot session and were included as a control for the differences in the patterns of motion of the launcher object between self-causal trials and linear movement trials. Different hypotheses can be made for findings constituting evidence for temporal binding in phenomenal causality, based on agency and causal accounts of temporal binding. Based on agency accounts, if temporal binding had taken place due to phenomenal causality, gap intervals would be perceived as shorter in forward compared with backward gap sequence animations, but only in self-causal trials. A further possibility was that if phenomenal causality did not contribute to temporal binding, interval estimate errors would be lower in self-causal trials compared with simulated self-causal and linear movement trials, as “launch” events were caused by the participant’s actions regardless of visual causal impressions. Based on the causal account of temporal binding, lower interval estimate errors would be expected in forward gap sequence animations than backward gap sequence animations, regardless of the type of movement. No effect of either gap sequence direction or type of launcher movement was found, however. Based on the causal account, this supports the hypothesis that temporal binding does not occur due to phenomenal causality, in the stimuli used here.

The findings of Experiment 6 suggest that the lack of intentional actions in experiments 1-5 does not account for the lack of evidence for temporal binding. However, the lack of a “classic” temporal binding effect when intentional actions were present suggests the possibility that the lack of temporal binding in experiments 1-6 was not caused by a lack of effect of phenomenal causality on time perception but due to other perceptual influences (several such possibilities are discussed in Sections 5.1 and 5.2). As discussed above, the findings of Experiments 1 and 4 demonstrate an effect of the presence of a colour change sequence on time perception, irrespective of causal impressions. Although estimated and reproduced intervals were closer to the actual delay intervals when gap sequences were present, this does not necessarily indicate that these estimates were more accurate, as both measures are only used to report subjective perceived intervals. The conversion of perceived intervals into a numerical value as in interval estimation tasks, or the conversion of visually perceived intervals to tactile feedback as in interval reproduction tasks, mean that participants' responses are not expected to be accurate relative to actual interval durations. Further, the effects of the gap sequences on time perception would not necessarily be expected to eliminate temporal binding.

Finally, experiments 7 and 8 made use of inferred causality to test alternative explanations for the findings of experiments 1-6. Experiment 7 compared three models derived from the predictions of causal and agency accounts of temporal binding. A novel apparatus using a laser and light sensor was used to remove all non-visual cues to the onset of either of the two judged events in each trial. Participants were asked to reproduce the delay interval between event pairs in self-causal, machine-causal and non-causal conditions. The results suggested that temporal binding can be observed with the use of visual stimuli, random delay intervals and interval reproduction. Furthermore, temporal binding appeared to occur in the absence of agency but was further increased by agency. It should be noted that although an increase in temporal binding in self-action conditions has been reported previously (Buehner, 2012; 2015) it is not easily accounted for by current theoretical accounts of temporal binding. However, it is possible, under the causal account of temporal binding, that temporal binding had also occurred between the disappearing of the laser beam and the event preceding it – the initiation of the hand movement in self-action conditions or the turning of the wheel in machine-action conditions. It is not clear whether this might have affected the different causal conditions to a different extent, however. Nevertheless, if such an effect did occur it would be expected to reduce the magnitude of the temporal binding effect or cause a shift in the perceived time of the judged interval toward the first event. The presence of a contraction of the subjective delay interval suggests that temporal binding had taken place despite these possible influences.

Experiment 8 controlled for the predictability of outcomes. A key difference between experiments 1-6 and most temporal binding experiments was that participants had access to external visual cues to the onset of the second event in and the duration of the delay interval in each sequence. Previous research does not indicate that predictability leads to a reduction in temporal binding (e.g. Cravo et al., 2011; Haggard et al., 2002; Ruess et al., 2017). However, in these experiments predictability was determined by the consistency of delay intervals, i.e. whether they varied between trials within a block of trials or not, rather than external cues. Of particular interest here is whether the availability of external cues would lead to a decreased reliance on internal prior assumptions that causally related events are likely to be temporally contiguous, as might be predicted based on the causal account of temporal binding. Experiment 8 made use of a typical keypress-response temporal binding paradigm and the Libet clock method. To vary predictability, on some trials the centre of the Libet clock changed gradually in colour from white to green for the duration of the delay interval. This experiment found a significantly greater shift of the perceived time of the outcome toward the action in self-causal compared with non-causal trials, in trials where visual cues were present. No significant difference was found when no such cue was present, suggesting that increased predictability enhanced temporal binding, rather than diminishing it. The predictability of the outcome event in experiments 1-6 is therefore unlikely to have led to the lack of evidence for temporal binding.

The findings of Experiment 8 should be considered with the caveat that the experiment did not exactly replicate the gap stimuli used in Experiments 1-6, or the measures of time perception used in those experiments. Here, the gap stimulus was altered as the findings of Experiment 5 suggest that even in the absence of motion a series of objects disappearing in sequence may create visual impressions of causality. This means the signal used in place of the gap stimulus is continuous rather than made up of a series of discrete visual events and the results suggest a lack of effect of an external visual cue rather than all possible effects of the gap stimulus. Further, these findings in event perception should be applied to temporal binding in time perception with caution, as temporal binding findings are not always replicable between event and time perception measures. Lastly, this experiment only investigated outcome binding (as the signal does not alter the predictability of the time of the action) and made use of both visual and auditory event. Consequently, the findings of this study constitute strong evidence against a general effect of predictability on temporal binding, whereby increased predictability due to external visual cues would lead to a reduction in temporal binding. This does not necessarily indicate that the same effect takes place when participants make interval judgements or in the specific settings of Experiments 1-6.

5.1. Possible objections to the class of stimuli used in experiments 1-6

Experiments 1-6 made use of novel phenomenal causality stimuli, based on recent research by White (2015). These experiments were carried out under the assumption that the stimuli and sequence of events taking place in the temporal gap between the cause and effect acted as visual cues to generative transmission, implying a transfer of causal force from the launcher to the target object. Experiments 1-6, in addition to the experiments reported by White, repeatedly found that participants reported high causal impressions when cues to generative transmission were present. Visually similar gap stimuli that did not imply generative transmission (backward gap sequences) yielded similar causal ratings to those found when no gap sequence was present. This confirms that backward colour change sequences did not moderate the typical reduction in visual impressions of causality due to temporal delays between cause and effect. Because of this and due to being more visually similar to forward colour change sequences than empty gap sequences, backward gap sequences were used as low-causality control conditions throughout experiments 1-6.

Apparent motion can be suggested to have caused participants to perceive the colour change sequence as a third “launching” object which expanded from one side of the spatial gap to the other, due to apparent motion. The question of apparent motion was addressed by White (2015) who found that the launching effect persisted even when the gap objects disappeared in sequence, where apparent motion could not have taken place.

A further objection to the use of these stimuli might be that the stimuli used here represented a “domino effect”, whereby the colour change sequence was perceived as a sequence of causally related events, rather than the gap sequence acting as an abstract representation of a medium by which causal force can be transferred across a spatial and temporal gap. The launching object would therefore be perceived as the cause of the first colour change, while all subsequent events would be perceived as following from the preceding event. It is not known whether events and temporally bound only to their immediate causes and effects; temporal binding research to date has only investigated causal chains of up to three events (Yabe et al., 2017). Yabe et al.’s research found evidence that temporal may take place between multiple events within a three-event chain but did not investigate binding between the first and third events in a three-event chain. However, Yabe et al.’s study showed that an event may be temporally bound toward both the preceding and following event, meaning temporal binding may be cancelled in these cases. It may be that participants perceived a series of causally related events as taking place during gap sequences, with each temporally bound to the preceding event, resulting in the absence of temporal binding. While this remains a possibility, care should be taken in applying Yabe et al.’s findings to the studies described here as Yabe et al.’s study made use of the Libet clock method and measured event, rather than time perception. As discussed previously, some differences have been found between temporal binding in event perception and time perception and the extent to which event perception contributes to the contraction of the perceived interval between cause and effect is unknown at this time.

