How to not act?
Cognitive foundations of intentional nonactions

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Zusammenfassung

Menschliches Verhalten ist im Allgemeinen nicht reizbestimmt, sondern zielgerichtet und hat die Absicht gewünschte Effekte in der Umwelt hervorzurufen. Häufig müssen Menschen eine Handlung ausführen, um diese Effekte zu erreichen. Manche Effekte können allerdings besser oder sogar nur dann erreicht werden, wenn man sich entscheidet nicht zu handeln. Welche mentalen Prozesse finden aber statt, wenn Menschen sich entscheiden nicht zu handeln? Oberflächlich betrachtet scheint es als würde nichts weiter Bemerkenswertes ablaufen, da keine Handlung zu beobachten ist. In drei Experimentalreihen zeige ich aber die kognitiven Prozesse auf, die das Nichthandeln kontrollieren.

In den vorliegenden Experimenten werden Situationen untersucht, in denen sich Menschen entscheiden nicht zu handeln, um vorhersehbare Effekte zu erzeugen. Die Experimente basieren auf der ideomotorischen Hypothese, die annimmt, dass bidirektionale Assoziationen zwischen Handlungen und den resultierenden Effekten gebildet werden können. Dadurch kann eine Vorstellung der Effekte wiederum die verbundene Handlung hervorrufen. Die Ergebnisse zeigen, dass Assoziationen auch zwischen Nichthandlungen und den daraus resultierenden Effekten gebildet werden können. Diese Assoziationen führen dazu, dass die Wahrnehmung der Effekte selbst die Nichthandlung hervorrufen kann (Exp. 1–3). Außerdem scheint die Planung einer Nichthandlung automatisch eine Vorstellung der assoziierten Effekte zu aktivieren (Exp. 4–5). Diese Befunde legen nahe, dass die ideomotorische Hypothese auch auf Nichthandlungen übertragen werden kann und dass Nichthandlungen kognitiv durch die

Effekte, die sie hervorrufen, repräsentiert sind. Darüber hinaus scheinen Menschen ein Verursachungsgefühl (“Sense of Agency”) für die Effekte ihrer Nichthandlungen zu haben (Exp. 6–8). Das bedeutet, dass die resultierenden Effekte (obwohl nicht gehandelt wurde) wie selbsterzeugte Effekte wahrgenommen werden können.

Zusammenfassend zeigen die Experimente, dass intentionale Nichthandlungen von spezifischen Mechanismen und Prozessen begleitet werden, die z.B. bei der Effektantizipation und dem Sense of Agency involviert sind. Obwohl es also von außen so scheint, als würde nichts Bemerkenswertes passieren, wenn Menschen intentional nicht handeln, laufen im Inneren komplexe Prozesse ab wie beim intentionalen Handeln.

Summary

Human actions are generally not determined by external stimuli, but by internal goals and by the urge to evoke desired effects in the environment. To reach these effects, humans typically have to act. But at times, deciding not to act can be better suited or even the only way to reach a desired effect. What mental processes are involved when people decide not to act to reach certain effects? From the outside it may seem that nothing remarkable is happening, because no action can be observed. However, I present three studies which disclose the cognitive processes that control nonactions.

The present experiments address situations where people intentionally decide to omit certain actions in order to produce a predictable effect in the environment. These experiments are based on the ideomotor hypothesis, which suggests that bidirectional associations can be formed between actions and the resulting effects. Because of these associations, anticipating the effects can in turn activate the respective action. The results of the present experiments show that associations can be formed between nonactions (i.e., the intentional decision not to act) and the resulting effects. Due to these associations, perceiving the nonaction effects encourages not acting (Exp. 1–3). What is more, planning a nonaction seems to come with an activation of the effects that inevitably follow the nonaction (Exp. 4–5). These results suggest that the ideomotor hypothesis can be expanded to nonactions and that nonactions are cognitively represented in terms of their sensory effects. Furthermore, nonaction effects can elicit a sense of agency (Exp. 6–8). That is, even though people refrain from acting, the resulting nonaction effects are perceived as self-produced effects.

In a nutshell, these findings demonstrate that intentional nonactions include specific mechanisms and processes, which are involved, for instance, in effect anticipation and the sense of agency. This means that, while it may seem that nothing remarkable is happening when people decide not to act, complex processes run on the inside, which are also involved in intentional actions.

1. Of actions, nonactions and their effects

Pursuing goals and changing the world around us to reach these goals is an essential part of the human nature. Generally, we have to act to reach our goals. Sometimes, however, the omission of an action can be even more fruitful. Imagine the following situation: A four-year-old child is placed alone in a room and has to wait in the room until an adult returns. If the child waits until the adult returns it gets a tasty marshmallow. The child is told that it can also terminate the waiting by ringing a bell, however, then it will only get an average tasty pretzel instead of the marshmallow. The child might thus decide, not to ring the bell, in order to get the strongly desired marshmallow. In such a delay-of-gratification situation, a strongly desired, later reward contrasts with a less desired, but immediate reward and the strongly desired reward can only be obtained by not acting (e.g., Mischel & Ebbesen, 1970; Mischel, Ebbesen, & Raskoff Zeiss, 1972). In pedagogic situations, not acting can also be used to reach a goal, in particular, to evoke a desired behavior in another person. In a dispute, for instance, one person might refuse to respond in order to provoke a certain reaction from the partner. Furthermore, parents who want their child to make own experiences and even own mistakes might refrain from intervening in a given situation to let the child handle the situation on its own. Thus, a **nonaction** (i.e., the intentional omission of an action) can be a mechanism to reach a desired effect.

The fact that nonactions – just like actions – can be used to reach desired effects, raises the question whether similar processes underlie these operations and whether findings in the domain of action control can be transferred to nonactions. The consequences and effects that can be reached by not acting are manifold. They can be long-range consequences, for instance, a parent might help the child to become more self-confident in the long run by not intervening in a given situation. More straightforward consequences of nonactions are the perceptual effects that follow a decision not to act rather directly, like auditory, visual or proprioceptive sensations (e.g., a children's cry). A vast amount of research on actions has found that the direct, perceptual effects of actions (like brightness when a light switch is pressed) are an integral part of action control and an important tool to control and initiate actions. More precisely, it seems that actions are represented in terms of the sensory effect they produce (Shin, Proctor, & Capaldi, 2010). With this in mind, it has recently been proposed that nonactions share these properties with actions and are also represented in terms of the effects they produce (Kühn, Elsner, Prinz, & Brass, 2009). This idea relies on the premise that actions and nonactions are effectively equal, even though actions comprise a specific motor pattern whereas nonactions are defined by the absence of any specific motor pattern. What makes them equal is that they both involve an intentional decision to reach a certain effect.

This dissertation set out to provide a deeper understanding of nonactions and the role of the sensory effects they produce (i.e., *nonaction effects*). Understanding how nonactions are represented is important to provide a full picture of human behavior. However, psychological research in this area is scarce to date. One reason for this is, without much doubt, that nonactions cannot be studied as easily as actions in psychological experiments, because an overt motor response is missing. In this dissertation, I will show that viewing nonactions from the perspective of action control models opens up new possibilities to study nonactions. In the following parts of the introduction, I will therefore elaborate on principles of action control that may inform the study of nonactions.

1.1 Effect-based control of actions

In order to reach a certain goal (a desired effect) we need to choose the correct behavior which produces this effect. But how can the goal to produce a certain effect be translated into one specific motor pattern? Ideomotor theory proposes an elegant solution and suggests that motor patterns and the resulting perceptual effects can be associated with each other in a bidirectional manner, so that anticipating certain effects automatically activates the corresponding motor pattern (see Figure 1). First formulations of this ideomotor principle date back more than 100 years ago (e.g., Harleß, 1861; Herbart, 1825; James, 1890/1981; cf. Pfister & Janczyk, 2012; Stock & Stock, 2004, for historical reviews).

The basis for the idea of an effect-based approach to action control is that all of our movements inevitably produce perceptual effects. For instance, if we knock on a table, we hear a knocking sound, see our hand moving and feel our knuckles on the table. According to ideomotor theory, bidirectional associations can be acquired between any movement and the sensory consequences that reliably follow this movement. When such associations have been acquired, actions can be represented in terms of their sensory effects and anticipation of the sensory effects automatically recollects the motor patterns that used to produce the effects. Several decades after the initial formulation of the ideomotor principle, recent models have reformulated this idea and the predications have been subjected to empirical testing (e.g., Elsner & Hommel, 2001; Greenwald, 1970a, 1970b; Hommel, 1996, 2009; Hommel, Müsseler, Aschersleben, & Prinz, 2001; Kunde, 2001; Prinz, 1997; for a review see e.g., Shin et al., 2010).

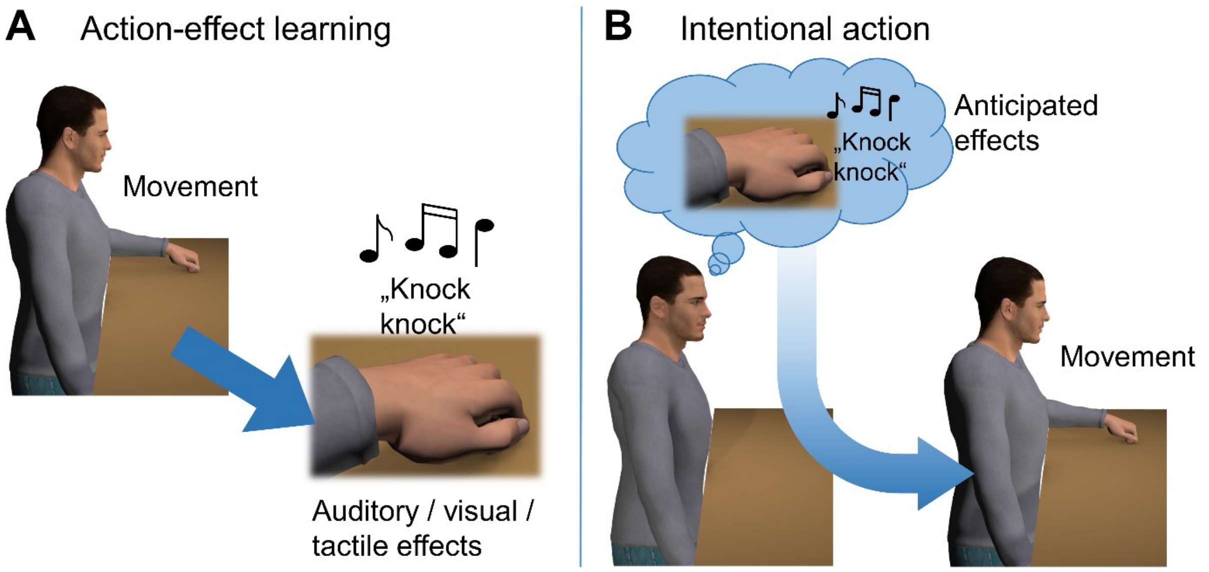

Figure 1. According to ideomotor theory (Herbart, 1825), people can acquire associations between movements (e.g., a knock on the table) and the perceptual effects of this movement (A). These associations are bidirectional. Anticipating the desired effects therefore activates the corresponding motor pattern (B).

The considerable empirical support for effect-based models of action control comes from studies which can broadly be divided into two groups. One group of studies focusses on the learning of new, bidirectional associations between actions and effects. The other group of studies aims to demonstrate that effects are anticipated (i.e., mentally represented) before action execution.

The first group of studies, investigating binding of action and effects, typically uses a two-stage *effect-learning paradigm* (Elsner & Hommel, 2001). In the acquisition phase of this paradigm, participants perform actions which are consistently followed by certain effects. For instance, participants press a left or a right key which will reliably be followed by a task-irrelevant high or low tone. In the subsequent test phase, the former effect tones are presented as target stimuli and participants have to respond fast and accurately to these stimuli with the same left or right keypresses. For different groups of participants, the tone-key mapping either corresponds to the acquisition phase (participants have to react to the former effect tones by using the keys that triggered these tones in the acquisition phase) or the mapping is reversed. The basic idea of the test phase is that, because of the bidirectional associations between actions and effects, the perception of the effects should induce the associated actions (Greenwald, 1970b) and, therefore, performance should be better if the key-tone mapping corresponds to the acquisition phase (Elsner & Hommel, 2001). In line with this assumption, participants generally react faster when using the corresponding mapping compared to the reversed mapping (e.g., Dignath, Pfister, Eder, Kiesel, & Kunde, 2014; Elsner & Hommel, 2001; Hoffmann, Lenhard, Sebald, & Pfister, 2009; Hommel, Alonso, & Fuentes, 2003; Wolfensteller & Ruge, 2011). The impact of action-effect binding cannot only be measured in terms of performance, but also in terms of response choices. Consequently, in the test phase participants then have to respond to the former effect tones by choosing freely one of the actions from the learning phase. Generally, a consistency effect emerges: participants prefer the effect-consistent response (i.e., the action that produced the tone in the acquisition phase) over effect-inconsistent responses (e.g., Eder, Rothermund, De Houwer, & Hommel, 2015; Elsner & Hommel, 2001; Hoffmann et al., 2009; Maes, 2006; Pfister, Kiesel, & Hoffmann, 2011). Taken together, these findings suggest that actions and effects can become bound together in bidirectional associations and perceiving the effects automatically primes the associated action (Elsner & Hommel, 2001).

Ideomotor theory further states that the anticipation (and not the perception) of action effects automatically recollects the corresponding motor action. Therefore, the second group of studies investigates whether representations of action effects indeed occur prior to action execution by using the *action-effect compatibility paradigm* (Kunde, 2001): Participants perform speeded actions to imperative stimuli which reliably trigger certain effects.

Importantly, actions and effects can be classified as compatible or incompatible with each other, that is, they can either share features in a certain dimension (e.g., space) or they do not share these features. For example, compatible effects could occur on the same side as the action (i.e., a left keypress produces a visual effect on the left side of the computer screen or a tone on the left side), whereas incompatible effects would occur in a location opposite to the action. These studies typically find that participants react faster when their responses are (foreseeably) followed by compatible effects rather than incompatible effects (Ansorge, 2002; Kunde, 2001; Pfister, Janczyk, Gressmann, Fournier, & Kunde, 2014; Pfister & Kunde, 2013; Zwosta, Ruge, & Wolfensteller, 2013). Similar results have been found not only for spatial actions effects, but also for other dimensions, such as time (long and short actions and effects; Kunde, 2003) or intensity (soft and forceful actions and effects; Paelecke & Kunde, 2007) and in a variety of action control domains, such as typing (Rieger, 2007), tool use (Kunde, Pfister, & Janczyk, 2012), bimanual coordination (Janczyk, Skirde, Weigelt, & Kunde, 2009), or even social interactions (Müller, 2016; Pfister, Dignath, Hommel, & Kunde, 2013; Pfister, Weller, Dignath, & Kunde, 2017; see Kunde, Weller, & Pfister, 2018, for a corresponding theoretical framework). Altogether, the results of previous studies generally support the predictions made by ideomotor theory, and the studies have used a wide variety of experimental settings, suggesting that the principles of an effect-based action control are applicable to many situations.

1.2 Sense of agency for actions and action effects

Effect-based approaches to actions assume that people anticipate the effects they want to produce, which then trigger the corresponding motor patterns. This idea relies on the premise that the motor actions and effects are causally linked. That is, an action is only successful in reaching the desired effects if these effects are a direct consequence of the action and do not merely coincide with it. Action execution should therefore also come with a process that identifies the action effects as being self-produced. This feeling that one is causing a specific action and the associated effects has been termed the *sense of agency* (Gallagher, 2000; Haggard, 2005). Registering that certain effects are self-produced (and not produced by anyone else), helps to differentiate the self and, thus, the sense of agency represents an integral mechanism of conscious self-perception (Gallagher, 2000; Vosgerau & Newen, 2007).

The sense of agency generally accompanies all our actions, however, it does not necessarily parallel objective causality. Causal relationships between actions and effects are not always obvious and sometimes they have to be inferred. Therefore, it is important to distinguish between an objective causality, i.e., whether an event was actually caused by oneself, and the subjective or perceived sense of agency, i.e., whether one believes or has the feeling that an event was caused by oneself (Hommel, 2015). The subjective sense of agency is influenced by aspects that inform about potential causal relationships, for example, temporal proximity and congruency (e.g., Oestreich et al., 2016; Shanks, 1989; Wegner, Sparrow, & Winerman, 2004; Weller, Schwarz, Kunde, & Pfister, 2017). However, sometimes an exaggerated sense of agency can also emerge for obviously non-causal events (Wegner et al., 2004). An illustrating example for this finding is an observation made by a sociologist named Henslin in the 1960s, who observed cab drivers in St. Louis, USA, for several weeks (Henslin, 1967). Between shifts, he participated in the group activities of the cab drivers and one of these activities was shooting crabs. Henslin observed that the cab drivers seemed to believe that they could control the dice. For instance, one principle was that “a hard throw produced a large number, and a soft or easy throw produced a low number” (Henslin, 1967, p. 319). Even though the taxi driver could not control the dices via the throwing force, they still seemed to feel a sense of control over the outcome of the dice. Observations of this kind, among others, have stimulated extensive research on the sense of agency, investigating how a sense of agency emerges. Different models explaining how the sense of agency is constructed in a given situation have been developed (see Moore, 2016, for a recent review). Below, I will briefly introduce influential models of the sense of agency and summarize basic findings of research on the sense of agency.

One influential approach that has shaped research on the sense of agency comes from comparator models or internal forward models (Blakemore, Wolpert, & Frith, 2000; Frith, 2005; Frith, Blakemore, & Wolpert, 2000). These models suggest that agency relies on predictive processes and assume that when a movement is initiated, information from the movement plan is used to predict the sensory effects of the movement. To that end, an efference copy of the motor command is sent to a forward model, which uses the copy to predict the sensory feedback of the movement. This prediction is then compared to the actual sensory feedback. If prediction and actual feedback match, a sense of agency is felt (see **Figure 2**). Sensory signals that are not self-produced cannot be predicted by the forward model (as there is no motor command). A mismatch between prediction and actual sensory signals therefore indicates that the signals were not self-produced but generated by some external cause.

The idea of motor prediction (predicting action outcomes from the motor command) is derived from models on sensorimotor control (Helmholtz, 1867; Sperry, 1950; von Holst & Mittelstaedt, 1950). These models assume that in order to efficiently control actions, efference copies of a motor command are used to predict action outcomes. That way, sensory effects of a movement can be predicted and compensated, motor planning errors can be identified and actions can be rapidly adjusted if the predicted outcome is undesirable, even before the actual sensory feedback of a particular action arrives. The existence of such a forward model has been supported by numerous studies on sensorimotor control (e.g., Blakemore, Wolpert, & Frith, 1998; Wolpert, Ghahramani, & Jordan, 1995; for a review see Miall & Wolpert, 1996). However, the involvement of the forward model in the sense of agency is still debated (Synofzik, Vosgerau, & Newen, 2008; Waszak, Cardoso-Leite, & Hughes, 2012). Nevertheless, comparator models of the sense of agency can explain a

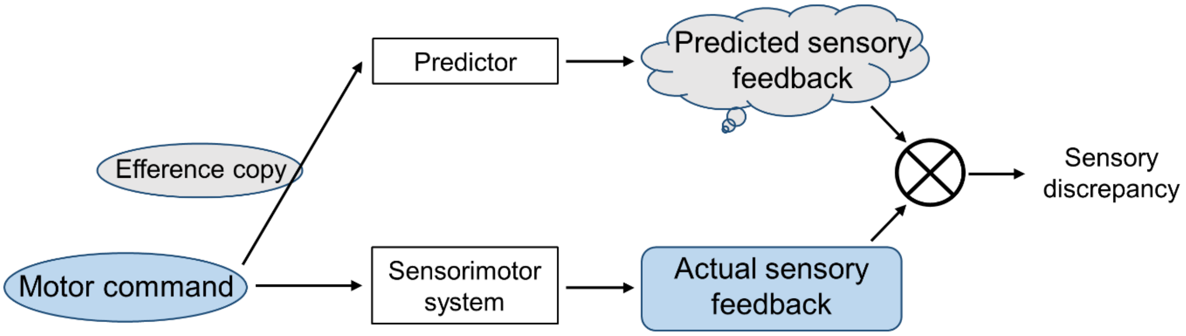

Figure 2. Comparator model of the sense of agency for self-produced sensory effects. Adapted from Blakemore et al. (2000).

range of phenomena, such as the perceptual attenuation of self-produced stimuli (Blakemore et al., 2000; Weiskrantz, Elliott, & Darlington, 1971) and a variety of neuropsychiatric symptoms, for example, delusions of control in schizophrenia (Blakemore, Wolpert, & Frith, 2002).