An assumption implicit in all accounts of temporal binding is that events are temporally bound to some, but not all causally related events. This is because if all events were equally perceptually bound to both previous and subsequent events, no temporal binding effect would be expected. In the mechanisms involved in keypress-tone event sequences, for instance, participants cause the key to depress, which completes a circuit and initiates computer operations resulting in the output of the tone. The immediate cause of the tone is therefore the unseen computer operations, rather than the action of the participant. Nevertheless, the pressing of the key is expected to be perceived as the cause of the tone. For this reason, as in White's (2015) research, in Experiments 1-6 participants were asked to report their impression of the first object to move as causing the launch of the target, rather than simply their impression of whether a causal event had taken place. The high causal ratings found in forward gap sequences suggest that participants perceived the "collision" of the launcher with the gap stimuli as the cause of the launch, rather than an intermediary event. This is similar to other evidence showing that even in "tool effect" stimuli, where an intermediary object is present between the launcher and target, participants perceive the first object to move as the cause of the motion of the target object (e.g. Michotte, 1963/46). As such, in order for the perception of a "domino" or tool effect to result in the absence of temporal binding, such an effect must only take place when participants are aware of the time at which the intermediary causal event had taken place, but not when the time of this event cannot be determined, as is the case in most temporal binding studies.

In a similar vein, it could be suggested that the collision of the launcher with the gap stimuli may be interpreted as having multiple consequences. The collision of the launcher with the gap objects can be viewed as causing the colour change sequence in addition to the "launch" of the target. Whether temporal binding occurs only between events and their most direct consequences, rather than between events and all their causal consequences, has not been investigated to date. However, in most temporal binding studies, actions lead to immediate effects, such as auditory and tactile feedback for the key/lever being pressed, in addition to the subsequent tone or flash. Given that evidence for evidence of temporal binding has been found despite this, it does not appear likely that the perception of multiple consequences attributed to the same cause would prevent temporal binding from taking place.

The evidence points to the gap sequences used here as maintaining causal impressions over temporal and spatial gaps by implying a transfer of force from the launcher to the target object. This, in addition to the consistently distinct causal impressions reported by participants suggests that the causal impressions generated by the class of stimuli used here were appropriate in studying temporal binding in phenomenal causality. Several potential drawbacks of this class of stimuli were discussed in this section. However, the existing literature does not support any of these as likely causes for the absence of evidence for temporal binding in Experiments 1-6. Likewise, theoretical accounts of temporal binding would not predict that these would lead to a lack of temporal binding.

5.2. Can the lack of temporal binding in phenomenal causality be explained by low-level perceptual differences?

A key difference between most temporal binding studies and experiments 1-6 is the presence of visual feedback at the onset of the events before and after the judged interval. Although participants made motor movements during self-causal trials, the time of the collision of the launcher object with the gap stimuli could not be inferred from proprioceptive or tactile feedback. This can be assumed as the beginning of the gap sequences was initiated depending on the position of the launcher object, rather than in response to any movement of the computer mouse by the participant. Although previous studies have replicated temporal binding with the use of visual outcomes (e.g. Engbert et al., 2008; Ruess et al. 2018), the intentional actions used in temporal binding studies typically include tactile and auditory feedback at the time of the lever/key-press. To address this, in experiment 7 visual feedback alone was available at the onset of the events before and after the judged interval in all conditions. Experiment 7 succeeded in replicating temporal binding in the absence of non-visual sensory feedback at the onset of causes and their outcomes, with the use of the same interval reproduction procedure used in experiments 4-6, and a similar number of participants and trials as used in experiments 4-6. The absence of multisensory feedback and the use of visual feedback alone do not appear to be likely explanations for the lack of temporal binding observed in experiments 1-6, therefore.

Experiments 1 and 4 both made use of conditions in which no gap objects were visible between the launcher and target objects, and no gap sequence took place. In both, this resulted in significantly higher over-estimations of the delay intervals compared with animations containing a gap sequence. As causal ratings in the absence of a gap sequence were similar to those found for backward gap sequences, causal impressions cannot account for this effect. The very presence of a colour change sequence, regardless of whether it acted as a cue to generative transmission, appears to have affected perceived delay intervals, therefore. The key question in interpreting the findings of experiments 1-6 is whether low-level perceptual differences between conditions may have eliminated a temporal binding effect which might otherwise have been observed. However, it is the relative differences in interval estimates/reproduced intervals, rather than absolute magnitude of these estimates, that are of interest in investigating temporal binding. Therefore, an effect of low-level perceptual differences between conditions can only have eliminated temporal binding if either a) this effect did not affect all gap sequence conditions equally, or b) the effect prevented any influence of causal perception on perceived interval durations.

Experiments 4 and 5 failed to rule out an effect of changes in eye movement due to differences in the spatial focus of attention between forward and backward gap sequence animations. Past research has found a compression of the perceived length of intervals presented during saccadic eye movements (Morrone et al., 2005). As such, if participants made more eye movements during non-causal animations this may have resulted in shorter estimates of interval durations, leading to a similar contraction in the perceived interval duration in visually non-causal conditions as would be expected in visually causal conditions due to temporal binding. While in forward gap sequence animations the focus of attention moved consistently from the left to the right side of the screen, in backward gap sequence animations this shifted from left (collision) to right (beginning of the gap sequence), right to left (the gap sequence) and back to the right (launch). This suggests the possibility that a greater number of eye movements were made during backward gap sequence animations, if participants failed to fixate on the fixation cross. Two caveats to this possible explanation of the findings, must be considered, however. Firstly, it requires the assumption that the contraction in perceived delays due to eye movements was consistently of a similar magnitude to the effect of temporal binding across all conditions. In experiments 2 and 3 this would have also had to occur in offset gap animations which were visually different from both forward and backward animation types, with middling causal ratings. Secondly, this explanation assumes that a sufficient number of participants affected the findings by not following the instructions to fixate on the fixation point. Overall, it is unlikely that differences in eye movement between gap sequence types can explain the lack of evidence for temporal binding in experiments 1-6, for the reasons discussed above.

However, this does not rule other possible effects of differences in the spatial focus of attention between different gap sequence types. Attention has been proposed to have a role in time perception (e.g. see Lejeune, 1998) whereby an increase in the attentional resources focussed on the passage of time results in a slowing of the perceived rate of the passage of time and longer perceived interval durations. Conversely, the shifting of attentional resources from the passage of time results in time being perceived as passing more quickly and interval durations appearing shorter. In the case of Experiments 1-6 it is possible that backward gap sequence types may have attracted attention to a greater extent than forward gap sequence animations due to the absence of a coherent direction of motion. For the same reason, it is further possible that participants were better able to distinguish the collision and launch from preceding events in backward colour change sequence. This is as the sequence of events was less continuous than forward colour change sequence animations and, therefore, the collision and launch may have appeared more salient.

However, discussed above in relation to the effects of eye movement, both of these explanations require that the perception differences between gap sequence types are consistently sufficient to eliminate a temporal binding effect which would otherwise be present, across all 6 experiments. In addition, this may only explain the findings under the causal account of temporal binding, which predicts temporal binding in Experiments 1-5 despite the absence of intentionality. In contrast, agency accounts would only predict temporal binding in Experiment 6 and therefore, if differences in eye movement, attention, or the salience of events between forward and backward colour change sequences affected time perception temporal estimates should have been significantly smaller in backward gap sequence animations, in the absence of agency. This, however, was not the case, with the exception of Experiment 5.

The alternative to temporal binding being masked by differences in low-level perceptual influences between conditions, as discussed above, is that temporal binding did not occur at all due to the perceptual features of the stimuli used in Experiments 1-6. As mentioned previously, in Experiments 1 and 4 the presence of a colour change sequence, regardless of its direction, led to significantly smaller estimates of the delay interval compared with animations with no visible colour change sequence. This finding is surprising due to the well-established “filled duration illusion” (e.g. Buffardi, 1971). In this illusion temporal intervals filled with visual or auditory events are perceived as longer than “empty” intervals, whereas the converse was observed in experiments 1 and 4. A key difference, however, is that the gap stimuli served as a cue to the end of the gap interval, whereas in investigations of the filled duration illusion participants typically do not know when the judged interval will end in advance. Although experiments 1 and 4 showed no evidence of a filled duration illusion taking place during visible gap sequence trials, a clear effect of the colour change sequences on interval perception was found. It is currently unknown how the temporal binding effect interacts with other temporal illusions - whether the effects are additive, or whether the presence of other temporal illusions moderates or even eliminates the temporal binding. Therefore, it remains possible the regularity of the colour changes may have prevented the influences of causality and agency on interval perception, although the mechanisms by which this could have occurred are unclear.

It could be claimed that the predictability of interval durations may have eliminated temporal binding by making the task “too easy” by providing external visual cues to the onset, duration and end of the judged intervals. Such a claim would assume that higher predictability would lead participants to rely less on internal cues when estimating interval durations. Based on the causal account of temporal binding, this would reflect a decreased reliance on the prior assumption that causally related events are likely to be contiguous on the perceived interval and an increased reliance on external sensory cues. This, however, is not supported by the existing evidence for an effect of predictability on temporal binding. Although the evidence to date is mixed - some studies have found temporal binding to be greater when predictability is increased (e.g. Cravo et al., 2011; Haggard et al., 2002) while others have found no such effect (e.g. Ruess et al., 2017) – it does not suggests that increased predictability diminishes temporal binding. However, the above studies manipulated predictability by altering the consistency of delay intervals (different delay intervals were presented in a randomised order rather than within separate blocks). While such manipulations indeed make the intervals more predictable, they do not do so with the use of external visual cues as was the case in experiments 1-6. Experiment 8 was carried out to test whether such external cues affect temporal binding and found evidence for an increase in temporal binding when predictability was increased. Participants performed a typical temporal binding task using a Libet clock, with separate blocks containing either no visual cues or a continuous visual cue taking place for the duration of the delay interval and providing a visual cue to the end of the interval. The increase in predictability was found to increase, rather than reduce, outcome binding. Based on this finding, along with others described above, it is unlikely that the predictability of the stimuli or the presence of external sensory cues had reduced temporal binding. On the contrary, if temporal binding had occurred, the effect is likely to have been greater due to increased predictability.

Past findings and the experiments presented in this thesis failed to eliminate some possible influences of low-level perceptual differences between stimuli on the lack of evidence for temporal binding in experiments 1-6. Differences in saccadic eye movements due to changes in the spatial focus of attention and the physical features of the gap sequences (regular sequences of visual events) cannot be ruled out. Both explanations, however, require some additional assumptions. While it cannot be verified whether participants successfully followed instructions to focus on the fixation cross during each trial, it seems unlikely that the effect of differences in eye movements would consistently lead to changes to time perception able to cancel out evidence of temporal binding, across different animation types with varying causal impressions. It should also be noted that eye movements were restricted by the use of a fixation cross, as in other temporal binding studies. Likewise, the regular sequence of visual events taking place during gap intervals appears to have affected perceived interval durations. However, there is no explanation for how this could have affected different gap sequence directions in different ways. It is further unclear why this would eliminate all other influences on the perceived duration of the delay, such as causality/intentionality or event perception.

5.3. Phenomenal causality and agency accounts of temporal binding

The issue of whether causality alone is sufficient for temporal binding remains contentious. Some studies, including experiment 7 reported here and experiments by Buehner (2012; 2015) have found evidence for this, while others have reported an increase in temporal binding due to agency (e.g. Caspar et al., 2016; Wohlschlager et al., 2003a; Wohlschlager et al., 2003b). Indeed, experiment 7 found evidence for an increase in temporal binding due to agency in addition to causality. Nevertheless, it is generally agreed that causality is necessary for temporal binding to take place. Experiment 6 made use of animations in which participants manually controlled the launcher object and found no evidence of temporal binding regardless of agency. It is unlikely, therefore, that the lack of temporal binding in experiments 1-5 can be explained by the lack of agency in those studies.

Cravo et al. (2009) found evidence for temporal binding in phenomenal causality, but only when the launcher was controlled by the participant. This is typically interpreted as evidence that both the perception of causality and agency are necessary for temporal binding to take place (for instance, see Moore & Obhi, 2012). However, agency accounts make no clear prediction as to whether visual impressions of causality are sufficient for temporal binding to occur, and as such either temporal binding in phenomenal causality or a lack thereof can be difficult to interpret under these accounts. This is as visual impressions of causality are illusory by their nature. Participants observing launching animations report their impressions of causality while simultaneously aware that these stimuli are two-dimensional representations of causal events, rather than real objects.

In both Cravo et al.'s experiment and Experiments 6, participants' actions caused the movement of the target objects in all animation types. Whether the animations created a visual impression of causality did not determine the effect of participants' actions on the events that followed. Visual impressions of causality were therefore in conflict with inferred causality, and indeed the actual causal mechanism taking place. There is further no evidence from either study for a cumulative effect of both forms of causal perception: estimated intervals were not significantly longer when they lacked both a visually perceived and inferred causal structure. However, Dogge et al. (2012) found evidence for temporal binding between a keypress and its outcome when participants were asked to imagine they are in control of their movements, despite their fingers being moved passively. Dogge et al.'s findings demonstrate that imagined causality and agency can be sufficient for temporal binding to take place, even when the participant is aware of the actual causal structure of the task. If imagined causality and agency can lead to temporal binding, it is plausible that illusory causal impressions could contribute to temporal binding.

It remains unclear why experiment 6 did not replicate the findings of Cravo et al. Experiment 6 aimed to improve on Cravo et al.'s design. The number of participants and trials was increased, and a simulated self-movement condition was added to control for the perceptual differences between self- and computer-generated patterns of motion. As such the findings are not easily explained by a lack of statistical power or perceptual differences between self-causal and other conditions. In addition, participants were given increased control over the movement of the launcher, including the direction of movement and momentum, in order to ensure the perception of agency was as high as possible. In contrast, it can be argued that Cravo et al.'s participants were not in full control of the launcher, but able only to play or pause the animation, by holding down or releasing the mouse button, respectively. The methods used in Experiment 6 resulted in less experimental control over the speed and direction of the motion of the launcher object, adding an additional perceptual difference between conditions. It is possible that this resulted in the absence of temporal binding in some way. However, no significant differences were found between the control, simulated self-movement condition and either the linear movement or self-movement conditions, suggesting that this was not the case. Furthermore, Cravo et al. did not report any constraints of participants' eye movements, such as a fixation cross or instructions to fixate on a particular part of the screen. It is possible, as discussed previously, that differences saccadic eye movements had affected temporal estimates unequally between conditions.

Discussion of the role of causality in agency accounts of temporal binding is usually limited to investigations of whether causality is necessary or sufficient for the effect to occur. It is typically assumed that temporal binding only takes place between actions and their perceived consequences (e.g. Moorre & Obhi, 2012; Desantis et al., 2011). – indeed, "intentional binding", as first reported, was defined as an attraction in perceived time between intentional action and their sensory consequences. Agency accounts of temporal binding had not considered phenomenal causality as distinct from inferred causality, including in the interpretation of the findings of Cravo et al. (2009). Instead, causal attribution often is studied as a single variable (e.g. Desantis et al., 2011; Haggard et al., 2002). Fewer investigations had been carried out where causal beliefs and actual causality are in conflict (e.g. Dogge et al., 2012). Cravo et al.'s findings suggest either that temporal binding can only occur if visual causal impressions and inferred causality are in agreement, or that stimuli visually perceived as non-causal eliminate temporal binding. However, this was not replicated in Experiment 6. The findings of experiment 6 were equally surprising under agency accounts of temporal binding. Not only did visual impressions of causality not lead to a temporal binding effect, but no temporal binding appeared to take place between participants' actions and their consequences. These results cannot be explained either by a lack of temporal binding in phenomenal causality.