Another theory that has strongly influenced research on the sense of agency is the theory of apparent mental causation (Wegner, 2002; Wegner & Wheatley, 1999). This theory deals with the question how the conscious will to perform an action is formed and suggests that inferential processes influence the conscious awareness of actions. The underlying idea of this theory is that there is no direct causal link between the conscious thought of an action and the resulting action (including the resulting action effects). Rather, there are two unconscious pathways involved, one triggering a conscious thought and the other triggering an action. These pathways may or may not be linked (see **Figure 3**). People perceive themselves as agents of an action if they “interpret their own thought as the cause of their action” (Wegner & Wheatley, 1999, p. 480). To interpret a thought as the cause of an action, different causal indicators are used: authorship is felt when a thought arises prior to action execution, when the thought is consistent with the action and when no other explanations for the action exist (Wegner, 2002; Wegner & Wheatley, 1999). This approach can explain, why people report a feeling of agency for another person’s actions when they hear consistent instructions about the action before action execution (Wegner et al., 2004), or why people tend to attribute their own actions or the effects of their actions to another person when this person is a plausible cause (Desantis, Roussel, & Waszak, 2011; Wegner, Fuller, & Sparrow, 2003) – these findings cannot readily be explained by comparator models alone.

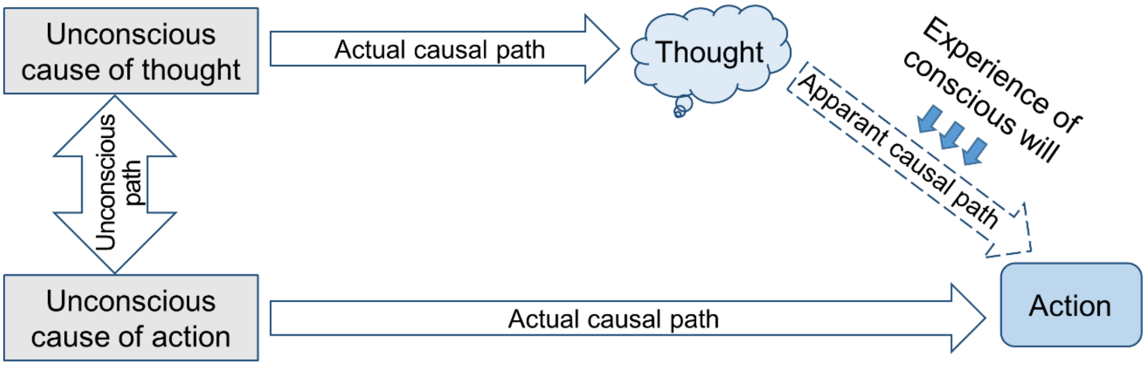

Figure 3. Model of the theory of apparent mental causation. Adapted from Wegner and Wheatley (1999).

Altogether it seems that the sense of agency can be influenced by predictive processes, as proposed by comparator models, and by inferential process, as suggested by the theory of apparent mental causation (see also Moore & Haggard, 2008; van der Weiden, Aarts, & Ruys, 2011). To integrate these different mechanisms, Synofzik et al. (2008) have proposed that the sense of agency is informed from and composed of various different processes. In their model, they distinguish *feelings of agency* from *judgements of agency*. This distinction is also valuable in order to precisely define the rather broad term of *sense of agency*, which is slightly differently defined by different authors (for a discussion see Gallagher, 2012). The feeling of agency has been defined as the low-level, non-conceptual feeling of causing an action and the sensory effects (Synofzik et al., 2008). It is a “rather diffuse sense of a coherent, harmonious ongoing flow of action processing” (Synofzik et al., 2008, p. 228), i.e., the sense of control we normally feel for our actions and the effects of that action when we do not explicitly think about them. At this point, actions and effects can only be classified as self-caused or not self-caused, they cannot be attributed to a specific external agent. The feeling of agency is influenced by low-level, sensorimotor mechanisms, like a comparison between predicted and actual sensory feedback (as proposed, e.g., by comparator models[1]) or a comparison of motor intentions and sensory feedback (without direct involvement of efference copies). The judgement of agency, in contrast, is an explicit, conceptual interpretation of authorship. For the judgement of agency, the feeling of agency is further processed and complemented by higher-order cognitive processes, such as intentions, contextual and social cues, thoughts and knowledge. At this point, actions and their sensory effects can be attributed to a specific agent. To what extent the overall sense of agency is informed by feelings and judgements of agency depends on the specific situation. In an unambiguous context, feelings of agency might suffice to inform our sense of agency, whereas in agent-ambiguous situations belief processes might come into play and judgements of agency could override feelings of agency (Synofzik et al., 2008). In a nutshell, the sense of agency seems to be the result of a dynamic evaluation of predictive and inferential processes.

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[1] Comparator models propose that the predicted sensory effects are estimated by a comparator, which uses an efference copy from the motor command for this estimation (Blakemore et al., 2000). However, ideomotor theory suggests that predicted sensory effects are directly involved in action control (Shin, Proctor, & Capaldi, 2010). Thus, it might in fact not be necessary to assume a new prediction by a comparator.

1.3 Approaching the effect-based representation of nonactions

In a given situation, the decision to refrain from acting can be essential to reach a desired goal. Imagine, for instance, a football game where a striker is in the opposing box and the ball is coming straight towards him. The striker could lift the right foot and shoot, but the opposing keeper is blocking the way to the goal. Thus, the striker decides not to act, but to let the ball pass because he knows that another player is standing in a better position to shoot and make a goal. The striker's decision not to act is followed by foreseeable effects: the visual perception of the ball passing by, maybe a puff of air from the ball, and the sound of the other player shooting and cheering after the goal. This example shows that when people act intentionally they cannot only decide what to do and when to act, but also whether they want to act at all (Brass & Haggard, 2008). That is to say, a nonaction – i.e., the intentional omission of an action – can also be chosen to bring about certain consequences. It has therefore recently been proposed that nonactions share essential properties with actions, especially the notion that they are represented in terms of their sensory effects (Kühn et al., 2009). This idea is based on the ideomotor framework and assumes a great flexibility of the action control system. In order to adapt to all environmental circumstances, it is crucial that any motor pattern can be bound to any sensory effect. This mechanism might be so profound that it can expand to situations where nonactions are the means to reach desired effects.

In contrast to actions, nonactions do not inevitably produce specific effects, but the decision not to act can only produce desired effects, if the circumstances are right. For instance, not acting as a striker can only result in a goal, when another player is standing in the right position to shoot a goal. A nonaction therefore requires a context-dependent anticipation of a desired effect and thus an effect-based representations of nonactions should at least consist of an association between a situation where people decide not to act and the resulting effects. Importantly, even though such an association does not (necessarily) contain any motor-related information, I would still expect that it influences the execution of other actions. This is suggested by findings from previous studies, which were able to show that effect anticipation governs the action selection stage of information processing (Kunde et al., 2012; Paelecke & Kunde, 2007; Wirth, Pfister, Janczyk, & Kunde, 2015). This stage is assumed to be capacity-limited, creating a so-called “bottleneck”, which means that this stage can only be carried out for one task at the same time, in contrast to perceptual or motor processes (McCann & Johnston, 1992; Pashler, 1994; Pashler & Johnston, 1989). Anticipating nonaction effects should thus also create a bottleneck and influence subsequent actions. It is therefore necessary to investigate whether people do form associations with nonaction effects when they decide not to act and whether they represent their nonactions in terms of these effects.

Preliminary evidence indeed suggests that nonactions and their effects can become bound to each other, i.e., that new bidirectional associations between the decision not to act and the resulting effect can be acquired. To investigate these associations, Kühn et al. (2009) used the typical two-stage effect-learning paradigm to investigate nonaction-effect binding (e.g., Elsner & Hommel, 2001; see above). In the acquisition phase, participants could choose between three different responses, a left keypress, a right keypress, or no keypress, which were each followed by specific sounds. In the test phase, the former tones were presented as target stimuli and participants could again choose one of the three responses to react to the tones. Response choices were influenced by the tones and a consistency effect emerged: participants generally preferred the consistent action and, crucially, they also preferred not to act when the former nonaction sound was presented. These findings suggest that nonactions and their effects can become associated with each other in a bidirectional manner, so that perceiving the effects automatically primes not acting (see also Kühn & Brass, 2010a; Röttger & Haider, 2017). Further evidence for binding between nonactions and effects comes from a study using functional magnetic resonance imaging (fMRI; Kühn & Brass, 2010b). In the acquisition phase, participants learned that not acting causes a specific effect tone. Later on, participants performed those nonactions again in an fMRI scanner but, critically, the effect tone was omitted. Nonetheless, the auditory cortex was activated. This finding can be explained by assuming that the sensory effects of not acting were bound to the nonaction and reactivated when the nonaction was performed.

Even though the results of these studies appear convincing at first in suggesting that effects can trigger not acting, the results should be taken with caution. In the fMRI study (Kühn & Brass, 2010b), no effects were actually presented in the test phase, so that effect-specific associations cannot be studied. In the two-stage effect-learning study by Kühn et al. (2009), effects were presented in the test phase and participants preferred not to act whenever the nonaction effect was perceived. This result, however, might also be explained in terms of strategic response choices rather than reflecting actual effect-based priming. In the simplest case, participants might have remembered the (non)action-effect mapping from the acquisition phase and decided to stick with this mapping as a default in the test phase. This decision does not necessarily have to involve ideomotor processes. Thus, convincing evidence for an effect-based representation of nonactions is still lacking.

In the first two empirical parts of this dissertation, I describe critical tests of an effect-based representation of nonaction effects. For one, it should be possible to form new, bidirectional associations between situations where people decide not to act and the corresponding effects, just as associations between actions and effects can be formed (Kühn et al., 2009). This prediction is addressed by experiments described in the first empirical part (Chapter 2, Experiments 1–3). Second, when such associations have been formed and nonactions are represented in terms of their effects, deciding not to act should involve an anticipation of the expected sensory effects (Kühn et al., 2009; Kühn & Brass, 2010b). This prediction is addressed by experiments described in the second empirical part (Chapter 3; Experiment 4 and 5). Taken together, these approaches test critical assumptions of an ideomotor approach to nonactions.

Under the assumption of an effect-based representation of nonactions, one can further assume that nonaction effects should be perceived as being self-produced, since they are integrated into (non)action control. Thus – just like action effects – nonaction effects should elicit a sense of agency. This prediction is addressed by experiments describe in the third empirical part (Chapter 4; Experiments 6–8).

2. Nonaction-effect binding

The three experiments described in this first empirical part were designed to provide unambiguous evidence for the formation of bidirectional associations between nonactions and effects[2]. As explained above, previous results, suggesting that nonactions and their effects can become bound to each other in a bidirectional manner, can also be explained in terms of strategic response choices (Kühn et al., 2009; Kühn & Brass, 2010a). These experiments applied the two-stage effect-learning paradigm: Participants first learned associations between actions and corresponding action effects, as well as nonactions and corresponding nonaction effects. In a following test phase, the former effects were presented as stimuli and participants could choose how to react to these effects. Participants generally preferred the consistent action and they also preferred not to act when the former nonaction effect was presented. The simplest way to explain this consistency effect is to assume that participants remembered the (non)action-effect mapping from the acquisition phase and decided to stick with this mapping in the test phase. This decision does not necessarily have to involve ideomotor processes and might even be issued before presentation of the previous effect stimuli.

Some previous studies on action-effect binding have acknowledged a possible role for such strategies in effect-learning paradigms. A first and straightforward way to address strategic factors has been to eliminate

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[2] Note that this chapter is a modified version of a previously published work: Weller, L., Kunde, W., & Pfister, R. (2017). Non-action effect binding: A critical re-assessment. *Acta Psychologica*, *180*, 137–146. https://doi.org/10.1016/j.actpsy.2017.09.001

participants with implausible (i.e., near-perfect) consistency effects (Eder et al., 2015). Additionally, there are two variations of the test phase that counter strategic factors by design. For one, a secondary task has been implemented in the free choice test phase to deplete the participants’ cognitive resources: Under high cognitive demands participants should be less likely to apply deliberate response strategies, but the action effects should still activate the consistent response. Indeed, results show that the consistency effect persists under high cognitive demands (Elsner & Hommel, 2001, Exp. 4–5). For another, the test phase can also be construed as a forced choice task, as explained in the introduction. To that end, effects from the acquisition phase are presented as imperative stimuli and one half of the participants has to react with the consistent response to the former effects, whereas the mapping is reversed for the other half. Typically, responses are faster if the mapping is consistent rather than inconsistent (e.g., Dignath et al., 2014; Elsner & Hommel, 2001; Hoffmann et al., 2009; Hommel et al., 2003; Wolfensteller & Ruge, 2011) and the small reaction time (RT) differences do not leave time for strategical decisions. This is particularly true when visual actions effects are additionally masked in the test phase to a degree that precludes any deliberate choice strategies (Kunde, 2004).

Forced choice test phases have also been used to corroborate evidence for nonaction-effect binding (Kühn et al., 2009, Exp. 2). But since RTs of nonactions (or the decision not to act) could not be measured, only RTs of actions were analyzed. Faster RTs were observed for the consistent mapping (acting when the former action effect is presented) compared to the inconsistent mapping (acting when the former nonaction effect is presented). However, this RT difference can be explained by action-effect binding alone: Presentation of an action effect activates the corresponding action and, thus, this action is retrieved more easily when the action effect is presented than when it is not presented. Nonaction-effect binding does not necessarily have to be involved. Röttger and Haider (2017, Exp. 3a), thus, expanded the experimental setup and introduced a neutral tone in the test phase. As expected, presentation of the compatible tone facilitated responding and participants reacted faster when the compatible tone was presented compared to the neutral tone. On the other hand, participants reacted slightly slower when the incompatible nonaction tone was presented compared to the neutral tone, suggesting that the nonaction effect hindered responding. Although these results are in line with the assumption that the perception of nonaction effects can activate the corresponding nonaction, these forced choice test phases only provide information about actions and, thus, the facilitation of nonactions via their effects cannot be analyzed. Studies on nonaction-effect binding using a free choice test phase, however, lack critical control conditions to weaken alternative explanations, such as strategy use, for the consistency effect. Thus, the experiments of this first empirical part were designed to scrutinize strategy use in a free choice test phase and to provide unambiguous evidence for nonaction-effect binding while controlling for strategy use.

A related finding of previous studies on nonaction-effect binding was that, generally, participants seemed to prefer acting over not acting – even if they were instructed to aim at an equal distribution of actions and nonactions (Kühn & Brass, 2010a). An unequal distribution of actions and nonactions, however, distorts the typical comparison of the observed frequency of consistent responses to chance (e.g., 50% for a two choice task of action vs. not acting, 33% for a choice between pressing a left key, pressing a right key, and not pressing any key). The relevance of this potential pitfall becomes evident when assessing previous findings that indicated overall choice frequencies to amount to 57% for acting and to 43% for not acting (computed as the mean percentage of action/nonaction choices from the information provided in Kühn & Brass, 2010a, about absolute response frequencies in the acquisition and test phases). This statistical effect likely biases the assessment of nonaction-effect binding and should therefore be taken into account when analyzing consistency effects for actions and nonactions.

2.1 General approach of the present experiments

In the three experiments[3] described in this first part, participants completed an acquisition phase to associate actions and nonactions with specific effects (visual effects in Experiment 1; auditory effects in Experiment 2-3), following previous methods. In the subsequent test phase, participants reacted to the former effects and were free to choose between effect-consistent or effect-inconsistent (non)actions.

In Experiment 1 and 2, I used an experimental design that closely resembled the setup of Kühn et al. (2009, Exp. 1) and examined if participants used deliberate response strategies in this setup. As a first indicator of deliberate strategies, I identified participants who showed an implausibly large consistency effect. According to ideomotor theory, (non)action effects should prime the consistent response, however, other response tendencies can influence response selection as well (e.g., tendencies toward repetition or alternation; Elsner & Hommel, 2001), so that the amount of consistent choices should be substantially lower than 100%. This assumption is supported by previous studies on action-effect binding, which showed mean consistency effects of only up to 64% for two-choice test phases (Elsner & Hommel, 2001; Hoffmann et al., 2009; Pfister et al., 2011). Therefore, participants were excluded in the present experiments if they chose the consistent response in more than 75% (given the fact that participants could choose between three rather than two potential responses in the present setup, the exclusion criterion

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[3] Data and analysis scripts of these three experiments are publicly available via the Open Science Framework at https://osf.io/mhrjg.

of 75% largely exceeds the mean consistency effect of up to 64% of previous studies).

Evidently, choosing the consistent response is not the only possible response strategy. Therefore, I examined the data further to detect other potential strategies. Two additional strategies suggested themselves. First, participants could also deliberately choose an inconsistent mapping, which would reduce the possibility to find evidence for nonaction-effect binding. Data from such participants would also distort the assessment of (non)action-effect binding and therefore also participants who chose an inconsistent response in more than 75% of the cases were excluded. Second, I supposed that allowing participants to freely choose between acting (left and right keypresses) and not acting might encourage some participants to switch between an action mode (a sequence of trials where participants respond to the color effect with left or right keypresses) and a nonaction mode (a sequence of trials where participants can lean back and relax without pressing a key). Such a strategy would minimize the time during which participants have to stay alert, while at the same time allowing them to comply with the instructions, namely to use all three responses. Furthermore, such a strategy is not discouraged in the experiments. However, this strategy is easily revealed by the resulting data, as participants should show long trial sequences with only actions (left and right keypresses) or nonactions and can thus be excluded. To assess the impact of strategies, I analyzed the data of each experiment twice: once using the data of all participants and once using only the data of participants who were not identified as using strategies. I expected a preference for effect-consistent response choices in both groups.

In Experiment 3, I introduced a secondary task to prevent the use of response strategies while also controlling for strategies as in Experiment 1 and 2. Participants had to complete a mouse-tracking task while listening and reacting to the effect sounds from the acquisition phase. Even under dual-task conditions, the sound should activate the associated response, leading to an overall preference for consistent responses (Elsner & Hommel, 2001).

2.2 Experiment 1

The experiment was set up to replicate the consistency effect for actions and nonactions, while examining if participants used response strategies. In an acquisition phase, participants were allowed to choose a left keypress, a right keypress, or no keypress. Each response was consistently followed by a colored effect on the computer screen. In the test phase, participants' task was to react spontaneously to the former color effects with one of the three responses.

In order to take unequal overall preferences for actions and nonactions into account, I baseline-corrected the frequency of consistent choices. To that end, I calculated the frequency of consistent choices for each effect (e.g., the number of left keypresses divided by the number of trials with the left action effect) and the global frequency of each response (e.g., the number of left keypresses divided by the total number of (correct) trials in the test phase). Then, I subtracted the global frequency from the frequency of consistent choices for each response and participant. If participants preferred the consistent response, this difference should be substantially higher than zero. This approach takes into account that participants might prefer left and right actions over not acting (Kühn & Brass, 2010a; see above). Following the idea that actions and nonactions are similarly represented in terms of their effects (Kühn et al., 2009), a systematic preference for consistent choices should be visible in the baseline-corrected frequencies of both actions and nonactions.

The data of each participant was examined to check if participants had used a specific response strategy. I used two different approaches to check for response strategies. For one, I examined if participants' data indicated that they had used a specific color-response mapping (e.g., if they had always pressed the left key when they saw the former left color-effect). As a cut-off, participants were excluded when they chose to respond to a specific color with the same response (either consistent or inconsistent) in more than 75% of the trials. For another, I examined if participants showed extraordinarily long sequences of trials with only actions (left or right keypresses) or only nonactions and excluded participants if the longest or even more extreme sequences were highly unlikely ($p < .0001$).

Data analyses were performed twice, once using the entire set of participants and once using the subset of participants who were not identified as using response strategies. If nonactions and effect can indeed become associated with each other in a bidirectional manner, a preference for consistent responses should be visible in both groups, the whole data set (comparable to the results of Kühn et al., 2009) and, crucially, also in the subset when controlling for strategy use.

2.2.1 Method

Participants

Thirty-three participants were recruited (mean age = 27.8, *SD* = 7.5, 8 male, 3 left-handed). All participants gave informed consent and received either course credit or monetary compensation for participation. An a priori power analysis based on the results of Kühn et al. (2009, Exp. 1) suggested that a sample size of n = 9 ensured a power of 0.8 to detect nonaction-effect binding (with $d_z = \frac{t}{\sqrt{n}} = \frac{3.98}{\sqrt{12}} = 1.15$), and a sample size of n = 20 for action-effect binding (with $d_z = \frac{2.35}{\sqrt{12}} = 0.68$). In order to have sufficient power to show both action-effect and nonaction-effect binding, I decided to collect data of at least twenty participants who did not use any strategies.