More research is required to determine how conflicting causal impressions and beliefs modulate temporal binding, and the theoretical reasons why this might occur. Although it is possible that the illusion of causality caused by the launching effect has a similar effect to the imagined causality studied by Dogge et al. (2012). However, findings of Dogge et al., as well as those of Desantis et al. (2011), demonstrate a top-down influence of beliefs of causality/intentionality. In contrast, phenomenal causality is suggested to be processed visually (e.g. Scholl & Tremoulet, 2000) and is processed rapidly and automatically, without the need for higher-level processing. This would suggest that any influence of inferred causality on motor predictions would not take place in the same manner as imagined causality.

5.4. Is temporal binding specific to inferred causality?

The lack of evidence for temporal binding in phenomenal causality raises the possibility that temporal binding arises from causal inferences alone and not visual impressions of causality. While Cravo et al.'s (2009) findings suggest temporal binding due to phenomenal causality is possible, their study has not been replicated since and alternative explanations for their findings have not been ruled out at the time of writing. For instance, the stimuli used were not perceptually similar due to differences in the direction of movement of the launching objects. This is further complicated by the lack of control over participants' eye movements. Any general inferences about the role of phenomenal causality in temporal binding cannot be made based on Cravo et al.'s findings alone, particularly as they were contradicted by the findings of experiment 6 reported here. As noted above, it is difficult to account for phenomenal causality in agency accounts of temporal binding as causality had not been the focus of the majority of temporal binding research and theory. Under the causal account of temporal binding, however, an absence of temporal binding in phenomenal causality may indicate that time perception is not influenced by visual impressions of causality in the same way as inferred causality.

The causal account of temporal binding proposes a bidirectional relationship between the perception of causality and time: just as temporally contiguous events are more likely to be perceived as causally related, binding is suggested to result from causally related events being perceived as likely to be temporally contiguous (Eagleman & Holcombe, 2002). As is the case in inferred causality, temporal contiguity has been found to be important in the formation of visual causal impressions (see Chapter 1, Section 1.3 for more detail on temporal contiguity and phenomenal causality.

By implication, if temporal binding does not occur due to phenomenal causality, the relationship between time perception and visual causal would have to be one-sided, with temporal cues informing causal impressions, while causal impressions do not affect temporal judgements. Scholl and Tremoulet (2000) proposed that that phenomenal causality is processed visually and that the perceptual processes responsible for phenomenal causality are encapsulated from other processes. The processing of temporal cues contributing to phenomenal causality would have to be similarly encapsulated from other time perception processes.

However, recent research has found evidence for visual causal impressions leading to a contraction in the perceived distance between launching objects, which the authors suggested resulted from the implicit assumption that causally related events are likely to be spatially contiguous (Buehner & Humphreys, 2010). Likewise, Bechlivanidis and Lagnado (2016) found that causal assumptions in launching animations can lead to an illusory reversal of the perceived temporal order of events. These findings suggest that visual impressions of causality can indeed affect the perception of space and time. Such findings, although qualitatively different from temporal binding, are predicted by the causal account of temporal binding, which does not presuppose that the influence of prior assumptions based on causal perception would apply only to time perception.

The question of whether phenomenal causality is processed by an encapsulated module is beyond the scope of this thesis. The inference that temporal binding is unique to inferred causality cannot be made based on the findings presented here and the temporal binding literature to date. The studies discussed above suggest a clear relationship between causal impressions and other perceptual processes, in addition to the well-documented effects of temporal cues on causal impressions. To infer that phenomenal causality does not contribute to temporal binding requires two additional assumptions: that visual impressions of causality are processed in a separate module from inferred causality, that this processing does not affect interval judgements. Both the research presented here and Cravo et al.'s research made use of "launching" impressions to manipulate perceived causality, whereas other, qualitatively different forms of visual causal impressions have also been found (see Hubbard, 2011, for a review of these findings). As such, care should be taken in generalising the findings reported here to all forms of visual causal impressions.

5.5. Conclusion

Experiments 1-6 have found no clear evidence for temporal binding in phenomenal causality, while experiments 7 and 8 eliminated some potential explanation for these findings. The results expected if temporal binding had taken place due to phenomenal causality, based on both causal and agency accounts, had not occurred. In addition, experiment 6 did not find evidence for temporal binding between intentional actions and their consequences when participants controlled the launcher object in similar animations to those used in Experiments 1-6.

These results are difficult to account for under both agency accounts and the causal account of temporal binding. This raises the possibility that findings had been affected in an unforeseen way by the stimuli used here. While the use of interval estimation and interval reproduction tasks and the use of visual stimuli had been validated by Experiment 7, the use of phenomenal causality and the colour change sequences used here are novel in temporal binding research. As discussed earlier in this chapter, it is possible that temporal binding did not take place due to the causal structure perceived in these animations, or that temporal estimates and were affected by the perceptual features of the animations in a way which had eliminated temporal binding.

The above hypotheses have been largely unexplored to date. More specifically, very little research has been carried out investigating temporal binding in causal chains of more than two events (with the exception of Yabe et al., 2017) or between actions and their multiple consequences. Similarly, it is not known how temporal binding interacts with other factors influencing time perception, such as attention, movement and the accuracy of temporal judgements. There is clear scope for future research on temporal binding which could not only enhance our understanding of its underlying mechanisms but enable us to understand the circumstances under which temporal binding does not take place, and how this may affect the findings of temporal binding research. This is of particular importance in studies comparing causal and non-casual events, or intentional actions and other events, where it is difficult to create different conditions which differ in causality or agency while remaining otherwise perceptually identical.

The attempt to interpret the findings of the experiments presented here under agency accounts of temporal binding demonstrates the difficulty in accounting for causality in these models. These often make no clear predictions regarding causality other than it must be necessary for temporal binding to occur. Whether causal or agency accounts can be demonstrated to best explain temporal binding, little is known about how, rather than simply whether, causality contributes to the effect. The disproportionate focus on agency in temporal binding research is surprising given that causality and agency appear to be at least equally necessary for temporal binding to take place. Further investigation of the role of causality in temporal binding have the potential to greatly further our knowledge of the effect.

Under the causal account of temporal binding the results of Experiments 1-6 are particularly surprising. It is unclear why phenomenal causality should not affect temporal judgements in a similar way to inferred causality given that phenomenal causality is at least as sensitive to temporal contiguity, if not more so. As discussed earlier, if other perceptual influences cannot explain the findings reported here it may indicate a difference in how visual causal impressions and causal inferences contribute to time perception, although this is beyond the scope of the research reported here.

Overall, although temporal binding had attracted much attention since it was first reported in 2002, many potential research questions remain unexplored. The difficulty in accounting for the findings of Experiments 1-6 demonstrates the need for more research on temporal binding in phenomenal causality and the need for greater variety of stimuli and apparatus in temporal binding research, such as the actions performed by participants and the sensory feedback available throughout the task. More research is needed to establish the exact role of causality in temporal binding and temporal binding in more complex settings and causal structures. While much has been discovered about the effect to date, there is further potential for temporal binding research to inform our understanding of the perception of causality, agency and time and how all three interact.

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7. Appendix A: full instructions provided to participants in experiments 1-6

Below are the full main instructions describing the stimuli and tasks to participants, in experiments 1-6. These were presented at the beginning of each experimental block of trials, before the practice section.

7.1.1. Instructions presented to participants before the causal block in experiment 1

In this experiment you will see a series of short movies, about two or three seconds in duration. When you see the movies, you will need to keep your eyes fixated on a red fixation cross.

In each movie you will see a black rectangle move across the screen. At some point the black rectangle might come into contact with a row of rectangles, depending on the condition. After the black rectangle stops moving, the white rectangle at the far end of the row will start to move.

We are interested in your impression of whether the black rectangle brings about the motion of the white rectangle. This might sound odd, since the black rectangle does not come into contact with the white rectangle. However, it is still possible to have the visual impression that the black rectangle makes the white rectangle move even though they don't come into contact. For each movie you will be asked the following question:

"Did you have the impression that the black rectangle brought about the motion of the white rectangle?"

You should answer this question by providing a number from 0 to 100. If you had an impression that the black rectangle brought about the motion of the white rectangle, put a rating somewhere between 0 and 100 depending on how strong the impression was. A rating of 100 indicates a very strong impression, whereas a rating of 0 indicates that did not have an impression that the black rectangle brought about the motion the white rectangle at all, and that the white rectangle moved of its own accord.

You will now see several practice movies so you can get familiar with the procedure.

For each movie you will have to provide a causal estimate.

Please read these instructions again and then press SPACE to begin the practice trials.

7.1.2. Instructions presented to participants before the temporal block in experiment 1

“In this experiment you will see a series of short movies, about two or three seconds in duration. When you see the movies, you will need to keep your eyes fixated on a red fixation cross.

In each movie you will see a black rectangle move across the screen. At some point the black rectangle might come into contact with a row of rectangles, depending on the condition. After the black rectangle stops moving, the white rectangle at the far end of the row will start to move.

We are interested in your time perception with respect to these events.

More specifically, we want to know how much time you think has passed between the stopping of the black rectangle and the onset of motion of the white rectangle. For each movie you will be asked the following question:

"How much time passed between the stopping of the left rectangle and the motion onset of the right rectangle (in milliseconds)"

You should answer this question by providing a number from 0ms(no time at all) to 1000ms. Please remember that 1 second = 1000 milliseconds. None of the intervals you will be asked to judge will be longer than 1000 milliseconds.

You will now see several practice movies so you can get familiar with the procedure. After each, you will have to provide a temporal estimate in milliseconds.”

Please read these instructions again and then press SPACE to begin the practice trials.

7.1.3. Instructions presented to participants before the causal block in experiment 2

In this experiment you will see a series of short movies, about two or three seconds in duration. When you see the movies, you will need to keep your eyes fixated on a red fixation cross.

In each movie you will see a black rectangle move across the screen. At some point the black rectangle might come into contact with a row of rectangles, depending on the condition. After the black rectangle stops moving, the white rectangle at the far end of the row will start to move.

We are interested in your impression of whether the black rectangle brings about the motion of the white rectangle. This might sound odd, since the black rectangle does not come into contact with the white rectangle. However, it is still possible to have the visual impression that the black rectangle makes the white rectangle move even though they don't come into contact. For each movie you will be asked the following question:

"Did you have the impression that the black rectangle brought about the motion of the white rectangle?"

You should answer this question by providing a number from 0 to 100. If you had an impression that the black rectangle brought about the motion of the white rectangle, put a rating somewhere between 0 and 100 depending on how strong the impression was. A rating of 100 indicates a very strong impression, whereas a rating of 0 indicates that did not have an impression that the black rectangle brought about the motion the white rectangle at all, and that the white rectangle moved of its own accord.

You will now see several practice movies so you can get familiar with the procedure.

For each movie you will have to provide a causal estimate.

Please read these instructions again and then press SPACE to begin the practice trials.

7.1.4. Instructions presented to participants before the temporal block in experiment 2

In this experiment you will see a series of short movies, about two or three seconds in duration. When you see the movies, you will need to keep your eyes fixated on a red fixation cross.

In each movie you will see a black rectangle move across the screen. At some point the black rectangle might come into contact with a row of rectangles, depending on the condition. After the black rectangle stops moving, the white rectangle at the far end of the row will start to move.

We are interested in your time perception with respect to these events.

More specifically, we want to know how much time you think has passed between the stopping of the black rectangle and the onset of motion of the white rectangle. For each movie you will be asked the following question:

"How much time passed between the stopping of the left rectangle and the motion onset of the right rectangle (in milliseconds)"

You should answer this question by providing a number from 0ms (no time at all) to 1000ms. Please remember that 1 second = 1000 milliseconds. None of the intervals you will be asked to judge will be longer than 1000 milliseconds.

You will now see several practice movies so you can get familiar with the procedure. After each, you will have to provide a temporal estimate in milliseconds.

Please read these instructions again and then press SPACE to begin the practice trials.

7.1.5. Instructions presented to participants at the beginning of experiment 3

In this experiment you will see a series of short movies, about two or three seconds in duration. When you see the movies, you will need to keep your eyes fixated on a red fixation cross.

In each movie you will see a black rectangle move across the screen. At some point the black rectangle might come into contact with a row of rectangles, depending on the condition. After the black rectangle stops moving, the white rectangle at the far end of the row will start to move.

On each trial we will ask you one of two questions. In some of the trials we will be interested in your impression of whether the black rectangle brings about the motion of the white rectangle. This might sound odd, since the black rectangle does not come into contact with the white rectangle. However, it is still possible to have the visual impression that the black rectangle makes the white rectangle move even though they don't come into contact. For each movie you will be asked the following question:

"Did you have the impression that the black rectangle brought about the motion of the white rectangle?"

You should answer this question by providing a number from 0 to 100. If you had an impression that the black rectangle brought about the motion of the white rectangle, put a rating somewhere between 0 and 100 depending on how strong the impression was. A rating of 100 indicates a very strong impression, whereas a rating of 0 indicates that did not have an impression that the black rectangle brought about the motion the white rectangle at all, and that the white rectangle moved of its own accord.

On other trials you will be asked to hold down the control key for an amount of time that matches the duration between the time at which the black rectangle stopped and the white rectangle started moving. Please try to match the duration as accurately as you can. You will not find out which task you will be performing until after you have seen the animation.

You will now see several practice movies so you can get familiar with the procedure.

Please read these instructions again and then press SPACE to begin the practice trials, or alert the experimenter if you have any questions.

7.1.6. Instructions presented to participants at the beginning of the first block of trials in experiment 4

This experiment is made of two blocks of trials. In each block you will see a series of short movies, a few seconds in duration. When you see the movies, you will need to keep your eyes fixated on a red fixation cross.

In each movie you will see a black rectangle move across the screen. At some point the black rectangle will come into contact with a row of rectangles. After the black rectangle stops moving, the white rectangle at the far end of the row will start to move. On some trials a part of the screen might be hidden behind another object.

On each trial we will ask you one of two questions. In some of the trials we will be interested in your impression of whether the black rectangle brings about the motion of the white rectangle. This might sound odd, since the black rectangle does not come into contact with the white rectangle. However, it is still possible to have the visual impression that the black rectangle makes the white rectangle move even though they don't come into contact. For each movie you will be asked the following question:

"Did you have the impression that the black rectangle brought about the motion of the white rectangle?"

You should answer this question by providing a number from 0 to 100. If you had an impression that the black rectangle brought about the motion of the white rectangle, put a rating somewhere between 0 and 100 depending on how strong the impression was. A rating of 100 indicates a very strong impression, whereas a rating of 0 indicates that you did not have an impression that the black rectangle brought about the motion the white rectangle at all, and that the white rectangle moved of its own accord.

On other trials you will be asked to hold down the left mouse button for an amount of time that matches the duration between the time at which the black rectangle stopped and the white rectangle started moving. Please try to match the duration as accurately as you can. You will not find out which task you will be performing until after you have seen the animation.

At the beginning of each block you will see several practice movies so you can get familiarised with the procedure.

Please read these instructions again and then press [P] to proceed to the first block of trials, or alert the experimenter if you have any questions.

7.1.7. Instructions presented to participants at the beginning of “movement” block of trials in experiment 5

In this block of trials you will see a series of short animations. When you see the animations, you will need to keep your eyes fixated on a red fixation cross.