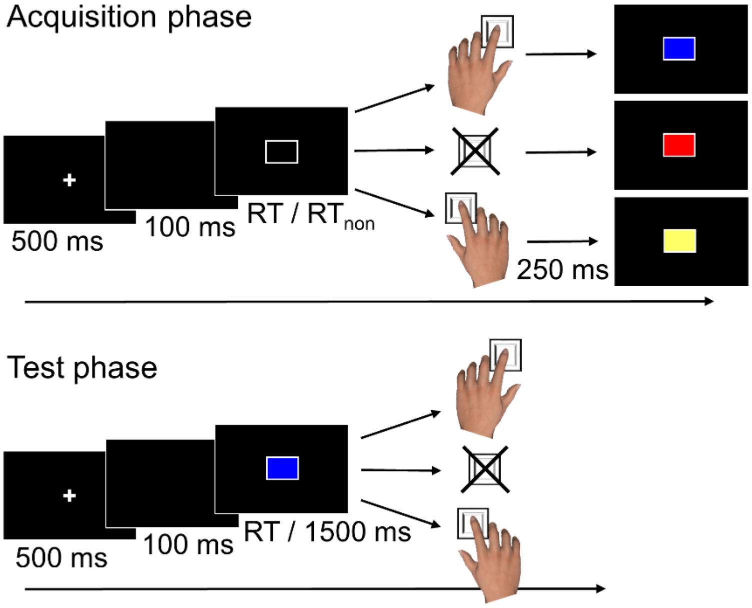

Figure 4. Setup of Experiment 1. In the acquisition phase, participants chose between a left or right keypress (reaction time; RT), and no keypress (RT_{non}) and thereby triggered a contingent color effect. In the test phase, the former color effects were presented and participants reacted to the color stimuli by choosing between a left keypress, a right keypress, and no keypress.

Stimuli and experimental setup

Participants were seated in front of a 17'' computer monitor at a viewing distance of approximately 60 cm. They operated the *V* key of standard German QWERTZ keyboard with their left index finger and the *N* key with their right index finger. Colored rectangles (red, blue and yellow) of 3 x 4 cm were used as action-effects and appeared in the center of the screen on a black background.

Experimental procedure

Participants received written instructions at the beginning of the experiment. The acquisition and test phase were introduced one after another and participants could practice each phase. The phases were named *phase A* and *phase B* in the instructions. Figure 4 depicts the experimental setup. The experiment consisted of thirteen blocks in total, seven acquisition blocks (A) and six test blocks (B). The block order for all participants was AAAAABBABBABB. Participants were allowed to take a break between blocks.

For the acquisition phase, participants were informed that in each trial they should produce one of three responses, either a keypress with their left index finger (key *V*), a keypress with their right index finger (key *N*) or no keypress. Participants were encouraged to produce each response equally often within one block and received feedback about the frequency of each response after every block. Each acquisition block consisted of 45 trials. Acquisition trials started with a white cross against a black background, which was displayed for 500 ms, followed by a blank screen for 100 ms. Then, a white-framed rectangle with a black filling appeared, signaling participants to choose one of the three responses. The participants' responses filled the rectangle with a response-specific color. For each participant, the response-color mapping remained constant throughout the experiment, but the mapping was randomized across all participants. If participants chose a left or right keypress, the color changed 250 ms after the keypress. The RT history of the participants' left and right keypresses was used to determine when to present the nonaction color. Therefore, the participant's RTs of keypresses within the current block were saved and the interval between onset of the white rectangle and the nonaction color was calculated as (mean RT + mean RT + last RT) / 3 + 300 ms (cf. Kühn & Brass, 2010a). If no RT history was available (e.g., in the first trial of each block) the nonaction interval was set to 1500 ms. If no keypress occurred in this interval, the nonaction effect color was displayed. In follow-up questionnaires participants reported that they had indeed had the impression of causing the nonaction effect. The next trial started after an intertrial interval of 1000 ms. If participants pressed a key before presentation of the white rectangle, as well as during or after presentation of the color effect, an error message occurred immediately and a new trial started. All trials containing such errors were excluded from analysis.

Test blocks consisted of 45 trials each. Trials started with a white cross, which was displayed for 500 ms, followed by a blank screen for 100 ms. Then, one of the three colored rectangles of the acquisition phase appeared within a white frame, signaling participants to choose one of the responses. Participants were instructed to respond spontaneously to the color without using any specific strategy. After 500 ms the color disappeared but the white-framed rectangle (now filled black) remained on the screen for another 1000 ms. If participants had not pressed a key within these 1500 ms after color onset, a nonaction was registered. To discourage participants from deciding for and pre-planning a response before the color onset, an error message was displayed whenever participants responded before or within 200 ms after color onset. If participants pressed a key after the white rectangle had disappeared (i.e., 1500 ms after color onset), an error message appeared, informing participants that they had responded too late and encouraging them to respond faster in the next trial. The next trial started after an intertrial interval of 1000 ms.

Statistical analysis

To ensure that participants had the opportunity to establish links between action, nonaction and the associated effects, the number of valid trials per response-effect pairing in the acquisition phase was assessed and participants were excluded if the number of valid trials was below 75 for at least one response-effect pair (this applied to one participant).

Then, the baseline-corrected frequency of consistent choices was computed. To that end, I calculated the global frequency of each response in the test phase for every participant (e.g., the number of left keypresses divided by the total number of correct test trials) and the frequency of consistent choices for each response and participant (e.g., the number of left keypresses divided by the number of trials with the left action-effect). Then, I subtracted the former from the latter. These baseline-corrected frequencies of consistent choices were tested against zero using two-tailed, one-sample *t*-tests in order to test if participants chose the consistent response significantly more often than chance would suggest. Effect sizes were calculated as $d_z = \frac{t}{\sqrt{n}}$.

A repeated-measures analysis of variance (ANOVAs) with the within-subject factor response (left keypress, right keypress, no keypress) was used to test whether global response frequencies differed between responses. If the assumption of sphericity was violated, Greenhouse-Geisser corrections were applied and corrected *p*-values along with original degrees of freedom and the corresponding ε estimate for correcting degrees of freedom are reported. To analyze if the baseline-corrected frequencies of consistent choices differed between actions and nonactions, the data of both action effects (left and right keypress effects) were averaged and a two-tailed, paired *t*-test was computed. In case of a non-significant test, I computed the Bayes factor according to the BayesFactor package of the R software environment to further analyze the data (BF greater than 3 were considered evidence for one hypothesis over the other; Rouder, Speckman, Sun, Morey, & Iverson, 2009).

RTs of actions (left and right keypresses) were also analyzed using a repeated-measures ANOVA with the within-subject factor tone relation. The factor tone relation comprised the levels compatible (i.e., a left action effect responded to by a left keypress and right action effect responded to by a right keypress), incompatible (right action effect ► left keypress; left action effect ► right keypress) and nonaction (nonaction effect ► left keypress; nonaction effect ► right keypress). For the RT analysis, all trials with errors, as well as all trials following errors and all trials deviating more than 2.5 standard deviations from the cell mean were excluded.

All statistical analyses of the test phase were performed twice, once on the whole data set, and once on a subset of participants who were not identified as using deliberate response strategies. In order to decrease the effect of response strategies, I applied the following criteria to determine which participants entered the subset: Participants were classified as using strategies if they used a predefined stimulus-response mapping. To that end, I computed the relative frequency of each stimulus-response pair for each participant and the data of participants with any of these frequencies exceeding 75% were discarded. This criterion identified seven participants, who were not included in the subset analysis. Five of these participants predominantly chose a consistent mapping. Furthermore, participants were also classified as using strategies if they showed implausibly long sequences of trials with only actions or only nonactions. To identify those participants, I inspected the participants' data of the test phase and chose the longest sequence of successive trials with only actions (left and right keypresses) and only nonactions. Assuming a Bernoulli process under the assumption of no strategy use, I calculated the probability of a trial sequence – P(action) for action sequences and P(nonaction) for nonaction sequences – with at least this length according to the binomial distribution (as described in the formulas below). The participant's overall frequency of actions or nonactions in the test phase served as an estimate for the probabilities p_{action} and $p_{\text{nonaction}}$, respectively, and k represents the length of the trial sequence:

$$P(\text{action}) = p_{\text{action}}^{k}$$

$$P(\text{nonaction}) = p_{\text{nonaction}}^{k}\ .$$

A trial sequence was considered implausible if the probability was less than 10^{-4}. This criterion identified five participants who were not included in the subset analysis.

2.2.2 Results

In the acquisition phase, participants produced all required responses with a substantial frequency (on average 34.2% left keypresses, 33.9% right keypresses, 31.9% nonactions), but the frequencies of responses differed significantly, $F(2,62) = 7.43$, $p = .003$, $\eta_p^2 = .19$ ($\varepsilon = 0.82$). In the test phase, participants responded too early on 3.9% of the trials (i.e., they pressed a key before or within 200 ms after color onset) and too late on 0.5% of the trials. These trials were excluded from further analyses. **Figure 5** shows response frequencies for left keypresses, right keypresses and nonactions as a function of the presented color effect, both for the whole set of participants and the reduced subset of participants, who were not identified as using response strategies. Global response frequencies and frequencies of consistent choices are also listed in **Table 1**.

Analysis of the entire sample (n = 32)

In the test phase, participants chose the left keypress in 33.2% of the trials, the right keypress in 34.8% of the trials and no keypress in 32.0% of the trials and these global response frequencies did not differ significantly from each other, $F(2,62) = 2.24$, $p = .131$, $\eta_p^2 = .07$ ($\varepsilon = 0.74$). Participants preferred the consistent response when the action effects of the left keypress, $t(31) = 2.45$, $p = .020$ $d_z = 0.43$, and the right keypress were presented, $t(31) = 2.94$, $p = .006$, $d_z = 0.52$. Importantly, a consistency effect also emerged when the nonaction effect was presented, $t(31) = 2.68$, $p = .012$, $d_z = 0.47$. The baseline-corrected frequency of consistent choices did not differ between action effects and the nonaction effect, $t(31) = 0.05$, $p = .960$, $d_z = 0.01$ and a Bayes factor of $B_{01} = 7.29$ indicated evidence in favor of the null hypothesis of equally strong consistency effects.

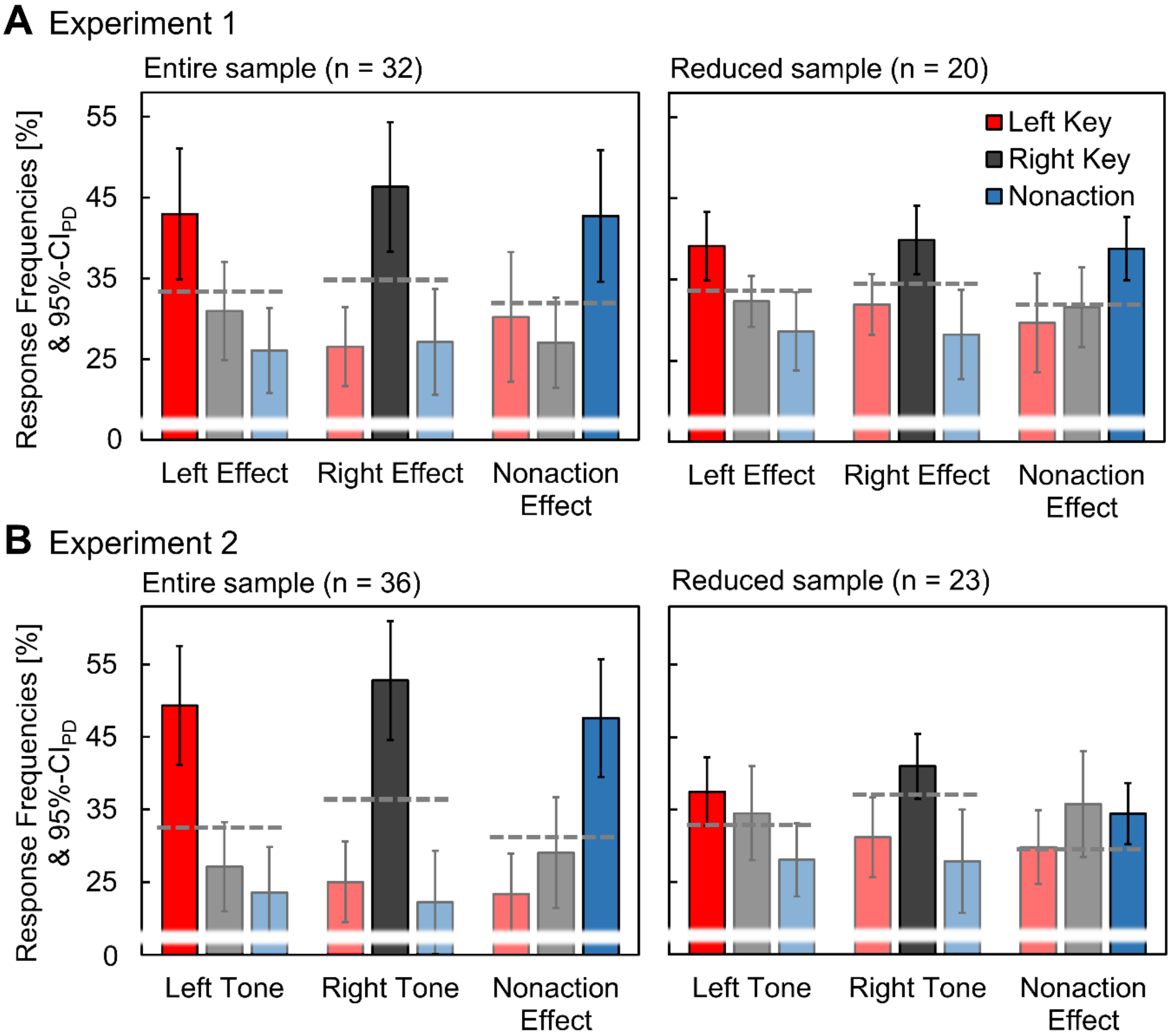

Figure 5. Mean response frequencies in the test phase in response to the effects of the acquisition phase for Experiment 1 (panel A) and Experiment 2 (panel B). Graphs on the left show the data of the entire sample, whereas graphs on the right show the data of the reduced sample without participants who showed signs of strategy use. Dashed gray lines indicate the mean global frequency of left keypresses (left line in each graph), right keypresses (middle line) and nonactions (right line). Error bars indicate the 95%-confidence interval of paired differences (CI_{PD}) for the comparison of consistent and inconsistent left action, right action and nonaction frequencies to the respective baseline frequencies (cf. Pfister & Janczyk, 2013).

For RT analysis, one participant had to be excluded because of missing values in one cell. RTs for actions following a compatible action effect (*M* = 458 ms), an incompatible action effect (*M* = 448 ms) or the nonaction effect (*M* = 447 ms) did not differ from each other, $F < 1$.

Analysis without response strategies (n = 20)

In the subset of participants without detected response strategies, the left keypress was chosen in 33.5%, the right press in 34.6% and the nonaction in 31.9% of the trials in the test phase and these global response frequencies did not differ from each other, $F < 1$. As in the whole data set, the percentage of consistent choices was significantly greater than chance for the left action effect, $t(19) = 2.77$, $p = .012$, $d_z = 0.62$, the right action effect, $t(19) = 2.62$, $p = .017$, $d_z = 0.59$, as well as for the nonaction effect, $t(19) = 3.70$, $p = .002$, $d_z = 0.83$. The baseline-corrected frequency of consistent choices did not differ between action effects and the nonaction effect, $t(19) = 1.36$, $p = .189$, $d_z = 0.30$ and the calculated Bayes factor of $B_{01} = 2.50$ provided ambiguous support for the null hypothesis (with numerically stronger consistency effects for the nonaction effect).

RTs for actions following a compatible action effect (*M* = 415 ms), an incompatible action effect (*M* = 400 ms) or the nonaction effect (*M* = 414 ms) differed from each other, $F(2,38) = 3.67$, $p = .035$, $\eta_p^2 = .162$. Two-tailed, paired *t*-tests showed that participants reacted faster following an incompatible action effect compared to a compatible action effect, $t(19) = 2.39$, $p = .027$, $d_z = 0.53$, and compared to the nonaction effect, $t(19) = 2.79$, $p = .012$, $d_z = 0.62$. RTs did not differ following a compatible effect and the nonaction effect, $t(19) = 0.26$, $p = .795$, $d_z = 0.06$.

Table 1. Mean global response frequencies and mean frequencies of consistent choices (standard error of the mean) of actions and nonactions in the test phase for Experiment 1–3.

	Left Key		**Right Key**		**Nonaction**	
	Global	Consistent	Global	Consistent	Global	Consistent
Experiment 1						
Entire sample (n = 32)	33.2% (0.6)	43.0% (3.9)	34.8% (0.6)	46.3% (4.0)	32.0% (0.9)	42.7% (4.2)
Reduced sample (n = 20)	33.5% (0.9)	39.1% (2.1)	34.6% (0.9)	39.9% (2.3)	31.9% (1.4)	38.8% (2.5)
Experiment 2						
Entire sample (n = 36)	32.5% (0.9)	49.3% (4.2)	36.4% (1.4)	52.8% (4.2)	31.1% (1.6)	47.6% (4.3)
Reduced sample (n = 23)	32.8% (1.1)	37.5% (2.7)	37.1% (1.8)	41.0% (2.8)	30.1% (1.6)	34.4% (2.7)
Experiment 3						
Entire sample (n = 28)	51.2% (1.1)	55.5% (2.2)	-	-	48.8% (1.1)	53.2% (1.6)
Reduced sample (n = 23)	50.8% (1.2)	53.7% (2.0)	-	-	49.2% (1.2)	52.2% (1.7)

2.2.3 Discussion

The purpose of Experiment 1 was to investigate whether typical measures of nonaction-effect binding might be affected by deliberate response strategies and, if this was the case, to assess nonaction-effect binding when these strategies are controlled for. About one third of the participants did indeed show clear signs of strategy use, suggesting that the nature of the free choice task does prompt some participants to rely on response strategies rather than spontaneous response selection. This indicates that evidence for (non)action-effect binding in common experimental designs is confounded with participants' use of such deliberate strategies. Still, in the present experiment a consistency effect was found not only in the entire set of participants, but also when analyzing only the subset of participants who were not identified as using response strategies. Furthermore, Bayes factors indicated that the frequency of consistent choices was not smaller for nonactions than for actions. The results are in line with the assumption that participants acquired nonaction-effect associations in the acquisition phase and that presentation of the nonaction effect activated the nonaction in the test phase.

The results of the response strategy analysis indicate that in common experimental studies on (non)action-effect binding, a substantial number of participants does not answer spontaneously, as instructed, but according to a deliberate response strategy. However, the high number of participants using strategies in the current experiment could also be due to the visual effects used in Experiment 1, because these effects come with low saliency and may therefore invite participants to focus their attention on other aspects of the task (such as deliberate strategies). Auditory stimuli (as used by e.g., Elsner & Hommel, 2001; Kühn et al., 2009) draw attention more automatically than visual stimuli (Posner, Nissen, & Klein, 1976) and might create a more engaging situation in which participants do not rely as strongly on using explicit strategies. To test this assumption, auditory stimuli were used in Experiment 2.

Finally, RTs following compatible, incompatible and nonaction effects did not differ in the analysis of the entire sample. That is in line with previous studies using the free choice test phase (Elsner & Hommel, 2001; Hoffmann et al., 2009; Pfister et al., 2011). In the subset, however, participants reacted faster to incompatible action effects compared to both compatible action effects and the nonaction effect. This result is unexpected in the light of ideomotor theory and it is at odds with studies using compatible and incompatible effects as imperative stimuli (e.g., Dignath et al., 2014; Elsner & Hommel, 2001; Hoffmann et al., 2009; Wolfensteller & Ruge, 2011). However, because this result did not replicate in the following Experiment 2, I am cautious to draw any conclusions from this effect.

2.3 Experiment 2

In Experiment 2, I attempted to replicate Experiment 1 with auditory action and nonaction effects. At the same time, I assumed that auditory action effects would reduce the number of participants who use response strategies.