In each animation you will see a black rectangle move across the screen. At some point the black rectangle will come into contact with a row of smaller rectangles. After the tall black rectangle stops moving, the white rectangle at the far end of the row will start to move.

On each trial we will ask you about your impression of whether the black rectangle brought about the motion of the white rectangle. This might sound odd, since the tall black rectangle does not come into contact with the white rectangle. However, it is still possible to have the visual impression that the black rectangle makes the white rectangle move even though they don't come into contact. For each movie you will be asked the following question:

"Did you have the impression that the black rectangle brought about the motion of the white rectangle?"

You should answer this question by providing a number from 0 to 100. If you had an impression that the black rectangle brought about the motion of the white rectangle, put a rating somewhere between 0 and 100 depending on how strong the impression was. A rating of 100 indicates a very strong impression, whereas a rating of 0 indicates that you did not have an impression that the black rectangle brought about the motion the white rectangle at all, and that the white rectangle moved of its own accord.

Please read these instructions again and then press [C] to continue, or alert the experimenter if you have any questions.

7.1.8. Instructions presented to participants at the beginning of the "signal" block of trials in experiment 5

In this block of trials you will see a series of short animations. When you see the animations, you will need to keep your eyes fixated on a red fixation cross.

In each animation you will see a tall black rectangle beside a row of shorter rectangles, and a tall white rectangle on the other side of the row of shorter rectangles. In each trial the tall black rectangle will disappear, followed by the white rectangle disappearing.

On each trial we will ask you about your impression of whether the black rectangle disappearing made the white rectangle disappear. This might sound odd, since the black rectangle does not come into contact with the white rectangle. However, it is still possible to have the visual impression that the black rectangle makes the white rectangle disappear even though they don't come into contact. For each movie you will be asked the following question:

"Did you have the impression that the black rectangle made the white rectangle disappear?"

You should answer this question by providing a number from 0 to 100. If you had an impression that the black rectangle made the white rectangle disappear, put a rating somewhere between 0 and 100 depending on how strong the impression was. A rating of 100 indicates a very strong impression, whereas a rating of 0 indicates that you did not have an impression that the black rectangle made the white rectangle disappear at all, and that the white rectangle disappeared of its own accord.

Please read these instructions again and then press [C] to continue, or alert the experimenter if you have any questions.

7.1.9. Instructions presented to participants at the beginning of the “linear” and “simulated self-causal” blocks of trials in experiment 6

In this block of trials you will see a series of short animations. When you see the animations, you will need to keep your eyes fixated on a red fixation cross.

In each animation you will see a black rectangle move across the screen. At some point the black rectangle will come into contact with a row of rectangles. After the black rectangle stops moving, the white rectangle at the far end of the row will start to move.

On each trial we will ask you about your impression of whether the black rectangle brought about the motion of the white rectangle. This might sound odd, since the black rectangle does not come into contact with the white rectangle. However, it is still possible to have the visual impression that the black rectangle makes the white rectangle move even though they don't come into contact. For each movie you will be asked the following question:

"Did you have the impression that the black rectangle brought about the motion of the white rectangle?"

You should answer this question by providing a number from 0 to 100. If you had an impression that the black rectangle brought about the motion of the white rectangle, put a rating somewhere between 0 and 100 depending on how strong the impression was. A rating of 100 indicates a very strong impression, whereas a rating of 0 indicates that you did not have an impression that the black rectangle brought about the motion the white rectangle at all, and that the white rectangle moved of its own accord.

Please read these instructions again and then press [C] to continue, or alert the experimenter if you have any questions.

7.1.10. Instructions presented to participants at the beginning of the “self-causal” block of trials in experiment 6

In this block of trials you will be interacting with a series of short animations. During each trial, you will need to keep your eyes fixated on a red fixation cross.

In each trial you will see a row of grey rectangles, with a white rectangle at the end. There will also be a black rectangle, to the left of the row of rectangles, which you will be controlling using the mouse. In each trial you will need to move the black object to the right, until it makes contact with the row of grey rectangles. Nothing will happen if you move the black object too slowly. If the black rectangle was moving fast enough, the white rectangle at the far end of the row will start to move. Between these two events, the row of grey rectangles will change colour.

You will now have several practice trials to get used to this task.

Please read these instructions again and then press [C] to continue, or alert the experimenter if you have any questions.

8. Appendix B: additional analyses: interaction effects: experiments 1-3

In Chapter 3, interaction effects in temporal estimate errors and interval reproduction errors not predicted by temporal binding. If temporal binding had taken place, main effects and interactions of estimate and reproduction errors were expected to mirror the effects found in causal ratings. As such, interactions which did not indicate a temporal binding effect, or did not find evidence for a moderation of temporal binding by variables other than causality or agency, were omitted as they were of limited theoretical value. Interactions in causal ratings are included here where these are of a small magnitude, do not indicate a crossover effect and are unlikely to be expected to cause detectable effects on perceived interval durations.

8.1.1. Interaction effects in temporal estimate errors: experiment 1

As reported in Chapter 3, a significant interaction was found between gap sequence type and animation speed. This interaction was explored using the same planned contrasts used to explore the main effect of animation type, and comparisons between the slow and medium, and medium and fast speeds. Interaction graphs of significant interactions can be seen in Figures 8.1, 8.2 and 8.3.

Significant interactions were found between the continuous + covariation and continuous sequences at fast and medium speeds ($F(1, 29) = 8.75$, $p = .006$, $\eta_p^2 = .23$), between the continuous and backward sequences at fast and slow speeds ($F(1, 29) = 4.95$, $p = .03$, $\eta_p^2 = .15$), and between the backward and empty gap sequences at medium and slow speeds ($F(1, 29) = 4.41$, $p = .04$, $\eta_p^2 = .13$). No significant interactions were found when comparing the forward and continuous + covariation gap sequence types at fast and medium speeds ($F(1, 29) = .10$, $p = .76$, $\eta_p^2 = .003$) and medium and slow speeds ($F(1, 29) = 1.78$, $p = .19$, $\eta_p^2 = .06$). Likewise, no significant interactions were found when comparing the two continuous gap sequence types and medium and slow speeds ($F(1, 29) = .004$, $p = .95$, $\eta_p^2 < .001$), continuous gap sequences and backward gap sequences and medium and slow speeds ($F(1, 29) = .70$, $p = .41$, $\eta_p^2 = .02$) or backward and empty gap sequences and fast and medium speeds ($F(1, 29) = .02$, $p = .88$, $\eta_p^2 = .001$).

The comparison of the continuous + covariation and continuous gap sequences shows a clear crossover effect between the fast and medium speeds (Figure 8.1), in which estimate errors were lower for continuous gap sequences compared with continuous + covariation gap sequences at fast speeds, and lower at medium speeds. A similar effect can be seen between the continuous and backward gap sequences at fast and medium speeds, where a larger difference in estimate errors is seen at fast compared with medium intervals (Figure 8.2). Finally, the backward and empty gap sequences show a greater difference in estimate errors at slow compared with medium speeds.