2.3.1 Method

Participants, stimuli and experimental setup

Forty participants were recruited (mean age = 27.4 *SD* = 6.7, 14 male, 1 left-handed). Considering a dropout rate of about one third due to strategy use (as in Experiment 1), a sample size of 40 ensured that at least 20 participants should remain in the group of participants without response strategy use (the necessary sample size to detect action and nonaction-effect binding based on the results of Kühn et al., 2009; see also Experiment 1). All participants gave informed consent and received either course credit or monetary compensation for participation.

The experimental setup and trial procedure were identical to Experiment 1 except for the following changes. Participant wore headphones to listen to the sound effects, which were delivered binaurally and consisted of three different animal sounds (a dog barking, a cat meowing, and a bird chirping) with durations between 522 and 862 ms. In the acquisition phase, after participant's response the corresponding sound effect was played instead of the colored action effect of Experiment 1. In the test phase, participants heard one of the former effect tones and simultaneously the white-framed rectangle appeared, signaling participants to choose one of the response alternatives. The white-framed rectangle remained on the screen for 2000 ms in total. If participants had not pressed a key within 1500 ms after tone onset, a nonaction was registered.[4]

Statistical analysis

Statistical analysis was identical to Experiment 1 (see statistical analysis in *Section 2.2.1* for details). In total, 13 participants were identified as using deliberate response strategies. Three participants showed an implausibly long sequence of nonaction trials and ten additional participants used a predefined stimulus-response mapping (nine of these participants predominantly chose the consistent response).

2.3.2 Results

Four participants were excluded from all analyses because the number of valid trials in the acquisition phase was below 75 for at least one response-effect pair. All other participants produced the required responses about equally often (on average 33.5% left keypresses, 33.5% right keypresses, 33.0% nonactions) in the acquisition phase. The response frequencies did not differ, $F < 1$. In the test phase, participants responded too early on 1.5% and too late on 0.4% of the trials. These trials were excluded from further analyses. Response frequencies for left keypresses, right keypresses and nonactions as a function of the presented effects are shown in **Figure 5**, both for the whole set of participants and the reduced subset of participants, who were not identified as

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[4] Due to a programming error, the white rectangle remained on the display for another 500 ms after the participant's response was counted as a nonaction. This was different from Experiment 1, were the white rectangle disappeared after 1500 ms, informing participants that a nonaction had be registered.

using response strategies. Global response frequencies and frequencies of consistent choices for both analyses are also listed in **Table 1**.

Analysis of the entire sample (n = 36)

In the test phase, participants chose a left keypress in 32.5% of the trials, a right keypress in 36.4% of the trials, and no keypress in 31.1% of the trials. These differences between global frequencies were marginally significant, $F(2,70) = 2.87$, $p = .080$, $\eta_p^2 = .08$ ($\varepsilon = 0.74$). Participants preferred the consistent response when the left action effect was presented, $t(35) = 4.17$, $p < .001$ $d_z = 0.70$, when the right action effect was presented, $t(35) = 4.07$, $p < .001$ $d_z = 0.68$, and when the nonaction effect was presented, $t(35) = 4.12$, $p < .001$ $d_z = 0.69$. The baseline-corrected frequency of consistent choices did not differ between action-effects and the nonaction effect, $t(35) = 0.08$, $p = .934$, $d_z = 0.01$, and a Bayes factor of $B_{01} = 7.69$ indicated evidence in favor of the null hypothesis of equally strong consistency effects.

For the RT analysis, one participant had to be excluded because of missing values in one cell. RTs for actions following a compatible action effect ($M = 683$ ms), an incompatible action effect ($M = 683$ ms) or the nonaction effect ($M = 650$ ms) did not differ from each other, $F(2,68) = 1.94$, $p = .164$, $\eta_p^2 = .054$ ($\varepsilon = 0.76$).

Analysis without response strategies (n = 23)

The subset of participants who were not identified as using response strategies chose a left keypress in 32.8%, a right keypress in 37.1%, and no keypress in 30.1% and these global frequencies differed significantly, $F(2,44) = 3.60$, $p = .036$, $\eta_p^2 = .14$. Furthermore, participants preferred the nonaction when the nonaction effect was presented, *t*(22) = 2.12, *p* = .046 d_z = 0.44. The preference of consistent choices for actions was only marginally significant, *t*(22) = 2.05, *p* = .053, d_z = 0.43 and *t*(22) = 1.80, *p* = .085, d_z = 0.38, for the left and right action effect respectively. However, the baseline-corrected frequencies of consistent choices did not differ between action effects and the nonaction effect, *t*(22) = 0.03, *p* = .976, d_z = 0.01, and a Bayes factor of B_{01} = 6.25 indicated evidence for the null hypothesis of equally strong consistency effects. RTs for actions following a compatible action effect (*M* = 674 ms), an incompatible action effect (*M* = 659 ms) or the nonaction effect (*M* = 662 ms) did not differ from each other, *F* < 1.

2.3.3 Discussion

In Experiment 2 participants preferred not to act when the former nonaction effect was presented. As in Experiment 1, this was true in both analyses, indicating that nonactions and their effect became associated with each other. However, the number of participants that were classified as using response strategies was not reduced compared to Experiment 1, indicating that strategy use introduces a strong confound in common experimental designs of (non)action-effect binding.

When analyzing only those participants who did not use strategies, the consistency effect for actions (left and right keypresses) was only marginally significant, although Bayes factors indicated that the relative frequency of consistent choices was equally high for actions and nonactions. As previous studies have found convincing evidence for action-effect binding (e.g., Dignath et al., 2014; Elsner & Hommel, 2001; Hoffmann et al., 2009; Hommel et al., 2003; Pfister et al., 2011; Wolfensteller & Ruge, 2011), the present results appear to stem from a Type II error. However, they also suggest that response strategies do indeed inflate consistency effects in typical free choice designs and should be carefully controlled for also in studies on action-effect binding.

The rather high amount of participants using response strategies in Experiment 1 and 2 also indicates that simply measuring response strategies is not a particularly economic approach. As outlined in the introduction of this chapter, however, a complementary method to control for response strategies can be implemented by changing the design of the test phase. High cognitive demands can reduce the participants’ ability to use deliberate response strategies. However, as the influence of action and nonaction effects on response selection should be automatic, the consistency effect should persist even under higher cognitive demands (Elsner & Hommel, 2001). Therefore, an additional task was implemented in the test phase of Experiment 3.

2.4 Experiment 3

In Experiment 3, a secondary task in the test phase was used to prevent the use of response strategies by design. The acquisition phase was similar to Experiment 1 and 2, but participants could only choose between two responses, a keypress with their left hand or no keypress, which were consistently followed by specific tones. In the test phase, the former effect tones were presented as stimuli and again, participants were allowed to respond either with a keypress or by not pressing the key. Simultaneously, participants performed a mouse-tracking task with their right hand. As the mouse-tracking task should induce a distraction from the free choice task, less or no participants should use explicit response strategies in the present test phase. Still, I expected a reliable consistency effect for actions and nonactions alike.

2.4.1 Method

Participants

Twenty-eight participants (mean age = 21.2, *SD* = 5.3, 4 male, 3 left-handed) were recruited for the experiment. Based on the mean effect size computed from all four effect size estimates for nonaction-effect binding determined in Experiment 1 and 2, a sample size of 28 ensured a power above $1–\beta = .8$ to detect nonaction-effect binding. All participants gave informed consent prior to the experiment and received either course credit or monetary compensation for participation.

Stimuli and experimental setup

Participants sat in front of a 22’’ flat screen monitor at a viewing distance of approximately 60 cm and operated the *C* key of standard German QWERTZ keyboard with their left index finger and the mouse with their right hand (one left-handed participant used the mouse with the left hand and operated the *C* key with the right index finger). Stimuli appeared in the center of the screen on a black background. A high-pitched and a low-pitched MIDI tone (dulcimer timbre) of 500 ms duration served as sound effects and were delivered binaurally through headphones to the participants.

Experimental procedure

The acquisition phase and test phase were introduced separately by written instructions at the beginning of the experiment. The phases were named *phase A* and *phase B* in the instructions and participants were allowed to practice each phase separately at the beginning of the experiment and pose questions. The experimental setup is depicted in **Figure 6** (Panel A). Acquisition blocks (A) comprised 50 trials per block and test blocks (B) comprised 30 trials per block. The block order for all participants was AAAABBBABBB.

In the acquisition phase, participants were requested to choose between two responses, a keypress or no keypress. Keypresses were performed with the index finger of the hand that participants normally do not use to operate the mouse (i.e., in most cases the left hand). The experimental procedure of the acquisition phase was identical to Experiment 1 and 2, with the exception that no blank screen was presented between the fixation cross and the white rectangle.

In the test phase, participants had to complete two tasks, a mouse-tracking task and a free choice task and the instructions encouraged participants to prioritize the mouse-tracking task. For the mouse-tracking task, a small circle (radius: 50 pixels, corresponding to 2.5 cm) with blue circumference was displayed on the screen and moved randomly to the left or to the right with a constant horizontal velocity (176 px/s). Participants' task was to track the circle with the mouse in a way that the mouse cursor remained within the circumference. For the free choice task, one of the effect tones of the acquisition phase was presented as a stimulus. Participants had to respond to this tone with one freely chosen response, either a keypress or no keypress. Participants were told to use both responses about equally often and not to use any specific strategies to choose between keypress and no keypress, but to choose spontaneously. Both tasks had to be handled simultaneously. This is, each trial started with a display of the circle and the mouse cursor in the center of the screen for 500 ms. Then, the circle began to move and participants had to track the circle. After a randomly chosen delay of 1000 to 3000 ms, one of the former effect tones was presented, prompting participants to choose a response. The mouse-tracking task lasted five seconds in total. Between trials, a black screen was shown for 1000 ms. If participants pressed a key before tone presentation, an error message was displayed and a new trial started. Additionally, participants received warning messages if they produced unequal amounts of actions or nonactions (more than 75% of the trials with only one type of response). After the experiment, participants completed an additional questionnaire about whether they had used specific strategies to choose between keypress and no keypress in the acquisition and the test phase.

Statistical analysis

Statistical analysis was similar to Experiment 1 and 2 (see statistical analysis in *Section 2.2.1*). To analyze if participants preferred the consistent response, for each participant the global frequency of each response in the entire test phase was subtracted from the frequency of consistent choices for this response. Two-tailed, one-sample *t*-tests were used to evaluate if these baseline-corrected frequencies of consistent choices were greater than zero. Two-tailed, paired-sample *t*-tests were used to test whether participants preferred one of the responses in the acquisition and the test phase and whether the baseline-corrected frequencies of consistent choices differed between response alternatives.

RTs of keypresses were also analyzed using a two-tailed, paired *t*-test, comparing the RTs of keypresses following the (compatible) action effect and of keypresses following the nonaction effect. For the RT analysis, all trials with errors, as well as all trials following errors and all trials deviating more than 2.5 standard deviations from the cell mean were excluded.

Statistical analyses were again performed twice, once on the entire data set, and once on the subset of participants who were not identified as using response strategies. In the post-experimental questionnaire, four participants reported that they had used strategies throughout the test phase. Using the strategy criteria from Experiment 1 and 2, two participants were identified as using deliberate response strategies. One of these participants chose a consistent response in more than 75% of the trials and also indicated so in the questionnaire. The other participant also chose the consistent response in more than 75% of the trials and additionally showed an unnatural long sequence of nonactions. However, that participant did not indicate strategy use in the post-experimental questionnaire. Nevertheless, all five participants were excluded from the subset analysis[5].

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[5] One participant indicated that he or she had not used strategies in the entire test phase, but only occasionally. Further exclusion of this participant from the subset showed that participants still preferred the consistent response for the action effect, $p_{\text{one-tailed}} = .043$, and the nonaction effect, $p_{\text{one-tailed}} = .044$.

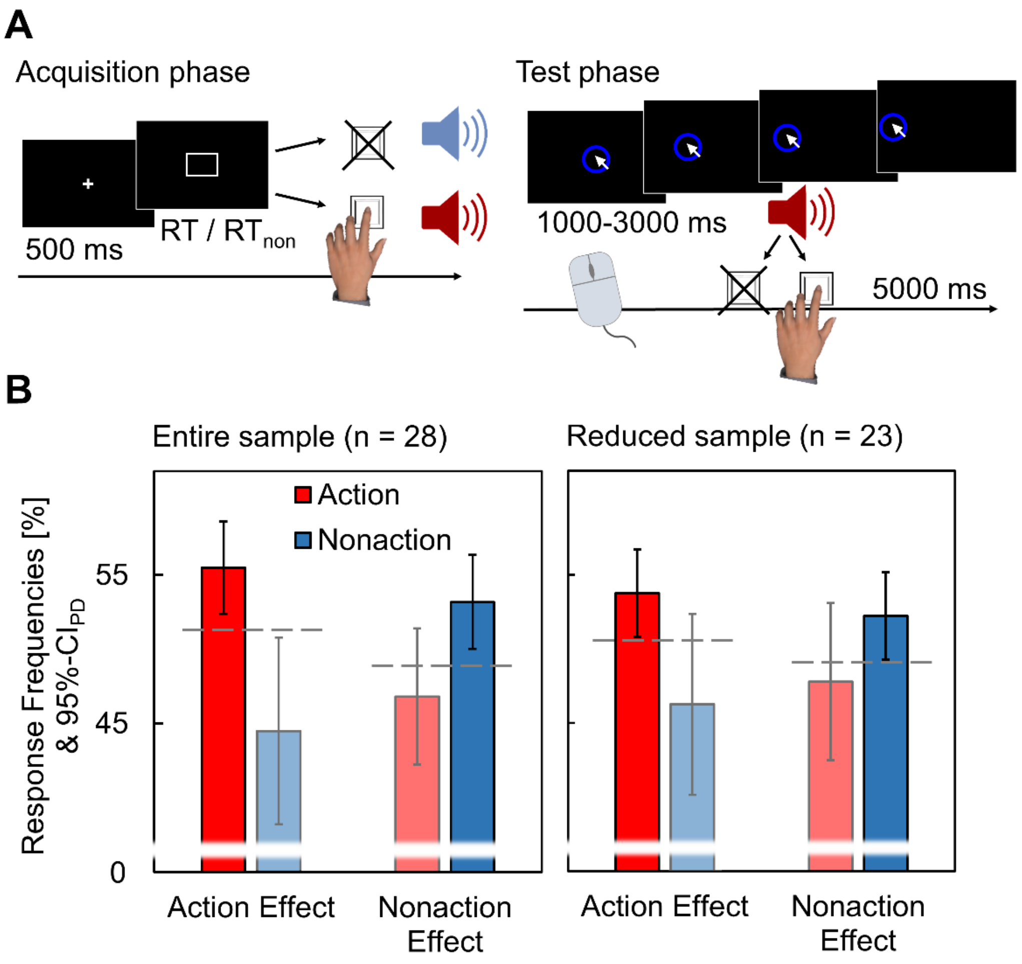

Figure 6. Setup and results of Experiment 3. (A) In the acquisition phase, participants chose a keypress or no keypress and thereby triggered a contingent sound effect. In the test phase, they completed a mouse-tracking task, during which one of the former sound effects was presented. Participants reacted to the sound by choosing a keypress or no keypress. (B) Mean response frequencies of actions and nonactions in the test phase in response to the previous sound effects. The left graph shows data of the entire sample, whereas the right graph shows data of the reduced subset of participants who did not show signs of strategy use. Dashed gray lines indicate the mean global frequency of actions (left line in each graph) and nonactions (right line). Error bars indicate the 95%-confidence interval of paired differences (CI$_{PD}$) for the comparison of consistent and inconsistent action and nonaction frequencies to the respective baseline frequencies (cf. Pfister & Janczyk, 2013).

2.4.2 Results

In the acquisition phase, keypresses (51.2%) were performed more often than nonactions (48.8%), $t(27) = 2.17$, $p = .039$, $d_z = 0.41$. In the test phase, participants responded too early on 1.5% of the trials. These trials were excluded from further analyses. Response frequencies for keypresses and nonactions as a function of the presented effects are shown in **Figure 6** (Panel B), both for the whole set of participants and the reduced subset of participants. Global response frequencies and frequencies of consistent choices in the test phase are listed in **Table 1**.

Analysis of the entire sample (n = 28)

Participants chose to press a key in 51.2% of the valid trials, nonactions in 48.8% and these global frequencies did not differ, $t(27) = 1.06$, $p = .297$, $d_z = 0.20$. Participants preferred the consistent response for both, the action effect, $t(27) = 2.84$, $p = .008$, $d_z = 0.54$, and the nonaction effect, $t(27) = 2.83$, $p = .009$, $d_z = 0.53$. The baseline-corrected frequencies of consistent choices did not differ between response alternatives, $t(27) = 0.75$, $p = .457$, $d_z = 0.14$ and a Bayes factor of $B_{01} = 5.21$ indicated evidence for the null hypothesis of equally strong consistency effects. RTs for keypresses following the action effect (M = 709 ms, SE = 28.6) did not differ from RTs following the nonaction effect (M = 714 ms, SE = 30.0), $t(27) = 0.38$, $p = .710$, $d_z = 0.07$.

Analysis without response strategies (n = 23)

Keypresses were performed in 50.8% of the valid trials, nonactions in 49.2% and these global frequencies did not differ, $t(22) = 0.64$, $p = .528$, $d_z = 0.13$. Participants preferred the consistent response for both, the action effect, $t(22) = 2.10$, $p = .047$, $d_z = 0.44$, and the nonaction effect, $t(22) = 2.10$, $p = .048$, $d_z = 0.44$. The baseline-corrected frequencies of consistent choices did not differ between response alternatives, $t(22) = 0.63$, $p = .533$, $d_z = 0.13$ and a Bayes factor of $B_{01} = 5.16$ indicated evidence for the null hypothesis of equally strong consistency effects. RTs for keypresses following the action effect (*M* = 700 ms, *SE* = 33.2) did not differ from RTs following the nonaction effect (*M* = 708 ms, *SE* = 34.0), *t*(22) = 0.45, *p* = .659, d_z = 0.09.

Mouse data

The mean deviation of the mouse cursor from the circle center was 11.9 pixel (*SE* = 0.2), which was smaller than the width of the circle radius (50 pixel). Participants' individual mean deviations were also below 50 pixels with a range from 6.7 to 28.6 pixels.

2.4.3 Discussion

Experiment 3 set out to corroborate evidence for nonaction-effect binding by preventing the use of response strategies with a dual-task setting in the test phase. Participants now performed a mouse-tracking task in the free choice test phase, which should induce higher cognitive demands so that participants are not able to use response strategies or to keep track of the strategies. Despite the additional task, participants clearly favored the consistent response for both action and nonaction. This indicates that nonactions and their effects became associated in the acquisition phase and that presentation of the nonaction effect in turn activated the nonaction in the test phase.

The additional task also successfully reduced strategy use. In the post experimental questionnaires only four participants indicated that they had used response strategies throughout the test phase and using the criteria of Experiment 1 and 2, only two participants were identified as using strategies. Introducing a secondary task thus proves to be a helpful tool to reduce strategy use in common free choice designs.

2.5 General discussion and interim conclusion

The three experiments presented in this chapter re-assessed the hypothesis that nonactions, i.e., intentional decisions not to act, can become bound in a bidirectional manner to the effects they produce. The experiments were divided into two phases. In the acquisition phase, participants could freely decide between pressing a key or not. Actions and nonactions were both consistently followed by specific and contingent effects. In the test phase, these effects were presented as stimuli and participants were again allowed to choose between acting and not acting. If actions and nonactions can become associated with their respective effects in the acquisition phase, this should lead to a preference for consistent action and nonaction choices in the test phase. The critical question was whether such a preference is driven by automatic influences of action and nonaction effects (as could be derived from ideomotor theory) or by explicit strategical considerations.

Experiment 1 and 2 showed that common free choice test phases are prone to strategy use and that evidence for (non)action-effect binding as provided by these free choice tasks is at least partly driven by strategy use. However, the experiments also showed reliable consistency effects when strategy use was controlled for by excluding participants, as in Experiment 1 and 2, or by introducing a secondary task, as in Experiment 3. The results therefore confirm that nonactions can become bound to the sensory effects they produce.

As a final methodological concern, however, preferences for not acting when the nonaction effect is presented could still be explained without the necessity to assume nonaction-effect binding. More precisely, a preference not to act when the nonaction effect is presented could also stem from the fact that participants prefer not to act unless a stimulus (i.e., a former action effect) activates an action. Even though the overall choice preferences of the participants might be taken as first evidence against this alternative explanation, it is also possible to directly assess its validity using data from the current experiments: The frequency of nonactions in the acquisition phases provides an estimation for participant’s default selection of nonactions. Pooling the data of all three experiments showed that the frequency of consistent nonaction choices in the test phase was considerably higher than the frequency of nonaction choices in the acquisition phase both, for the entire group of participants, $t(95) = 4.59$, $p < .001$, $d_z = 0.47$, and for the group of participants that was not identified as using response strategies, $t(65) = 2.60$, $p = .012$, $d_z = 0.32$. This indicates that a preference for nonactions in response to nonaction effects does not stem from a default preference for not acting but does indeed reflect nonaction-effect binding.

The experiments further showed that the frequency of consistent choices was equally high for nonactions and actions, indicating that associations between nonactions and their effects might be as strong as associations between actions and their effects. This is in line with the idea that nonactions and actions are represented in the same way, namely, via the sensory effects they produce (Kühn et al., 2009). The present state of evidence is thus suggestive of an effect-based representation of nonactions.

3. Nonaction effect anticipation

The experiments of the previous chapter indicated that bidirectional associations between nonactions and their effects can be formed. Thus, they provided initial evidence for the claim that nonactions, just like actions, can be represented in terms of their sensory effects. Another prediction that can be derived from this claim is that preparing a nonaction should involve an anticipation of the respective sensory effects, given that associations between nonactions and effects have been formed (Kühn et al., 2009; Kühn & Brass, 2010b). That is to say, not (only) the presentation of nonaction effects should prime not acting (as shown in the previous chapter), but rather the anticipation of nonaction effects should activate not acting. To test this prediction, an experiment has to be designed where the nonaction effects are presented only after a participant's reaction. That way, any influence of the effects has to be due to the anticipation of these effects.

Tentative evidence for an anticipation of nonaction effects comes from a previous fMRI study (Kühn & Brass, 2010b). In a free choice learning phase, participants learned to associate specific sound effects with actions and nonactions. In a subsequent test phase, participants were also allowed to choose between actions and nonactions on some trials and on other trials, a no-go cue forced participants not to act. However, the sound effects were no longer presented in the test phase. Still, the auditory cortex was significantly more active for freely chosen actions and nonactions compared to the no-go condition. These results can be explained along the lines of ideomotor theory, and the activation of the auditory cortex might reflect the anticipation of the respective sound effects during actions and nonactions (Kühn & Brass, 2010b). Even though the source and identity of the cortical activation cannot be determined in detail, this finding is in line with an effect-based representation of nonactions. The experiments of this second empirical part were designed to provide a more direct test of nonaction effect anticipation.

In studies on action control, effect anticipation is often investigated using the action-effect compatibility paradigm (Kunde, 2001; see also *Section 1.1*). In this paradigm, participants perform speeded actions to imperative stimuli and these actions reliably trigger certain effects. Importantly, actions and effects can be classified as compatible or incompatible to each other. For example, compatible effects could occur on the same side as the action, whereas incompatible effects would occur on the opposite side of the action. Participants typically react faster when their actions are (foreseeably) followed by compatible effects rather than incompatible effects. Because the action effects are only presented after action initiation, these findings indicate that effects are anticipated and influence action selection and planning (e.g., Ansorge, 2002; Kunde, 2001, 2003; Pfister et al., 2014; Pfister, Kiesel, & Melcher, 2010; Wirth, Pfister, Brandes, & Kunde, 2016; Zwosta et al., 2013).

A similar setting needs to be designed to investigate the anticipation of nonaction effects. This is not trivial, however. Since nonactions are characterized by the absence of any (specific) motor pattern, the timing of nonactions cannot be measured easily. Therefore, only the timing of actions can be examined in an action-effect compatibility experiment and only the influence of nonaction effects on actions can be analyzed. If nonactions are indeed represented in terms of their effects, anticipating nonaction effects should facilitate the nonaction, whereas it should not facilitate acting and might even hinder acting. In an action-effect compatibility design, one would therefore expect that participants perform actions slower when they anticipate a nonaction effect rather than the compatible action effect. However, this very result pattern can also be explained without assuming nonaction effect anticipation, as action initiation might be generally facilitated when a compatible action effect is anticipated, compared to a situation where no distinct, external action effect is anticipated.

A different experimental setting might be better suited to test the anticipation of nonaction effects. In the study of Kunde, Hoffmann, and Zellmann (2002, Exp. 2 and 3), participants had to prepare one action and then rapidly switch to a different action which could either result in the same effect as the prepared action or in a different effect. More precisely, participants had to complete two tasks. First, they saw a stimulus for task A and were instructed to prepare a corresponding keypress for this stimulus, but not to execute the keypress yet. Then, the stimulus for task B was presented and participants had to respond to this stimulus as fast and accurately as possible with a different keypress. Response B was followed immediately by a consistent, foreseeable sound effect. Only after participants had responded to task B, they were allowed to execute the prepared response A and response A was also followed by a consistent, foreseeable sound effect. Importantly, the sound effects of response A and response B could either be the same or different. The underlying idea of this paradigm was that preparing a certain action A should activate the corresponding sound effect. Because this activation can be considered a time-consuming process, preparing action A should to a certain degree also facilitate the initiation of an action B with the same effect, as the effect is already activated. In contrast, an action B with a different effect should not be facilitated. In line with that assumption, participants were generally faster to switch to an action B with the same rather than a different effect. This paradigm can be adapted to investigate nonaction effect anticipation by replacing the prepared action with a nonaction. Preparing a nonaction and switching to an action B with the same effect should be easier than preparing a nonaction and then switching to an action B with a different effect.

A fundamental difference between actions and nonactions is that actions comprise a specific motor pattern whereas for nonactions the specific motor pattern is absent. Preparing a nonaction might therefore have different consequences for a subsequent action than preparing an action. Consequently, nonactions might produce a different result pattern than actions in the present experimental setup. More precisely, it could also be easier to switch from a prepared nonaction to an action with a different effect rather than an action with the same effect. This depends on how the decision not to act influences other motor activity. So far, it is not clear what exactly the decision not to act comprises and how it affects the body, since *not acting* is not characterized by a specific motor pattern that could become activated, but rather by the absence of a (specific) motor pattern. There are, however, several possibilities how the decision not to act could exert influence on other motor activity. Deciding not act could, for instance, activate an alternative action which is compatible with the nonaction (Kühn & Brass, 2010a). For example, deciding not to press down a key with the index finger could result in the contrary action of lifting the index finger. On the other hand, the decision not to act could also result in a general deactivation of all activity, indicated by reduced corticospinal excitability, similar to the rapid stopping of an action (Badry et al., 2009). It is even plausible to assume that a variety of different mechanisms can potentially be associated with nonactions and that it depends on the specific situation how the decision not to act affects other motor-related activity. If the decision not to act results in a general deactivation of all activity, one could assume that switching to an action with a different effect ends this general deactivation of activity, whereas switching to an action with the same effect does not. In this case, it should be easier to switch from a prepared nonaction to an action with a different effect rather than an action with the same effect. However, this should not be the case for actions, as the activation of a specific action should not necessarily be coupled to a deactivation of all other actions. So, the result pattern for nonactions could differ from the result pattern that is expected for actions. Even though advantages as well as disadvantages can be expected when switching from a prepared nonaction to an action with the same rather than a different effect, any difference can still be attributed to an anticipation of the nonaction effects, as these effects are only presented after action initiation.

3.1 Experiment 4

Experiment 4 aimed to investigate the anticipation of nonaction effects. To that end, the experimental design of the study by Kunde et al. (2002) was slightly adapted. Participants completed two tasks (task A and task B) and in these tasks they had to respond to imperative stimuli with one of three different keypresses (i.e., actions) or a nonaction. First, they saw a stimulus for task A (a geometrical figure) and were instructed to prepare a corresponding keypress or the nonaction depending on the stimulus. Shortly afterwards, the stimulus for task B was presented (a digit ranging from 1 to 4). Participants had to immediately react to this stimulus and they heard the corresponding sound effect after their response. As soon as they heard the sound effect, they had to execute the prepared response A, which was also followed by the corresponding sound effect. Task A and task B could result in the same sound effect or two different sound effects, but they never required the same response.

In the study by Kunde et al. (2002), the stimulus onset asynchrony (SOA) between the stimuli for task A and task B was also manipulated. The results showed that response B was more strongly influenced by the prepared response A when SOA between tasks was relatively short, indicating that the sound effects had a stronger influence on earlier rather than later phases of action preparation. In the present study, it was not possible to manipulate the SOA between tasks within subjects, because that would have resulted in an unreasonably high number of trials. (Note that the trials of interest – trials in which a nonaction is prepared and participants switch to an action with the same or different effect – can make up only one fourth of the total amount of trials to ensure that all responses are used equally often and that all transitions from response A to response B occur equally often in the experiment.) To get an idea of the influence of SOA in the present experiment, participants were thus divided into two groups, one with a short and one with a long delay between stimulus A and stimulus B.

In order to investigate the anticipation of nonaction effects, associations between nonactions and effects have to be available and these associations have to be acquired within the experiment before they can be tested. Participants therefore completed an acquisition phase prior to the experimental phase. In the acquisition phase, participants chose one response in each trial (one of the three keypress actions or the nonaction) and the response was consistently followed by a sound effect. This sound effect was identical to the sound effect of the main experimental phase and two responses each shared the same sound effect. The procedure of the acquisition phase was derived from previous experiments on nonaction-effect binding (Kühn & Brass, 2009; Kühn & Brass, 2010a, 2010b; Röttger & Haider, 2017), but it was shortened compared to these studies to keep the experiment (which had a rather long and complex experimental phase) feasible. This initial acquisition phase with single (non)action-effect episodes was followed by three training blocks of the experimental phase to acquaint participants with the experimental task[6]. Additionally, these first three blocks of the experimental phase served as a further pseudo-acquisition phase. In total, the acquisition phase was practically of comparable length to the acquisition phases of previous studies.

Assuming that associations are learned in the acquisition phase, I expected that participants would be faster to initiate a response for task B when they had prepared an action with the same rather than a different effect for task A. I also expected that participants' reactions for task B would be influenced by

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[6] In a pilot study, participants reported difficulties with the experimental task, especially in the beginning of the experiment. This was reflected in a high number of error trials in the first blocks of the experiment.

the effect correspondence between a prepared nonaction in task A and the executed response B, but I did not make any prior assumption about the direction of this influence. The influence of effect correspondence should be further modulated by SOA. Regarding participants’ reactions in task A, I expected that responses in task A would not be influenced by the correspondence of effects, in line with the results of Kunde et al. (2002). This is likely due to the fact that the planning of response A is completed before response B is prepared. Still, the type of response in task B (i.e., action or nonaction) might influence responses in task A.

3.1.1 Method

Participants, stimuli and experimental setup

Seventy-two participants (mean age = 27.2, *SD* = 10.0; 18 male, 8 left-handed) were recruited for the experiment and divided into two groups, a short SOA group and a long SOA group. An a priori power analysis with a medium effect size ($d_z = 0.5$) and a power of 0.8 suggested a sample size of 34 participants per group for the critical comparison of switching from a nonaction to an action with the same effect versus an action with a different effect. As a multiple of three was needed to counterbalance which two responses produced the same effect, the final sample size was increased to 36 participants per group. All participants gave informed consent prior to the experiment and received either course credit or monetary compensation for participation.

Participants responded with the index, middle and ring finger of the right hand on the keys *B*, *N*, and *M* of a standard German QWERTZ keyboard or with no keypress, in case of a nonaction. Stimuli were presented on a 20’’ flat screen in white on a black background. Stimuli for task A of the experimental phase were a circle, a heart, a cross, and a star, mapped onto the index finger, middle finger, ring finger, and nonaction, respectively, for half of the participants. The mapping was reversed for the other half of the participants (half of each SOA group). Stimuli for task B were the numbers 1 to 4, mapped onto the index finger, middle finger, ring finger and nonaction, respectively. This mapping remained constant for all participants in order to help participants remember the mapping and reduce the number of errors.

Participant wore headphones to listen to the sound effects. The two effect tones were clearly distinct and had a duration of 150 ms including a 50 ms onset delay. One tone was a composed sound of four dual tone multi-frequency (DTMF) tones and one tone was a sinusoidal tone starting with a frequency of 600 Hz and increasing to a frequency of 2000 Hz. The mapping of the two effect tones to the four responses was counterbalanced across participants.

The experiment consisted of a free choice acquisition phase (two blocks à 40 trials), followed by the main experimental phase (12 block à 48 trials, 3 training and 9 test blocks). At the beginning of the experiment, participants received instructions about the two phases and completed a practice phase with six acquisition trials and 24 experimental trials. The practice phase could be repeated if necessary.

Acquisition phase

In acquisition trials, participants were instructed to choose in each trial between one of the four response alternatives, a keypress with the index finger, a keypress with the middle finger, a keypress with the ring finger or no keypress. Each response would be followed by a tone and two responses each would trigger the same effect tone (counterbalanced across participants).

Every trial began with a fixation cross presented for 500 ms. Then, a white framed rectangle was displayed, signaling participants to choose one response.

If participants chose one of the keypresses, a corresponding effect tone was played. Participant's RT history was used to determine when to present the nonaction effect tone. To that end, participant's RTs of keypresses were saved and the interval between the onset of the white rectangle and the nonaction tone was calculated as: (mean RT + mean RT + last RT)/3 + 250 ms. The nonaction tone was presented when no keypress was detected in that interval. For the first trial (i.e., when no RT history was available) the interval was set to 1500 ms. After tone presentation, the display remained black for 1000 ms and then a new trial started. If participants pressed a key during fixation or during tone presentation, an error message was displayed. At the end of both blocks, participants received a summary about the choice frequencies for each response.

Experimental phase

In experimental trials, participants completed two tasks (A and B) in succession (see **Figure 7**). Each trial started with a fixation cross displayed for 1000 ms. Then, the stimulus for task A was presented for 500 ms. Participants' task was to select the correct response for task A and prepare that response as well as possible, but not to execute the response. In the short SOA group, the stimulus for task B was then presented immediately (i.e., 500 ms after stimulus A onset). In the long SOA group, the display remained blank for 500 ms and then the stimulus for task B was presented (i.e., 1000 ms after stimulus A onset). Participants were instructed to respond as fast as possible to the task B stimulus with the correct response. If they responded correctly, the corresponding effect tone was played. After participants heard the confirmative tone, they were requested to execute the response for task A as fast as possible, which – if correct – was also followed by the corresponding effect tone. After presentation of the second effect tone, the display remained blank for 500 ms and then a new trial started. The responses required in task A and task B were always different from each other and each combination of first and second response was equally probable.

Initially, the response deadline for task A and task B was set to 1000 ms. That is, participants had 1000 ms to perform the correct response (action or nonaction) after stimulus onset. If the correct response occurred in this interval (i.e., the required keypress or no key press), the corresponding effect tone was played at the end of the interval. In the course of each block, participants' RT history was used to adjust this interval for task A and task B, respectively. As in the acquisition phase, the intervals for task A and B were computed as (mean RT + mean RT + last RT)/3 + 250 ms. However, very fast responses (below 250 ms for task B and 150 ms for task A) were not considered in the equation to prevent the interval from getting to short.

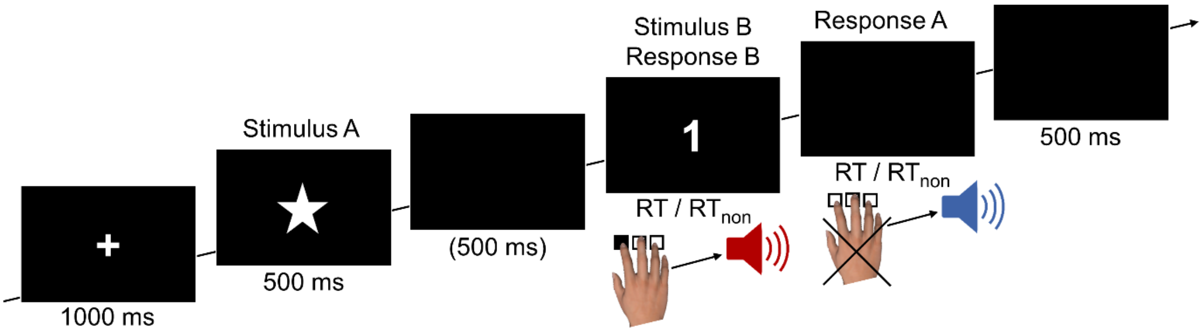

Figure 7. Exemplary trial for the experimental phase of Experiment 4. After a fixation cross, participants saw stimulus A and prepared the corresponding response without executing it. In the short stimulus onset asynchrony (SOA) group, stimulus A was immediately followed by stimulus B; in the long SOA group, the display remained blank for 500 ms before stimulus B was presented. Participants reacted immediately to stimulus B with a keypress (reaction time; RT) or a nonaction (RT$_{non}$) and the corresponding effect tone was presented. As soon as they heard the tone, participants executed the prepared response A, which was followed by the corresponding effect tone. Both effect tones could either be the same or different.

If participants responded incorrectly to task A or task B, an error message was displayed instead of the respective effect tones and the trial was continued after 300 ms. If participants responded too early (during fixation or presentation of the task A stimulus) and if they used a wrong key (none of the designated response keys), a corresponding error message was displayed for 1000 ms and the trial was aborted.

3.1.2 Results

Two participants reported having major difficulties with the task of the experimental phase. Inspection of the data showed that they had committed many errors in the experimental phase and correct responses were only given in about half of the trials (≤ 55%). These two participants were excluded from all analyses and the data was replaced.

Acquisition phase

In the free choice acquisition phase, the mean choice frequencies of all four responses were analyzed. To that end, all trials with errors were excluded, the frequency of keypresses with the index, middle and ring finger, and nonactions was calculated for each participant and a repeated-measures ANOVA with the factor response (index finger vs. middle finger vs. ring finger vs. nonaction) was calculated. For violations of the sphericity assumption, I report Greenhouse-Geisser corrected *p*-values along with the corresponding ε estimate for correcting degrees of freedom. Since different SOAs for the two groups of participants were only used in the experimental trials, the ANOVA was calculated for the two groups combined. The mean frequency differed between responses, $F(3,213) = 3.73$, $p = .050$, $\eta_p^2 = .05$ ($\varepsilon = 0.39$), with on average 26.3% keypresses with the index finger, 26.1% with the middle finger, 25.0%

with the ring finger and 22.6% nonactions. Four participants did not use all responses in the free choice acquisition phase. The first three blocks of the experimental phase served as a further training phase and were excluded from all statistical analyses.

Experimental phase – task B

The results of task B are illustrated in Figure 8. Detailed descriptive data is listed in Table 2. RTs and error rates were analyzed with a 2 x 2 x 2 mixed ANOVA with the within-subject factors response type (action vs. nonaction prepared for task A) and effect correspondence (same vs. different effect in task A and B) and the between-subject factor SOA (short vs. long). Significant interactions were further analyzed with two-tailed, paired *t*-tests. Corresponding effect sizes were calculated as $d_z = \frac{t}{\sqrt{n}}$.

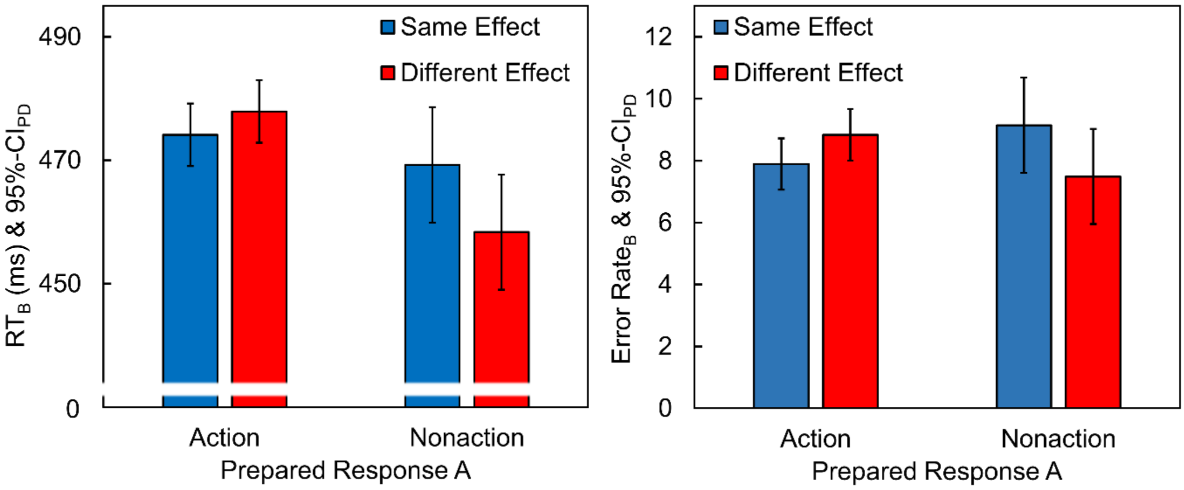

Figure 8. Mean reaction times (RT_B) and error rates for task B in Experiment 4. Error bars indicate the 95%-confidence interval of paired differences (CI_{PD}) for the comparison of same effects in task A and B versus different effects in task A and B, separately for prepared actions in task A and prepared nonactions in task A (cf. Pfister & Janczyk, 2013).