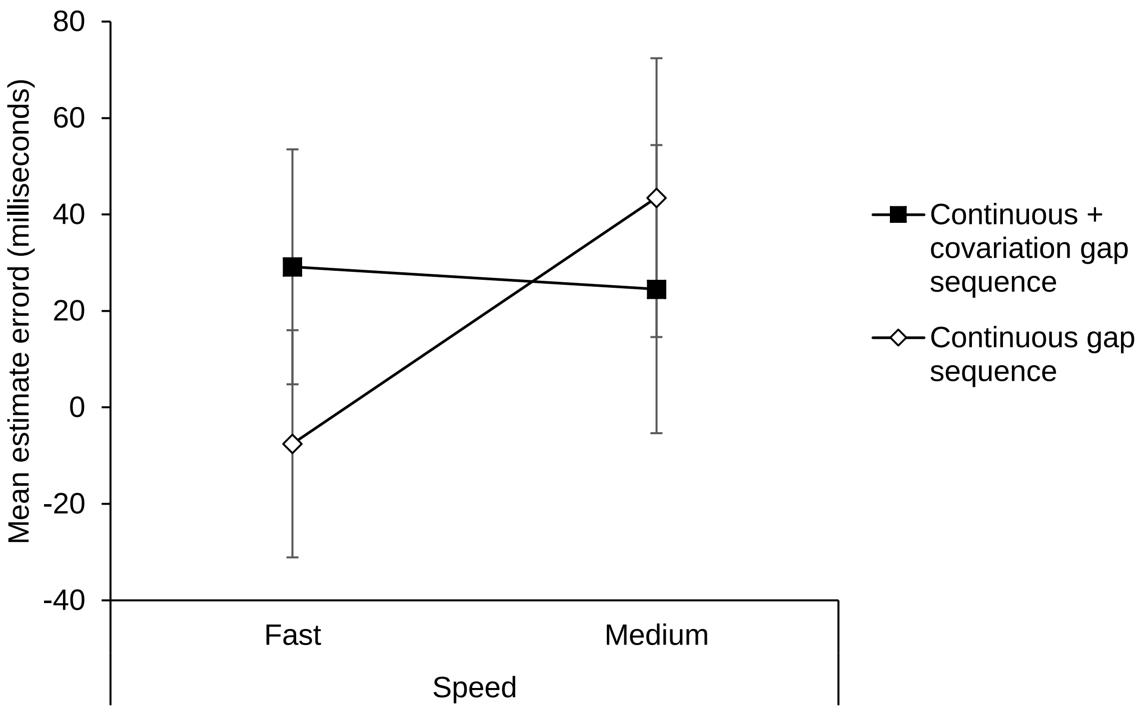

Figure 8.1. Interaction graph: temporal estimate errors by gap sequence type (Continuous + covariation v. continuous) and animation speed (fast v. medium). Error bars represent 95% confidence intervals.

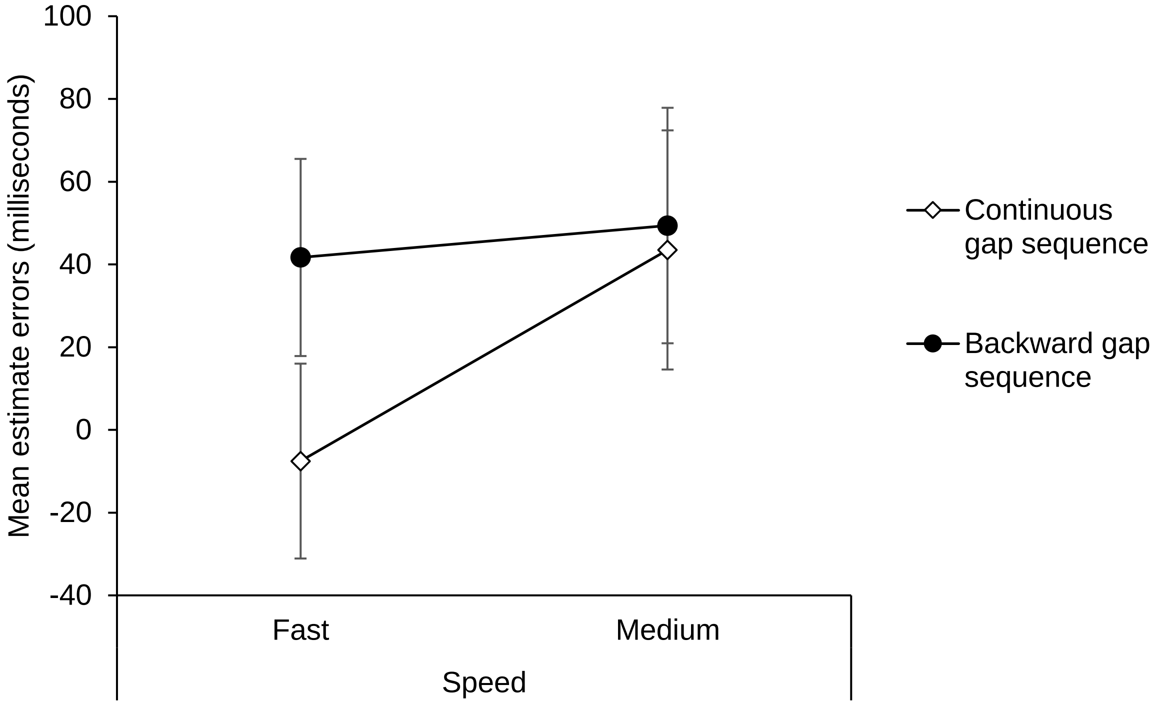

Figure 8.2. Interaction graph: temporal estimate errors by gap sequence type (CCC v. backward) and animation speed (fast v. medium). Error bars represent 95% confidence intervals.

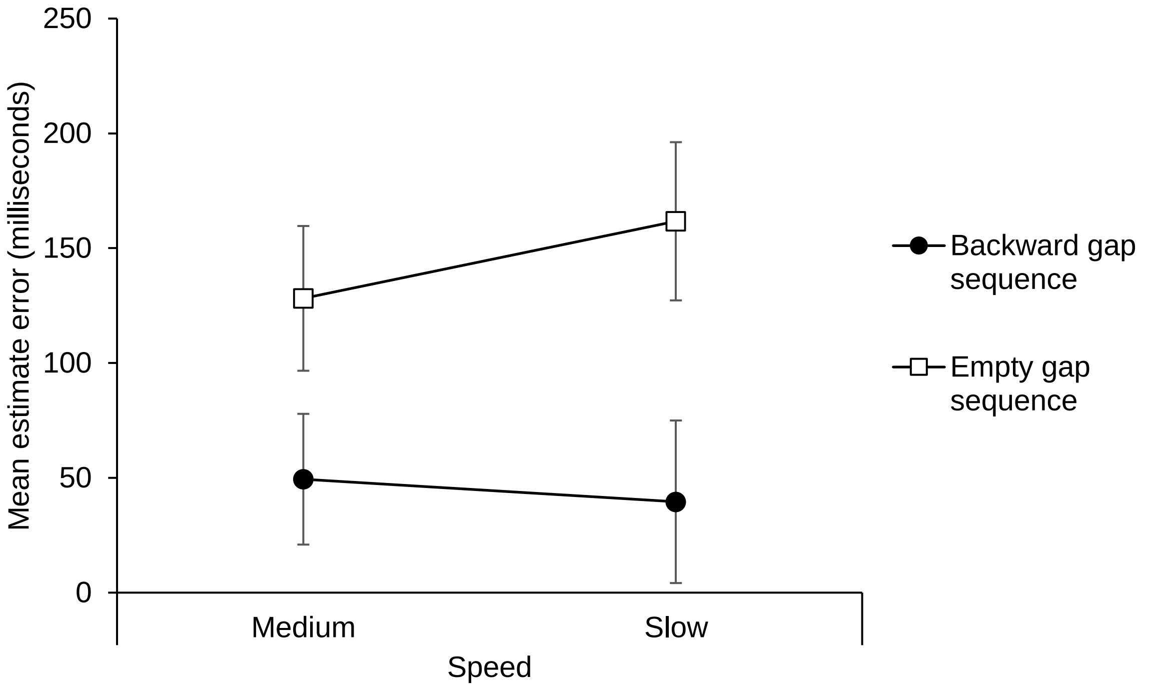

Figure 8.3. Interaction graph: temporal estimate errors by gap sequence type (backward v. empty) and animation speed (medium v. slow). Error bars represent 95% confidence intervals.