Table 2. Mean reaction times (RT; in ms) and error rates (in %) of task B with the respective standard errors of the mean (*SE*) in Experiment 4 and 5 as function of the prepared response in task A.

Response B	**Action prepared**		**Nonaction prepared**	
	Same	Different	Same	Different
Experiment 4				
Short SOA				
RT	478	481	475	465
(*SE*)	(10.5)	(10.1)	(9.7)	(9.2)
Error rate	8.6	9.9	8.7	7.7
(*SE*)	(0.8)	(1.0)	(1.2)	(0.9)
Long SOA				
RT	470	475	464	452
(*SE*)	(9.4)	(9.4)	(10.6)	(9.3)
Error rate	7.1	7.7	9.6	7.1
(*SE*)	(0.9)	(1.0)	(1.3)	(1.0)
Experiment 5				
RT	465	464	458	444
(*SE*)	(5.5)	(5.5)	(7.8)	(4.8)
Error rate	11.1	11.9	13.8	15.4
(*SE*)	(0.9)	(0.8)	(1.5)	(1.5)

SOA = stimulus onset asynchrony

RTs. For the RT analysis of task B, all trials with errors in task A or task B were excluded (short SOA: 16.9%; long SOA: 13.2%), as well as all trials following these erroneous trials and all trials with RTs deviating more than 2.5 standard deviations from the cell mean (short SOA: 2.1%; long SOA: 1.6%). Trials with nonactions in task B could not be analyzed, as no RT measure was available for those trials. A main effect of response type indicated faster responses, when a nonaction rather than an action had been prepared in task A, $F(1,70) = 11.20$, $p = .001$, $\eta_p^2 = .14$. The main effect of effect correspondence approached significance, $F(1,70) = 3.98$, $p = .050$, $\eta_p^2 = .05$. The ANOVA further revealed an interaction of effect correspondence and response type, $F(1,70) = 4.81$, $p = .032$, $\eta_p^2 = .06$. All other effects were not significant, all *F*s < 1. As the SOA had no influence on RTs, the entire set of participants was analyzed to further examine the interaction between effect correspondence and response type. The *t*-tests showed that when a nonaction had been prepared for task A, participants reacted slower when an action with the same effect tone was required in task B compared to when an action with a different effect tone was required (M_{same} = 469 ms, $M_{different}$ = 458 ms), $t(71) = 2.32$, $p = .023$, $d_z = 0.27$.However, when an action had been prepared for task A, there was no difference between switching to an action with the same effect and an action with a different effect and numerically the pattern was reversed (M_{same} = 474 ms, $M_{different}$ = 478 ms), $t(71) = 1.49$, $p = .141$, $d_z = 0.18$.

As an exploratory analysis, I analyzed whether the difference in RTs between switching to a response with the same effect and switching to a response with a different effect when the prepared response was an action correlated with the same difference in RTs when the prepared response was a nonaction. To that end, for each participant the mean RT of switching to a response with the same effect was subtracted from the mean RT of switching to a response with a different effect for trials where an action had been prepared in task A and trials where a nonaction had been prepared in task A, respectively. The test showed a significant, negative correlation, $r = –.665$, $t(70) = 7.45$, $p < .001$.

Error rates. For the analysis of the error rates in task B, only errors of commission were analyzed, anticipations and wrong keypresses (none of the designated response keys) were not included (note that potential omissions were also counted as errors of commission, because trials with no keypress were counted as nonactions). The ANOVA revealed an interaction of response type and effect correspondence, $F(1,70) = 6.77$, $p = .011$, $\eta_p^2 = .09$. Furthermore, a marginal significant interaction of SOA and response type, $F(1,70) = 3.04$, $p = .086$, $\eta_p^2 = .04$, hinted at more task B errors when an action was prepared for task A compared to a nonaction in the short SOA group, but a reversed pattern in the long SOA group. There was no interaction of SOA and effect correspondence, $F(1,70) = 1.53$, $p = .221$, $\eta_p^2 = .02$, and all other effects were also not significant, *F*s < 1. To further analyze the significant interaction of response type and effect correspondence in the whole set of participants, *t*-tests were computed. When an action had been prepared for task A, participants committed more errors when switching to an action with a different effect tone ($M_{\text{different}} = 8.8\%$) compared to an action with the same effect tone ($M_{\text{same}} = 7.8\%$), $t(71) = 2.09$, $p = .040$, $d_z = 0.25$. However, when a nonaction had been prepared, the pattern was reversed ($M_{\text{same}} = 9.1\%$; $M_{\text{different}} = 7.4\%$), $t(71) = 2.09$, $p = .040$, $d_z = 0.25$.

As for the RTs, I analyzed whether the influence of effect correspondence was correlated between a prepared action and a prepared nonaction. The correlation was not significant, $r = -.215$, $t(70) = 1.83$, $p = .070$.

Experimental phase – task A

Detailed descriptive data is listed in **Table 3**. RTs and error rates were analyzed with a 2 x 2 x 2 mixed ANOVA with the within-subject factors response type (action vs. nonaction executed in task B) and effect correspondence (same vs. different effect in task A and B) and the between-subject factor SOA (short vs. long).

Table 3. Mean reaction times (RT; in ms) and error rates (in %) of task A with the respective standard errors of the mean (*SE*) in Experiment 4 and 5 as function of the executed response in task B.

Response A	**Action executed**		**Nonaction executed**	
	Same	Different	Same	Different
Experiment 4				
Short SOA				
RT	274	267	350	354
(*SE*)	(17.3)	(17.3)	(15.2)	(15.7)
Error rate	6.8	6.7	15.6	13.7
(*SE*)	(0.7)	(0.6)	(1.7)	(1.3)
Long SOA				
RT	266	262	331	328
(*SE*)	(12.8)	(12.9)	(12.0)	(11.1)
Error rate	4.5	4.4	9.0	10.1
(*SE*)	(0.6)	(0.4)	(1.6)	(1.4)
Experiment 5				
RT	191	192	246	249
(*SE*)	(10.7)	(10.9)	(10.0)	(9.6)
Error rate	4.2	5.1	11.8	11.6
(*SE*)	(0.5)	(0.5)	(1.4)	(1.3)

SOA = stimulus onset asynchrony

RTs. For the RT analysis of task A, all trials with errors and all trials following these erroneous trials were again excluded. Furthermore, all trials with RTs deviating more than 2.5 standard deviations from the cell mean were excluded (short SOA: 1.8%; long SOA: 0.7%). Trials with nonactions in task A could not be analyzed, as no RT measure was available for those trials. Participants reacted slower after having performed a nonaction compared to an action, as indicated by a main effect of response type, $F(1,70) = 68.72$, $p < .001$, $\eta_p^2 = .50$. All other effects were not significant, all *F*s < 1.51, *p*s > .223.

Error rates. For the analysis of the error rates in task A, only errors of commission were analyzed, anticipations and wrong keypresses (none of the designated response keys) were not included (potential “omissions” were also counted as errors of commission). Additionally, all trials with errors in task B were excluded. The percentage of errors was higher in the short SOA group compared to the long SOA group, $F(1,70) = 10.06$, $p = .002$, $\eta_p^2 = .13$. Furthermore, participants committed overall more errors after having performed a nonaction for task B compared to an action, $F(1,70) = 67.91$, $p < .001$, $\eta_p^2 = .49$. A marginal significant interaction of response type and SOA, $F(1,70) = 3.12$, $p = .082$, $\eta_p^2 = .04$, suggested that this pattern might be more pronounced in the short SOA group compared to the long SOA group. All other effects were not significant, *F*s < 2.06, *p*s > .156.

3.1.3 Discussion

The purpose of the present experiment was to investigate whether the preparation of a nonaction would automatically activate the corresponding nonaction effect, as suggested from an effect-based representation of nonactions. The results showed that participants were faster (and less error prone) when switching from a prepared nonaction to an action with a different sound effect compared to an action with the same sound effect. Because the sound effects were only presented after the respective response had been executed, it can be assumed that an anticipation of the nonaction effect was effective.

While it was easier for participants to switch to an action with a different rather than the same effect when a nonaction had been prepared, this pattern was reversed when an action had been prepared. This finding is in line with previous results (Kunde et al., 2002; see also Janczyk & Kunde, 2014), even though in the present experiment a significant difference was only present in the error rates, but not in RTs. This inverted pattern for actions and nonactions was also evident in an explorative analysis, which showed a strong negative correlation between effect correspondence for actions and nonactions. This finding might suggest that the same mechanisms are involved in the preparation of action and nonactions with opposing consequences and will be discussed in more detail in the general discussion of this chapter.

In contrast to the findings of Kunde et al. (2002), the SOA did not modulate the influence of effect correspondence between task A and task B. However, these results are limited to the small range of SOAs that were used in the present experiment and the between-subjects manipulation of SOAs. Even though SOA had no strong influence on task B execution, it clearly influenced the error rate of task A and participants committed more errors when the SOA between the stimuli for task A and task B was shorter. This could suggest that, with a short SOA, participants did not have enough time to fully prepare response A. The SOA should therefore be selected carefully to allow for a sufficient time of response preparation in task A.

Taken together, the results of Experiment 4 are in line with the assumption of an effect-based representation of nonactions. However, because of the novelty of the results and the partly unpredicted results (e.g., the direction of the influence of effect correspondence for nonactions or the results of task A), I sought to replicate the findings of Experiment 4 before drawing further conclusions.

3.2 Experiment 5

Experiment 5 was conducted to investigate the anticipation of nonaction and action effects and to corroborate evidence for an effect-based representation of nonaction effects. Thus, Experiment 5 was a close replication of Experiment 4 with some adjustments of the experimental procedure. These adjustments aimed to increase the influence of effect correspondence, which showed only a small effect in Experiment 4 ($d_z = 0.3$ for nonactions). For one, two new sound effects were used, sounds of a barking dog and a ringing bell, to make the response effects more distinct than the effects of Experiment 4. For another, effects followed keypresses after a constant delay of 100 ms rather than being presented with a variable, RT-adjusted delay at the end of the response deadline. This was done because previous research suggests that action initiation can be influenced by the delay between action and effects (Dignath et al., 2014; Dignath & Janczyk, 2017). At last, the three keypress actions were made more distinct. To that end, participants did not use three fingers of one hand for the three actions as in Experiment 4, but rather the index fingers from the left and right hand, as well as the right thumb. I expected that participants would be faster to initiate a response for task B when they had prepared a nonaction with a different rather than the same effect, but slower when they had prepared an action with a different rather than the same effect. For task A, I expected that responses would be influenced by the type of response in task B, i.e., faster responses and fewer errors when an action rather than a nonaction had been executed in task B.

3.2.1 Method

Participants

The results of Experiment 4 suggested a small effect size for the critical comparison of switching from a nonaction to an action with the same versus a different effect. Experiment 5 aimed to increase this effect size. The a priori power analysis was therefore based on a small to medium effect size ($d_z = 0.4$) and a power of 0.8 and indicated that a sample size of at least 52 participants was necessary. To account for potential drop-out because of a high error rate as in Experiment 4, the initial sample size was set to 60 participants. Data of one additional participant was collected to counterbalance which two responses produced the same effect after drop-out. The final sample-size therefore amounted to 61 participants (mean age = 25.3, *SD* = 5.9, 15 male, 4 left-handed). All participants gave informed consent prior to the experiment and received either course credit or monetary compensation for participation.

Stimuli, experimental setup, and procedure

The experimental stimuli were identical to those of Experiment 4, only the two effect tones were replaced by the sound of a dog barking and the sound of a table bell (duration 220 ms each). To respond, participants used the keys *R*, *I*, and the spacebar of a standard German QWERTZ keyboard with the index fingers of the left and right hand and the thumb of the right hand, respectively (and no keypress, in case of a nonaction). The experimental procedure in the acquisition phase was identical to Experiment 4. In the experimental phase, the timing was derived from the short SOA group of Experiment 4. However, effect tones were always presented 100 ms after a keypress in case of actions and only for nonactions they were presented at the end of the calculated response interval.

3.2.2 Results

As in Experiment 4, participants were excluded when the number of correct trials (no error in task A and task B) was low (≤ 55%). This applied to four participants.

Acquisition phase

To analyze the mean choice frequencies of all four responses, all trials with errors were excluded, the frequency of keypresses with the left index finger, right index finger, right thumb, and nonactions was calculated for each participant and a repeated-measures ANOVA with the within-subject factor response (left index finger vs. right index finger vs. thumb vs. nonaction) was calculated. The mean frequency differed between responses, $F(3,168) = 10.25$, $p = .001$, $\eta_p^2 = .16$ ($\varepsilon = 0.46$), with on average 26.4% keypresses with the left index finger, 26.3% with the right index finger, 25.0% with the thumb, and 22.3% nonactions. One participant did not use all responses in the free choice acquisition phase. The first three blocks of the experimental phase served as a further training phase and were excluded from all statistical analyses.

Experimental phase – task B

The results are illustrated in **Figure 9**. Descriptive statistics are listed in **Table 2**. RTs and error rates were analyzed with a 2 x 2 repeated-measures ANOVA with the factors response type (action vs. nonaction prepared for task A) and effect correspondence (same vs. different effect in task A and B).

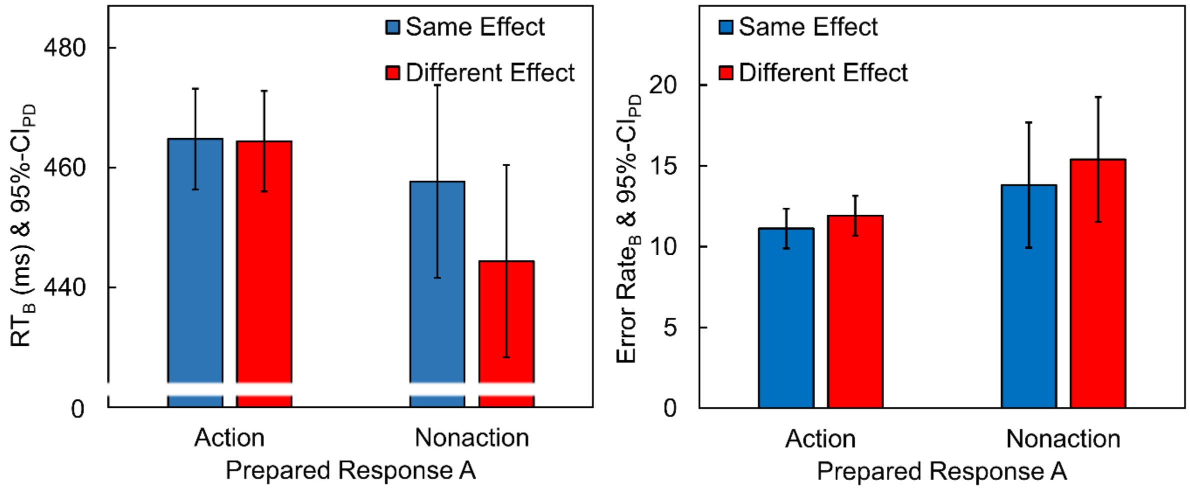

Figure 9. Mean reaction times (RT_B) and error rates for task B in Experiment 5. Error bars indicate the 95%-confidence interval of paired differences (CI_{PD}) for the comparison of same effects in task A and B versus different effects in task A and B, separately for prepared actions and prepared nonactions in task A (cf. Pfister & Janczyk, 2013).

RTs. For the RT analysis of task B, all trials with errors were excluded (18.3%), as well as all trials following these erroneous trials and all trials with RTs deviating more than 2.5 standard deviations from the cell mean (1.1%). Trials with nonactions in task B were not analyzed, as no RT measure was available for those trials. A main effect of response type indicated faster responses, when a nonaction rather than an action had been prepared in task A, $F(1,56) = 14.46$, $p < .001$, $\eta_p^2 = .21$. The ANOVA further revealed a main effect of effect correspondence, $F(1,56) = 5.98$, $p = .018$, $\eta_p^2 = .10$. The interaction of response type and effect correspondence was not significant, $F(1,56) = 1.27$, $p = .265$, $\eta_p^2 = .02$. The descriptive data indicated that the influence of effect correspondence was mainly existent when a nonaction had been prepared in task A (M_{same} = 458 ms, $M_{different}$ = 444 ms), but not when an action had been prepared in task A (M_{same} = 465 ms, $M_{different}$ = 464 ms), although the pairwise comparison with a two-tailed, paired *t*-tests was not significant in either case, $t(56) = 1.66$, $p = .102$, d_z = 0.22 and $t(56) = 0.09$, $p = .928$, d_z = 0.01, respectively.

As in Experiment 4, I further analyzed whether the influence of effect correspondence was correlated between a prepared action and a prepared nonaction. The test showed a strong, negative correlation, $r = -.750$, $t(55) = 8.40$, $p < .001$.

Error rates. For the analysis of the error rates in task B, only errors of commission were analyzed, anticipations and wrong keypresses (none of the designated response keys) were not included (note that potential omissions were also counted as errors of commission). The ANOVA revealed a main effect of response type, $F(1,56) = 8.40$, $p = .005$, $\eta_p^2 = .13$, as participants committed more errors when a nonaction had been prepared for task A compared to an action. There was no main effect of effect correspondence, $F(1,56) = 1.83$, $p = .184$, $\eta_p^2 = .03$, as well as no interaction between these factors, $F < 1$. As for the RTs, the influence of effect correspondence between a prepared action and a prepared nonaction was correlated, $r = -.440$, $t(55) = 3.64$, $p = .001$.

Experimental phase – task A

Detailed descriptive data is listed in **Table 3**. RTs and error rates were analyzed with a 2 x 2 repeated-measures ANOVA with the factors response type (action vs. nonaction executed in task B) and effect correspondence (same vs. different effect in task A and B) was calculated.

RTs. For the RT analysis of task A, all trials with errors and all trials following these erroneous trials were again excluded. Furthermore, all trials with RTs deviating more than 2.5 standard deviations from the cell mean were excluded (1.6%). Trials with nonactions in task A were not analyzed, as no RT measure was available for those trials. Participants reacted slower after having performed a nonaction compared to an action, as indicated by a main effect of response type, $F(1,56) = 64.77$, $p < .001$, $\eta_p^2 = .54$. Neither the main effect of effect correspondence nor the interaction of response type and effect correspondence were significant, $F(1,56) = 1.06$, $p = .308$, $\eta_p^2 = .02$ and $F < 1$, respectively.

Error rates. For the analysis of the error rates in task A, only errors of commission were analyzed, anticipations and wrong keypresses (none of the designated response keys) were not included (potential omissions were also counted as errors of commission). Participants committed overall more errors after having performed a nonaction for task B compared to an action, $F(1,56) = 50.65$, $p < .001$, $\eta_p^2 = .48$. All other effects were not significant, *F*s < 1.

3.2.3 Discussion

The purpose of Experiment 5 was to investigate the anticipation of nonaction and action effects and replicate the key findings of Experiment 4. The results showed that participants were faster when switching from a prepared response to an action with a different sound effect compared to an action with the same sound effect. In contrast to Experiment 4, this influence of effect correspondence was not modulated by the type of the prepared response (i.e., whether the prepared response was an action or a nonaction). The descriptive data, however, suggested that the influence of effect correspondence was mainly present when a nonaction had been prepared. The results thus seem to support the key finding of Experiment 4 for nonactions. The value of this finding, as well as the remaining findings of Experiment 4 and 5 will be jointly considered in the general discussion of this chapter.

Experiment 5 aimed to increase the influence of effect correspondence in comparison to Experiment 4. Even though the experimental procedure was adjusted, the effects in Experiment 5 were not boosted. The changes that were made to the experimental procedure thus did not work in the expected direction or cancelled each other out. In future research, different approaches to boost effects could be selected. For instance, previous research suggests that increasing the task relevance of response effects can increase their influence (e.g., Ansorge, 2002; Janczyk, Yamaguchi, Proctor, & Pfister, 2015; Müller, 2016; Wirth et al., 2016).

3.3 General discussion and interim conclusion

The two experiments of this second empirical part showed that execution of an action is faster when a nonaction with a different effect had been prepared beforehand rather than a nonaction with the same effect. Because the respective effects were only presented after action execution, it can be assumed that an anticipation of the nonaction effects was effective and influenced action initiation. The results of the present experiments thus corroborate evidence for an effect-based representation of nonactions.

Correlation analyses further showed a strong negative relationship between the influence of effect correspondence for prepared actions and the influence of effect correspondence for prepared nonactions. This could suggest that the same mechanisms underlie action and nonaction preparation, which, however, have opposite consequences if the prepared response has to be interrupted and a different action has to be executed. A stronger effect anticipation during actions might give a greater head start to actions with the same effect (cf. Kunde et al., 2002). In contrast, a stronger effect anticipation during nonactions might specifically suppress actions with the same effect.

While the present experiments showed an influence of effect correspondence when nonactions were prepared, this influence was reduced for actions in Experiment 4 and appeared to be absent in Experiment 5. There are different reasons to explain this deviation in the present experiments, which is also in contrast to previous results of action effect anticipation (Janczyk & Kunde, 2014; Kunde et al., 2002). For one, participants could have selectively ignored the sound effects of actions, but not those of nonactions. Because of the experimental design, the sound effects may have been particularly relevant for nonactions, because participants could only monitor the success of a nonaction by waiting for the nonaction tone. This may have led participants to represent the nonactions in terms of the distal sound effects. In contrast, participants may have relied more heavily on proprioceptive feedback for the monitoring of actions and may have represented actions less strongly (or less often) in terms of the distal sound effect (cf. Kunde et al., 2002).

Another way to explain the reduced influence of effect correspondence on actions compared to nonactions is to assume that code occupation mechanisms are involved. The code occupation hypothesis suggests that planning an action leads to an activation and temporary binding of the features of that action in an event file (e.g., spatial features of an action; Hommel, 1998). Importantly, this binding leads to an occupation of the features, making them unavailable for other activities (Stoet & Hommel, 1999). Evidence for this claim has been gathered in experimental setups that closely resemble the present experimental setup with a task A and a task B. Participants also had to prepare a response A to a stimulus A but withhold the execution of that response. Then, a stimulus B was presented, and participants had to immediately respond to that stimulus with a response B. Only after response B, they were allowed to perform the prepared response A. Responses were not followed by external sensory effects that could overlap (like the sound effects in the present experiments), but response A and B could overlap in terms of their spatial features (“left” or “right”). The results showed that when an action on one side was planned for task A (e.g., lifting the left index finger), the execution of another action on the same side (e.g., lifting the left foot) was hindered relative to the execution of an action on the other side (e.g., lifting the right foot). These results suggests that the action features were bound into an event file and were therefore temporarily unavailable (e.g., Stoet & Hommel, 1999; Wiediger & Fournier, 2008). Importantly, it is assumed that the features that can become bound into such an event file are manifold and can expand to action effects, as proposed by ideomotor accounts (Hommel et al., 2001; Hommel, 2009). This assumption is supported by several studies accumulating evidence for short-term binding between actions and effects (Dutzi & Hommel, 2009; Herwig & Waszak, 2012; Janczyk, Heinemann, & Pfister, 2012; Moeller, Pfister, Kunde, & Frings, 2016).

If action effects are bound into an event file just like other action features during action planning, they should also be temporarily unavailable for other processes. Applied to the present experimental setup this would mean that it should be harder to execute an action B when an action A with the same effect has been prepared rather than an action A with a different effect. This possibility has been addressed by Kunde et al. (2002, Exp. 2 and 3), but the data consistently showed the reversed pattern, i.e., a benefit from effect correspondence rather than costs due to partial feature overlap (i.e., effect correspondence) in task A and task B. The authors therefore suggested that preparing an action might have benefits as well as costs for other concurrent actions depending on the time it takes to activate a certain action effect compared to the time it takes to unbind an existing event file, which might actually differ from one action to another. In cases where recollecting an action effect is difficult but the resulting event file can be easily unbound, there should be effect correspondence benefits. On the other hand, when recollecting an action effect is easy but it is hard to unbind the resulting event file, there should be costs of effect correspondence. In the present setup, the difference between benefits and costs may have been shifted away from benefits of effect activation to costs of feature occupation relative to the experiments of Kunde et al. (2002; e.g., because different sounds and actions were used). Previous research has also been able to show that the costs from overlapping features can be reduced or reversed depending on the automaticity of actions and the respective overlapping features (Fournier, Gallimore, Feiszli, & Logan, 2014; Fournier, Wiediger, & Taddese, 2015; Wiediger & Fournier, 2008). However, further research needs to investigate the interplay of costs and benefits when effects of multiple actions overlap.

The feature occupation hypothesis can conveniently explain the present results for prepared nonactions without relying on further assumptions that are specific for nonactions. That is, it may have been more difficult for participants to switch from a prepared nonaction to an action with the same effect rather than a different effect, because the effect had already been bound to the nonaction event file and unbinding the effect took time. This provides a parsimonious explanation for the present results. Furthermore, it still rests on the critical assumption that nonaction effects are anticipated when a nonaction is prepared and is thus in line with an effect-based representation of nonactions.

It is tempting to draw further conclusions about nonactions from the additional results of the two experiments, especially from task A. Responses in task A were not influenced by the correspondence of effects. This is in line with the results of Kunde et al. (2002) and may be due to the fact that the planning of response A had been completed before response B was prepared or initiated. However, response A execution was clearly influenced by the type of response in task B. That is, participants were faster and committed less errors when they had executed an action for task B compared to a nonaction. The RT result pattern can be explained by trivial circumstances. Faster responses after actions can be easily explained by assuming that participants executed keypresses in fast succession if two actions were required in task B and task A without waiting for the sound effects. However, if a nonaction was required in task B, participants reacted slower because they waited for the nonaction tone to correctly time their action for task A. The increased error rate following a nonaction, on the other hand, cannot be explained along these lines and suggests that performing a nonaction might hinder subsequent actions. This finding can be explained along two lines which do not have to be mutually exclusive. For one, actions and nonactions could be considered as two different tasks. Switching between these tasks should then come along with switching costs, as generally found in task-switching paradigms (Kiesel et al., 2010). For another, nonactions might involve a global suppression of all actions. This global suppression could interfere with the rapid execution of a subsequent action. Importantly, the finding that performing a nonaction hinders action execution points to a fundamental difference between nonactions and actions and suggests that while actions and nonactions can employ the same mechanisms (such as effect anticipation), they are clearly distinct instances.

In a nutshell, the results of the two experiments showed a robust, albeit small, influence of effect correspondence and thus indicate that nonaction effects are anticipated when a nonaction is prepared. This finding corroborates the results of the previous chapter and suggests that nonactions can be represented in terms of their sensory effects.

4. Agency for nonaction effects

The results of the previous chapters indicate that bidirectional associations between nonactions and effects can be acquired and that nonaction effects are anticipated when performing a nonaction. These findings suggest that nonactions share essential characteristics with actions and are in line with the assumption that nonactions can be represented in terms of their sensory effects. Nonaction effects should therefore also be perceived as being self-produced and elicit a sense of agency – just like action effects. The experiments of this third empirical part were designed to test this hypothesis.

So far, the sense of agency has been investigated predominantly and intensively for actions. The scope of this research ranges from freely selected, self-initiated actions, to forced actions and externally controlled actions, as well as to actions of other people (e.g., Barlas, Hockley, & Obhi, 2017; Borhani, Beck, & Haggard, 2017; Burin, Pyasik, Salatino, & Pia, 2017; Farrer, Valentin, & Hupé, 2013; Gentsch & Schütz-Bosbach, 2011; Haering & Kiesel, 2016; Haggard & Clark, 2003; Haggard, Clark, & Kalogeras, 2002; Poonian, McFadyen, Ogden, & Cunnington, 2015; Sato, 2008; Timm, SanMiguel, Keil, Schröger, & Schönwiesner, 2014; Wegner et al., 2004). However, research on a sense of agency for nonactions is still lacking. The question whether people feel a sense of agency for the effects of their nonactions is fundamental – not only to complete theoretical models but also from an ethical viewpoint. In most human societies, people are thought to be in control of their actions and can therefore be held liable for their actions and the consequences of these actions. In other words, it is assumed that people generally have a sense of agency for their actions and people are evaluated and judged on this basis (Haggard, 2017). Sometimes, however, people are judged by the fact that they did not act in a certain situation. This is especially true when the nonaction entails negative consequences. In an extreme case a few years ago in Essen, Germany, an 83-year old man collapsed in a bank (Burger, 2017, September 18). Several bank customers, who entered the bank shortly afterwards, did not act and ignored the unconscious man, who later died in the hospital. The costumers were later on criticized for their nonactions and even indicted for a failure to render assistance. In Germany, France and several other countries, people can be punished for not acting with a fine or a prison sentence when they fail to render assistance to a person in need (§323c StGB; article 223-6 du Code pénal). Determining to what degree people feel a sense of agency for consequences of their nonactions is therefore essential and might influence how we judge people in the absence of overt behavior.

Objectively, people should not feel as causal agent for the effects of a nonaction, as there is generally no unique causal link between the decision not to act and a specific effect. Rather, situational circumstances allow that refraining from actions results in a specific effect. In previous experimental settings on nonactions, for instance, the computer eventually produced the nonaction effect, if participants decided not to press a key (e.g., Kühn et al., 2009; Kühn & Brass, 2010a, and see also the experiments of this dissertation). However, a sense of agency might still be felt for nonaction effects. For one, the sense of agency does not necessarily parallel objective causal relations, as evident in studies on vicarious agency effects. In a study by Wegner et al. (2004), participants saw themselves in a mirror, while wearing a smock which hid their arms. Another person was standing behind them and reached forward with his or her arms, so that the arms appeared to be in position of the participant’s arms. An obvious causal link between the participants and the other person’s movements was absent, but still participants reported an enhanced feeling of control for the other person’s arms when they heard consistent instructions announcing the movement. This finding suggests that agency can expand to situations where a direct causal link is absent, but inferential processes still suggest a sense of agency. These inferential processes might also cause a sense of agency for nonactions.

Furthermore, the findings of the previous chapter suggest that nonaction effects are anticipated for nonaction control. Recent models of the sense of agency suggest that a comparison of anticipated and actual sensory effects is at least one component involved in the sense of agency (e.g., Synofzik et al., 2008; Waszak et al., 2012; even though there may still be a controversy about when and how this anticipation is created in the first place). The anticipation of nonaction effects could directly inform the sense of agency, so that predictive processes might also be involved in forming a sense of agency for nonaction effects.

4.1 Measuring the sense of agency

Different types of measures have been employed to study the sense of agency. These measures can be divided into implicit and explicit measures. Explicit measures of the sense of agency directly ask participants to report their sense of agency. For instance, participants might be asked whether they think it was them or another person who produced a certain event, or they might be asked to rate how much they felt that they had caused a certain effect. These explicit measures capture the sense of agency in a quite intuitive way, however, they may be influenced by additional, agency-unrelated aspects, such as demand effects (Moore, 2016).

To measure the sense of agency implicitly, one can make use of two phenomena, sensory attenuation and intentional binding. *Sensory attenuation* describes the finding that self-produced sensory effects are perceived as less intense compared to externally produced effects. This effect is well-established for tactile and auditory effects and can be found using intensity ratings (e.g., Blakemore et al., 2000; Desantis, Weiss, Schütz-Bosbach, & Waszak, 2012) and brain-imaging techniques, such as fMRI and electroencephalography (EEG; e.g., Bäss, Jacobsen, & Schröger, 2008; Blakemore et al., 1998; Schafer & Marcus, 1973; Weller et al., 2017). However, it seems less reliable in the visual domain, where some studies report attenuation effects (e.g., Cardoso-Leite, Mamassian, Schütz-Bosbach, & Waszak, 2010; Gentsch & Schütz-Bosbach, 2011), whereas others find no attenuation or even an enhancement for self-produced effects (e.g., Mifsud et al., 2016; Schwarz, Pfister, Kluge, Weller, & Kunde, 2018).

Intentional binding, on the other hand, describes the finding that the interval between an action and a resulting sensory effect is perceived as compressed in time: action and effects are temporally shifted towards each other (Engbert, Wohlschläger, Thomas, & Haggard, 2007; Haggard, Aschersleben, Gehrke, & Prinz, 2002; Haggard, Clark et al., 2002; Humphreys & Buehner, 2009; Nolden, Haering, & Kiesel, 2012). This compression effect is evident when an intentional action causes a specific effect. However, no compression effect could be found for passive movements (Engbert, Wohlschläger, & Haggard, 2008; Nolden et al., 2012) or involuntary movements triggered by transcranial magnetic stimulation (TMS; Haggard, Clark et al., 2002; Haggard & Clark, 2003). Consequently, it was assumed that intentional binding can be used to estimate the sense of agency in a given situation (Moore, 2016; Moore & Obhi, 2012). Intentional binding has often been investigated using a clock procedure, in which participants observe a rotating clock hand while executing an action which is followed by a sound effect (Haggard, Clark et al., 2002). Participants are asked to report the position of the clock hand at the time when they heard the sound. This estimation is compared to a baseline condition in which participants hear sounds without a preceding action. Usually, participants judge the sound to occur earlier when it is preceded by an action compared to the baseline condition (also referred to as outcome binding). Similarly, participants judge the occurrence of their action later in time when the action is followed by a sound compared to a baseline condition, in which the action is not followed by any effect (also referred to as action binding). Alternatively, intentional binding can be assessed by using direct interval estimations (Engbert et al., 2007; Engbert et al., 2008; Humphreys & Buehner, 2009). To that end, participants are asked to estimate the time interval between a keypress and a following effect. Generally, participants judge the interval between a self-produced keypress and a following effect to be shorter compared to a physically identical interval without participants' involvement.

Implicit and explicit measures of the sense of agency are not necessarily strongly intertwined and sometimes yield divergent results (e.g., Dewey & Knoblich, 2014; Ebert & Wegner, 2010; Saito, Takahata, Murai, & Takahashi, 2015; Weller et al., 2017). This can be explained along different lines. For one, it is likely that they tap into different aspects of the sense of agency and are differentially influenced by predictive and inferential mechanisms (Synofzik et al., 2008; Weller et al., 2017). Furthermore, explicit measures are probably influenced more strongly by additional processes that are unrelated to the sense of agency, such as demand effects (Moore, 2016).

Since implicit and explicit agency measures do not necessarily yield the same results, the sense of agency for nonaction effects should be investigated using implicit and explicit agency measures alike. In Experiment 6, I used explicit agency ratings to test whether participants would report a sense of agency for nonactions effects. Furthermore, this experiment examined the sense of agency for commanded nonactions as well as freely chosen nonactions (i.e., it was either predetermined or not whether an action or a nonaction was to be performed in a given trial). In the following experiments, only freely chosen nonactions were used, as these elicited higher agency ratings. In Experiment 7, I tested whether a sense of agency for nonactions would also be found for an implicit measure, by applying the clock procedure of intentional binding. To strengthen the results of this experiment, Experiment 8 also assessed whether nonactions would elicit intentional binding by using the interval estimation procedure.

4.2 Experiment 6

This experiment set out to test whether participants would report a sense of agency for nonaction effects. To that end, participants saw a simplified pinball machine on the computer screen with a left and a right arm. The ball rested in the middle between the two arms and could be shot either into the left or the right arm by two opposing springs. Participants' task was to shoot the ball into the left or right arm. One of the springs would always be pre-activated. If participants opted for a nonaction (i.e., decided not to press a key), the ball would be shot into the pre-activated direction. If participants performed an action (i.e., a keypress), the activation of the springs would be reversed and the ball would be shot into the other direction. In different trials, participants either had to perform an action, had to perform a nonaction, or were free to choose between action and nonaction. Agency ratings for the ball movement were assessed at the end of a trial. In some trials, participants had no chance to perform an action or a nonaction and the ball was shot into either arm without participants' involvement. These trials served as a baseline condition.

I expected enhanced agency ratings for all conditions relative to the baseline condition. Furthermore, agency ratings should be higher for free choice actions compared to forced choice actions, in line with previous studies (e.g., Sebanz & Lackner, 2007; Wenke, Fleming, & Haggard, 2010). In the same line, agency ratings for free choice nonactions should be higher compared to forced choice nonactions. Agency ratings might further differ between forced actions and forced nonactions. A forced action still involves a motor response and thus participants need to be determined to eventually execute the commanded action. On the other hand, for a forced nonaction no such intentions need to be formed (Kühn et al., 2009). Thus, agency ratings might be strongly reduced or even absent for forced nonactions. In addition to the agency ratings for the ball movement (i.e., the (non)action effects), I asked participants how strongly they felt responsible for the action or nonaction itself. This question predominantly targeted the differences between free and forced choice responses. Feelings of responsibility should generally be higher for freely chosen responses compared to commanded responses, as participants have to select the response they want to give and this decision is not taken from them.

4.2.1 Method

Participants

Thirty-four participants were recruited for the experiment (mean age: 21.0, *SD* = 3.9; 2 male; 4 left-handed). The sample size was based on an a priori power analysis with a medium effect size ($d_z = 0.5$) and a power of 0.8. All participants gave informed consent prior to the experiment and received course credit for participation. One participant was excluded from all analyses, because he or she misunderstood the instructions and answered all questions with regard to the whole experiment instead of only the current trial.

Stimuli and experimental setup

Participants sat in front of a 22’’ flat screen and used the key *C* of a standard German QWERTZ keyboard to give responses. Stimuli were presented on a black background. The pinball-like machine was V-shaped and consisted of two arms (see **Figure 10**). A blue ball rested in the center between the two arms and two springs could shoot the ball either into the left or the right arm. At the end of each arm was a hole for the ball. If the ball was shot, it moved along one of the arms until it reached the hole and vanished into it. This movement took 375 ms from start to end. The pinball machine was displayed in the middle of the screen. Red arrows were presented directly above the arms to indicate the pre-activated direction in a trial. Imperative stimuli (colored rectangles in green, yellow, and red) and the agency questions were presented in the center of the screen above the pinball machine. The agency question was "*How strongly did you feel as causal agent for the ball movement to the left/right?*" (German original: *Wie sehr hast du dich als Verursacher der Ballbewegung nach links/rechts gefühlt?*). The responsibility question was „*How strongly did you feel responsible for your own (non)action?*" (German original: *Wie sehr hast du dich gerade verantwortlich für deine (Nicht)Handlung gefühlt?*). Participants could respond on a visual analog scale ranging from 0 ("*a little*") to 100 ("*a lot*") by moving the mouse to the left and right.

Experimental procedure

The pinball machine was continuously displayed on the screen throughout a trial and between trials. The time between two trials was 2000 ms. Each trial started with the ball in rest. In all trials, except for the baseline trials, an arrow indicating the pre-activated direction was then shown for 500 ms. If participants decided not to press a key, the ball would be shot in that direction. Then, a colored rectangle appeared. In case of a green rectangle, participants were requested to press the response key (*forced action*). In case of a red rectangle, participants were requested not to press the response key (*forced nonaction*). A yellow rectangle indicated that participants could freely decide whether to press the response key (*free action*) or not (*free nonaction*). If participants pressed the response key, the ball was shot in the opposite direction of the pre-activation 50 ms after the keypress. To determine, when participants decided not to press the response key, a nonaction interval was calculated using participants RT history, with (mean RT + mean RT + last RT)/3 + 300 ms. At the beginning of the experiment, when no RT history was available, the nonaction interval was set to 1500 ms. If no keypress was detected in this nonaction interval, a nonaction was registered and 50 ms later the ball was shot in the pre-activated direction. In *baseline* trials, no pre-activation arrows and imperative stimuli were presented. Instead, at the beginning of the trial the ball was immediately shot in either direction.