8.1.2. Interaction effects in causal ratings: experiment 2

As reported in Chapter 3, section 3.2.2, a significant interaction was found between gap sequence type and animation speed, in the analysis of mean causal ratings. The same planned contrasts used to investigate the main effects were carried out in order to explore this interaction. Contrasts found a significant interaction when comparing the forward and offset gap sequence types and the medium and slow speeds ($F(1, 28) = 4.64$, $p = .04$, $\eta_p^2 = .14$); see Figure 8.4 for an interaction graph. No significant interactions were found when comparing the fast and medium speeds and forward and offset animations ($F(1, 28) = .54$, $p = .47$, $\eta_p^2 = .02$), the fast and medium speeds and offset and backward gap sequences ($F(1, 28) = .24$, $p = .63$, $\eta_p^2 = .008$) or the medium and slow and offset and backward gap sequences ($F(1, 28) = 3.69$, $p = .07$, $\eta_p^2 = .12$). The source of the interaction appears to be the difference in the effect of speed on causal ratings, whereby causal ratings decreased more between the medium and slow speeds in offset gap sequence animations, compared with forward gap sequence animations. However, despite this small interaction effect, clear relationships between gap sequence type and causal impressions, and speed and causal impression, can still be seen.

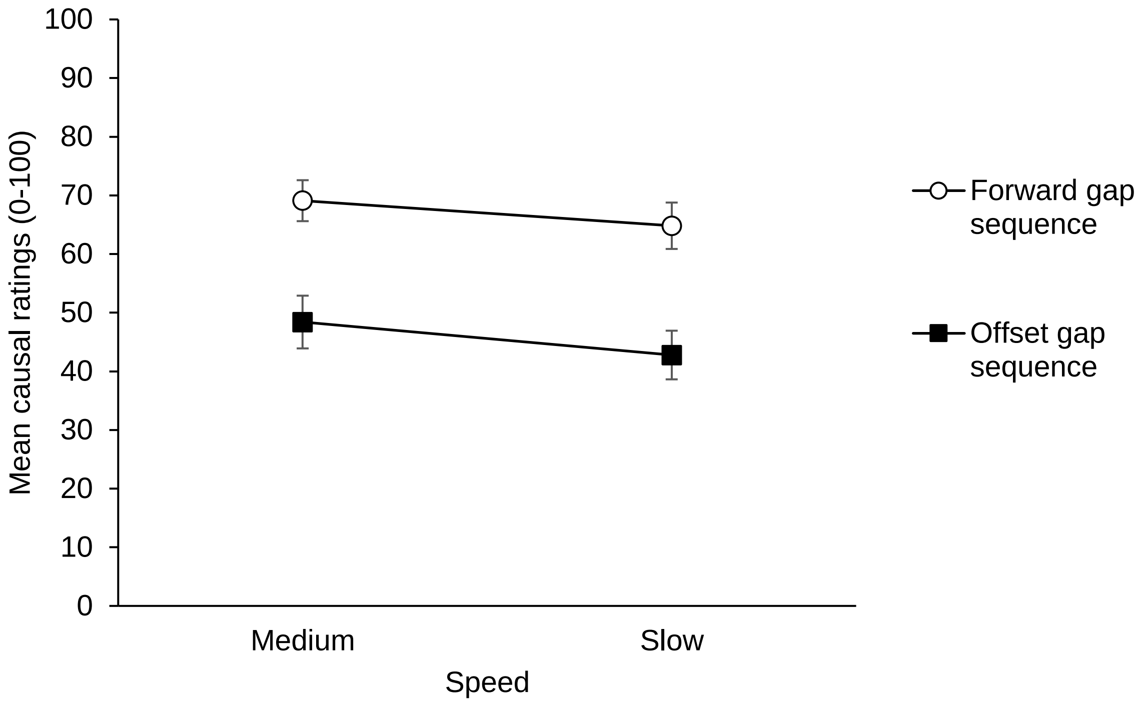

Figure 8.4. Interaction graph: mean causal rating by gap sequence type (forward v. offset) and animation speed (medium v. slow). Error bars represent 95% confidence intervals.

8.1.3. Interaction effects in causal ratings: experiment 3

As reported in Chapter 3, Section 3.5.2, a significant interaction was found between gap sequence type and animation speed in the analysis of causal ratings. This interaction was explored using the same planned comparisons reported for the main effects. A significant interaction was found when comparing the medium and slow animations speeds, and the forward gap sequence and offset gap sequence ($F(1, 25) = 5.95$, $p = .02$, $\eta_p^2 = .19$). Significant interaction effects were not found when comparing the fast and medium speeds and forward and offset gap sequences ($F(1, 25) = .17$, $p = .69$, $\eta_p^2 = .007$), the fast and medium speeds and offset and backward gap sequences ($F(1, 25) = 2.34$, $p = .14$, $\eta_p^2 = .09$) and the medium and slow speeds and offset and backward gap sequences ($F(1, 25) = .54$, $p = .47$, $\eta_p^2 = .02$). The interaction appears to indicate a smaller difference in causal ratings between medium and slow speeds for offset gap sequences than for forward gap sequences (see Figure 8.5 for an interaction graph). As in experiment 2, however, the interaction effect was small and did not appear to alter the overall pattern of findings shown in the main effects.

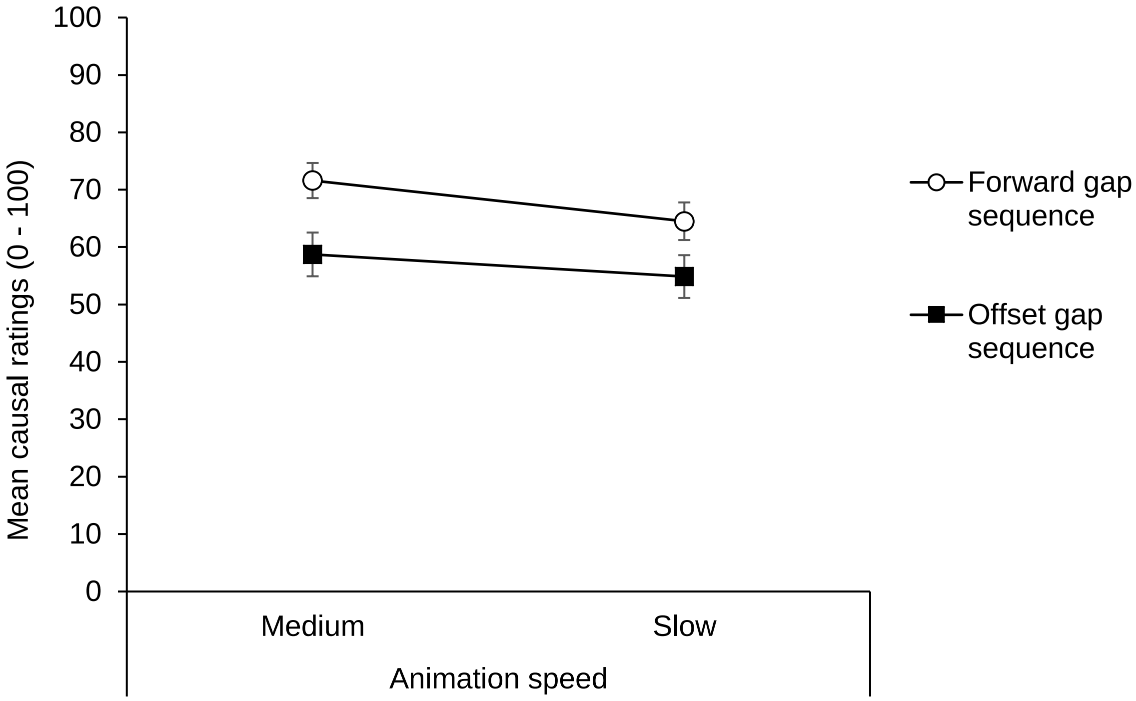

Figure 8.5. interaction graph showing the mean causal ratings by gap sequence type (forward v. offset) and animation speed (medium v. slow). Error bars represent 95% confidence intervals.