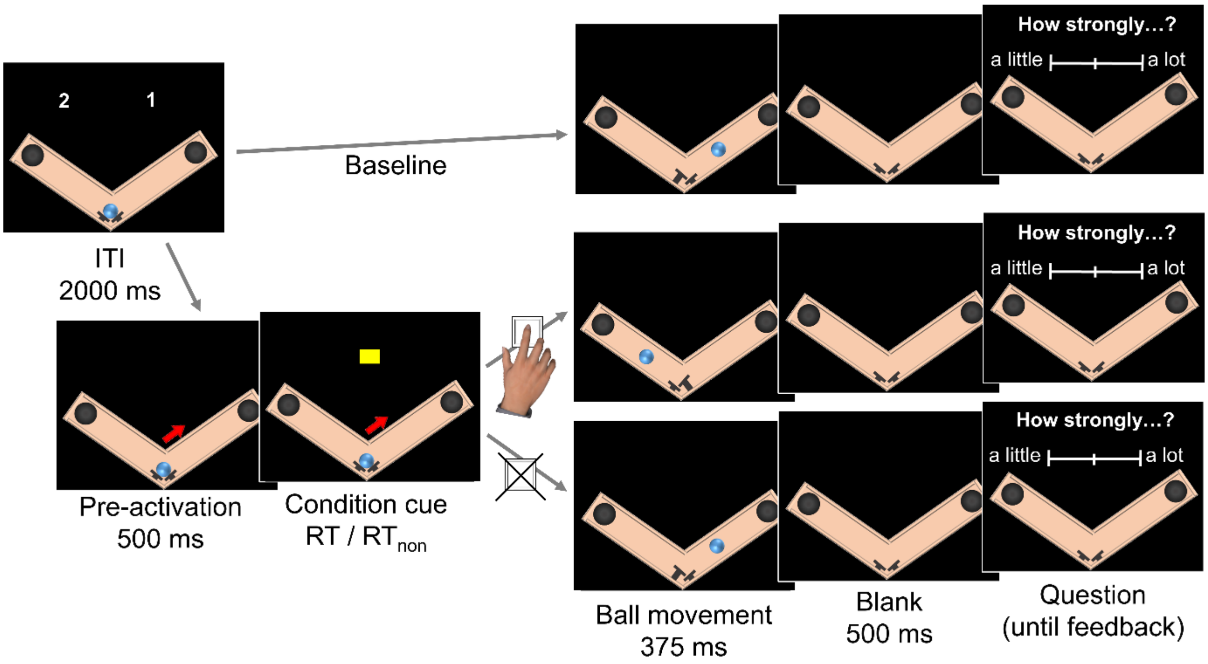

Figure 10. Trial structure in Experiment 6. Each trial started with the display of the pinball machine, the ball in rest and the number of shots to the left and right within one block displayed above the respective pinball arms (top left picture). In the ***baseline*** condition (top row), the ball was then shot into the left or right direction and participants had no opportunity to influence the ball direction. In all other conditions, an arrow was displayed after trial start, signifying the pre-activated direction. Then, a colored rectangle indicated participant's task in the current trial. A yellow rectangle indicated that participants could choose between pressing a key and not pressing a key (***free action*** or ***free nonaction***). A green rectangle signaled participants to press a key (***forced action***), a red rectangle signaled participants not to press a key (***forced nonaction***). If participants did not press a key, the ball was shot into the pre-activated direction. If participants pressed a key, the ball was shot into the opposite direction. In some trials, the agency or the responsibility question was presented to the participants after the ball movement. In other trials, the question was not displayed and the trial ended after the ball movement. Stimuli are not drawn to scale to increase legibility.

Participants were instructed to try to keep the number of ball movements to the left and right about equal within one block, whenever they had the chance to freely choose the direction of the ball. To that end, the number of shots to the left and right was displayed above the left and right arm of the pinball machine. This task was added to encourage participants to select both actions and nonactions in the free choice trials.

In case of errors, an error message was displayed for 1000 ms and the trial was aborted. This included errors of commission (if participants responded wrongly in the forced choice trials) and trials in which participants pressed a key during the ball movement. At the end of each block, participants were informed about the number of errors, as well as the number of movements to the left and the right in the previous block.

The experiment consisted of 14 blocks with 36 trials each. Eight trials were forced action trials, eight trials forced nonaction trials, two trials were baseline trials, and eighteen trials were free choice trials (the number of actions and nonactions in the free choice trials depended on participants' choices). Most trials did not contain a question and ended directly after the ball vanished into one of the holes. In the remaining trials, the agency or the responsibility question was presented 500 ms after the ball movement had ended and remained on the screen until participants responded. Only one type of question was presented within one block (the agency question in eight blocks and the responsibility question in six blocks), and the order of blocks was determined randomly. Participants were informed about the current question type before a block started. The questions were presented in two randomly selected trials of the forced action and the forced nonaction trials and in six randomly selected free choice trials (equally often for ball movement directions to the left and the right). Furthermore, the agency question was presented in the two baseline trials, but the responsibility question was not since there was no action or nonaction in baseline trials.

4.2.2. Results

Figure 11 shows the mean ratings for the agency and the responsibility question in all conditions. Detailed descriptive statistics are listed in Table 4. For statistical analysis, all trials with errors were excluded (4.5%). This included trials with errors of commission in the forced choice condition (5.7% of the forced choice trials) and trials in which keypresses occurred after the ball movement had already been initiated (2.0% of all trials). In free choice trials, participants chose actions (*M* = 51.3%, *SE* = 2.1) and nonactions (*M* = 48.7%, *SE* = 2.1) equally often, $t(32) = 0.61$, $p = .544$, $d_z = 0.11$.

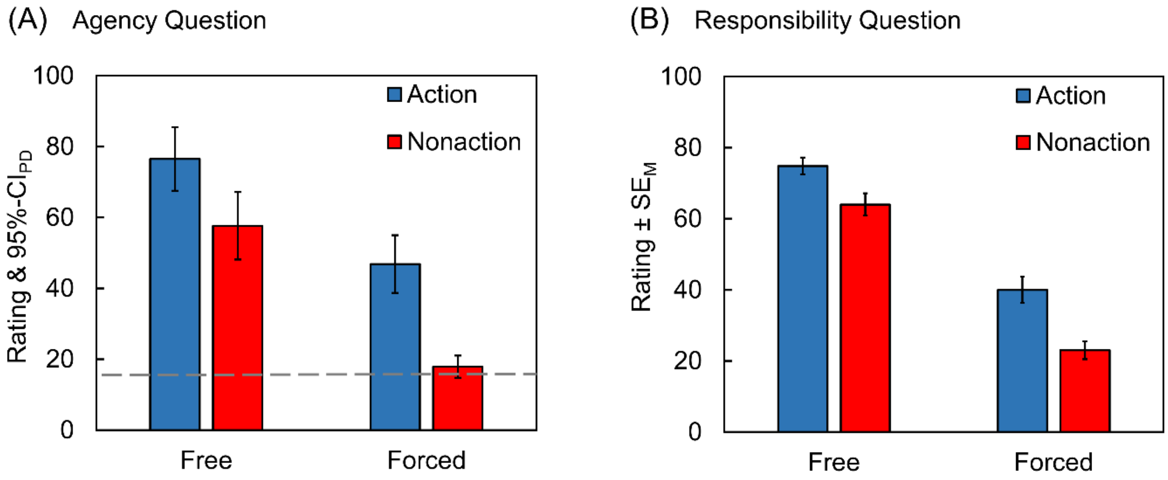

Figure 11. Mean ratings for Experiment 6 on a visual analog scale as a function of condition. (A) Mean agency ratings. The dashed line represents the mean agency rating in the baseline condition. Error bars indicate the 95%-confidence interval of paired differences (CI_{PD}) for the comparison of each bar with the baseline condition (cf. Pfister & Janczyk, 2013). (B) Mean responsibility ratings. There was no baseline condition for the responsibility question. Error bars show standard errors of the mean (SE_M).

Table 4. Mean ratings in response to the agency question and the responsibility question and the respective standard errors of the mean (*SE*) in Experiment 6 as a function of action type and choice.

	Free		**Forced**		**Baseline**
	Action	**Nonaction**	**Action**	**Nonaction**	
Agency question					
Mean rating (*SE*)	76.5 (2.5)	57.7 (3.7)	46.8 (3.8)	17.9 (2.2)	16.0 (2.6)
Responsibility question					
Mean rating (*SE*)	74.9 (2.4)	64.0 (3.1)	40.0 (3.7)	23.0 (2.5)	-

To analyze the mean ratings for the agency and the responsibility question, repeated-measures ANOVAs with the factors choice (free choice vs. forced choice) and action type (action vs. nonaction) were calculated. For all pairwise comparisons, two-tailed, paired *t*-tests were computed. Corresponding effect sizes were calculated as $d_z = \frac{t}{\sqrt{n}}$.

Agency for (non)action effects

Agency ratings were above baseline for all conditions, ts > 7.70, ps > .001, except for the forced nonaction condition, $t(32) = 1.25$, $p = .220$, $d_z = 0.22$. The ANOVA revealed a main effect of choice, $F(1,32) = 65.86$, $p < .001$, $\eta_p^2 = 0.67$, and main effect of action type, $F(1,32) = 63.59$, $p < .001$, $\eta_p^2 = 0.67$, as well as an interaction of choice and action type, $F(1,32) = 16.81$, $p < .001$, $\eta_p^2 = 0.34$. Planned comparisons with *t*-tests showed that agency ratings were lowest in the forced nonaction condition compared to all other conditions, all ts > 8.79, ps < .001.

Agency ratings were higher in the free action condition relative to the free nonaction condition, $t(32) = 5.92$, $p < .001$, $d_z = 1.03$, and the forced action condition, $t(32) = 6.35$, $p < .001$, $d_z = 1.11$. Agency ratings in the free nonaction condition were numerically higher than in the forced action condition, but the difference did not reach significance, $t(32) = 1.73$, $p = .093$, $d_z = 0.30$.

Responsibility for (non)actions.

The ANOVA on the responsibility ratings revealed a main effect of choice, $F(1, 32) = 81.52$, $p < .001$, $\eta_p^2 = 0.72$, showing that participants felt more responsible for free choice compared to forced choice actions and nonactions. Furthermore, a main effect of action type, $F(1, 32) = 36.83$, $p < .001$, $\eta_p^2 = 0.54$, revealed that participants felt more responsible for actions compared to nonactions. The interaction of choice and action type did not reach significance, $F(1,32) = 3.97$, $p = .055$, $\eta_p^2 = 0.11$, but hinted at smaller differences between actions and nonactions for free choice compared to the forced choice responses.

4.2.3 Discussion

Experiment 6 assessed whether a sense of agency is reported for nonaction effects. Agency ratings were indeed higher for nonaction effects that resulted from a free choice nonaction compared to the baseline condition. However, this was not the case if the effects resulted from a forced choice nonaction. Furthermore, ratings for freely chosen nonactions were lower than those for freely chosen actions. However, they were not lower compared to agency ratings for forced choice actions and the marginal significant trend rather showed the reversed pattern. Taken together, these results indicate that people can feel a sense of agency for nonaction effects if they freely decide not to act.

Forced choice nonactions, however, do not seem to elicit a sense of agency, unlike free nonactions, free actions and forced actions. The reason for this distinction might be the formation of intentions. Participants were encouraged to form intentions for free choice actions and nonactions, because they had to choose between these options. For forced choice actions, participants also needed to form intentions, because forced choice actions involved a motor response and participants thus needed to be determined to eventually execute this forced action. In contrast, this was not the case for forced choice nonactions and participants might not have formed any intention not to act in that case. Thus, forced choice nonactions might have been more similar to inactivity rather than to an intentional omission of actions (cf. Kühn et al., 2009). It is still possible that people occasionally form intentions for forced choice nonactions, for instance, if the nonaction is a rare event and motor responses have to be inhibited. However, to ensure that participants intentionally decide not to act, only free choice nonactions were used in the subsequent experiments.

Effects following actions always led to higher agency ratings compared to the baseline and agency ratings were higher for free choice compared to forced choice actions. This difference is in line with previous studies and might be driven both by top-down influences, such as participants’ control believes, and by low-level influences of action-selection processes in the motor system (Borhani et al., 2017; Sebanz & Lackner, 2007; Wenke et al., 2010).

Responsibility ratings mirrored the results of the agency question quite closely. Participants reported that they felt more responsible for freely chosen actions and nonactions compared to forced actions and nonactions. Thus, even when participants did not act, they clearly felt responsible for this decision.

Both, agency ratings and responsibility ratings were slightly higher for (free choice) actions compared to nonactions. This might reflect a general difference between actions and nonactions, for instance, because actions involve a specific motor pattern, whereas nonactions are characterized by the absence of a specific motor pattern. However, this difference might also be an artifact of the experimental setup. This will be discussed in more detail in the general discussion of this chapter.

All in all, the results of Experiment 6 indicate that a sense of agency can be felt for nonaction effects. However, as stated above, explicit agency ratings might be influenced by additional aspects that are not related to the sense of agency, such as demand effects (Moore, 2016). Implicit measures of the sense of agency might therefore yield different results and this was investigated in the following experiments.

4.3 Experiment 7

Experiment 7 was designed to test whether a sense of agency for nonaction effects would also be evident for implicit measures of agency. To that end, intentional binding for nonaction effects was measured using the clock procedure (Haggard, Clark et al., 2002). Participants saw a clock face on the screen and reported the position of a rotating clock hand when they heard a tone. In the baseline condition, the tones were presented at a randomly chosen point in time. In the operant condition, participants could choose on each trial between an action and a nonaction, which would result in a specific tone effect.

Typical intentional binding studies employing the clock design ask participants to judge not only the time of a tone, but also the time of their keypress. Thus, they can analyze the perceptual shift of the action and of the effect (i.e., action and outcome binding; see e.g., Beck, Di Costa, & Haggard, 2017; Borhani et al., 2017; Haggard, Clark et al., 2002; Ruess, Thomaschke, & Kiesel, 2017). In the present setup, however, the exact timing of a nonaction could not be measured. Thus, only outcome binding was assessed, that is, the difference between participant's perceived time of tone onset in the operant condition and the baseline condition. Some previous studies opted for a similar approach and analyzed only one type of binding if the experimental setup provided only one measure (e.g., Engbert & Wohlschläger, 2007; Haggard, Poonian, & Walsh, 2009; Moore & Haggard, 2008). In the present experiment, intentional binding would be evident if the tones were perceived earlier in the operant condition compared to the baseline condition (see **Figure 12**).

4.3.1 Method

Participants

Thirty-four participants were recruited (mean age = 21.2, *SD* = 2.2; 6 male; 4 left-handed). As there was no prior indicator of the effect size of intentional binding for nonactions, a medium effect size was assumed. An a priori power analysis suggested that 34 participants were needed to detect such an effect with a power of 1–β = .80. All participants gave informed consent prior to the experiment and received either course credit or monetary compensation for participation. The experiment and data analyses were pre-registered on the platform of the Open Science Framework (OSF) prior to data collection (https://osf.io/nzhrk).

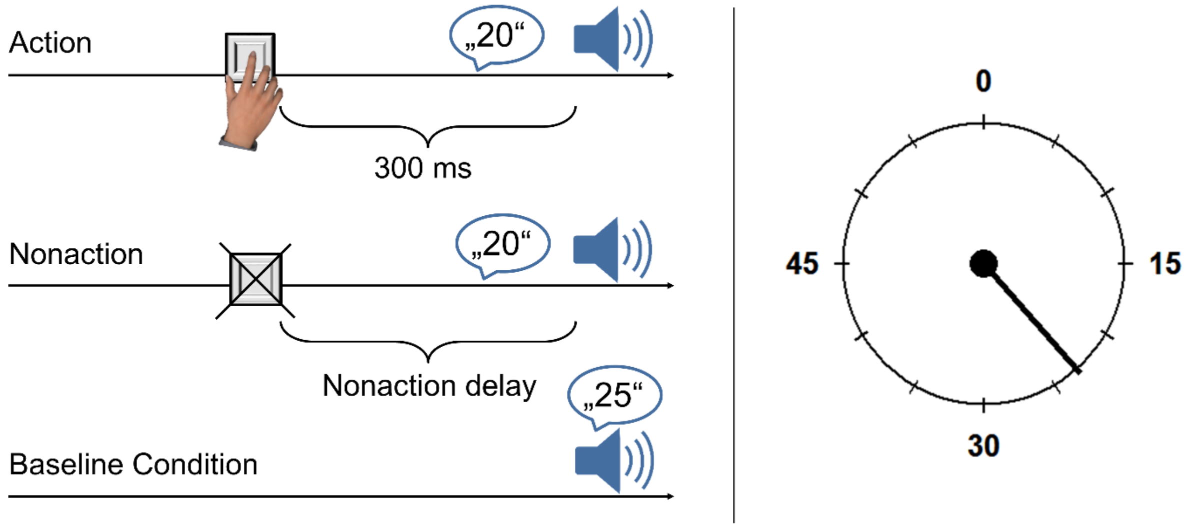

Figure 12. Illustration of the perceived time shift for the sound effect following an action and a nonaction compared to the baseline condition (left panel). Action sounds were always presented 300 ms after a keypress. Nonaction sounds were presented after a specific nonaction delay that was calculated from participants' past reaction times. Participants indicated the position of the clock hand (right panel) to estimate the time of sound presentation.

Stimuli and apparatus

Participants sat in front of a 17’’ monitor of a standard computer and watched a white clock face centrally presented on a black background (6 cm diameter). The clock hand needed 2560ms for a full rotation. One full rotation was labeled as 60 “minutes” and every five “minutes” (5, 10, 15…) were marked on the clock face (see **Figure 12**). The sound stimuli in the experiment were a high (600 Hz) and a low (300 Hz) sinusoidal tone of 100 ms duration, which were presented via headphones. Participants used the *V* key of a standard German QWERTZ keyboard with the index finger of the left hand to produce the sound effects and entered the estimated time of tone presentation using the number keys of the keyboard. The agency question for (non)action tones was similar to the one in Experiment 6 and read “*How strongly did you feel as causal agent for the tone in the current trial?*” (German original: *Wie sehr hast du dich gerade als Verursacher des Tones gefühlt?*). Participants responded on a visual analog scale ranging from 0 (“*a little*”) to 100 (“*a lot*”) by moving the mouse to the left and right.

Procedure

The experiment consisted of two conditions, a baseline and an operant condition, presented in different blocks. A clock face with a rotating clock hand was displayed on the screen. Participants’ task was to estimate the time of tone presentation using this clock. Each trial started with the display of the clock face, and the clock hand immediately started rotating.

In the baseline condition, one of the two tones was presented at a randomly chosen time between 750 and 5120 ms after trial start. After tone onset the clock hand kept rotating for another 2000 to 3000 ms. Then, the clock face disappeared, and participants were asked to enter the time of tone presentation in minutes. In the operant condition, participant could control the tone presentation with a keypress. The high and low tones were used as action and nonactions effects, respectively. The mapping of tone and response was held constant for each participant but was counterbalanced across participants. If participants pressed a key, the respective action tone was played 300 ms after the keypress. Participants were instructed to wait at least half a rotation of the clock hand before pressing a key and not to press the key at a predetermined point in time. To determine the time of the nonaction effect presentation, a participant's RT history (time between presentation of the clock face and a keypress) was used. To that end, each RT of a keypress was saved if it exceeded 1200 ms. The time of the nonaction effect presentation was then computed for each trials as (mean RT + mean RT + last RT) / 3 + 600 ms. If no RT history was available (i.e., at the beginning of each block), the time of the nonaction effect presentation was set to 2000 ms. If no keypress occurred between trial start and this time, the nonaction tone was presented. This approach was derived from former nonaction effect studies (e.g., Kühn & Brass, 2010a), but adjusted to prevent a presentation of the nonaction effect directly after trial start. If participants pressed the key during or after presentation of the nonaction effect, an error message occurred, and the trial was aborted. In correct trials, the clock hand kept rotating for another 1000 to 3000 ms after tone onset. Then, the clock face disappeared, and participants were asked to enter the time of tone presentation in minutes or to answer the agency question.

The experiment consisted of eight blocks in total, four blocks of the baseline condition and four of the operant condition. Blocks of different conditions alternated (ABABABAB) and the order of conditions was counterbalanced across participants. The baseline blocks consisted of 26 trials each. In two of these trials, participants had to answer the agency question instead of the time estimation question. The operant blocks consisted of 30 trials, of which six were trials with the agency question. Before the actual experiment, participants were familiarized with the clock hand and practiced time estimation in minutes using the clock hand. The practice phase consisted of six trials in the baseline condition and six trials in the operant condition.

4.3.2 Results

Figure 13 shows the mean results for agency ratings and intentional binding. Participants committed on average 1.1% errors. Trials with errors were excluded from all statistical analyses. For pairwise comparisons two-tailed, paired *t*-tests were computed. Corresponding effect sizes were calculated as $d_z = \frac{t}{\sqrt{n}}$. Participants chose actions (*M* = 49.9%, *SE* = 0.7) and nonactions (*M* = 50.1%, *SE* = 0.7) equally often, $t(33) = 0.08$, $p = .941$, $d_z = 0.01$.

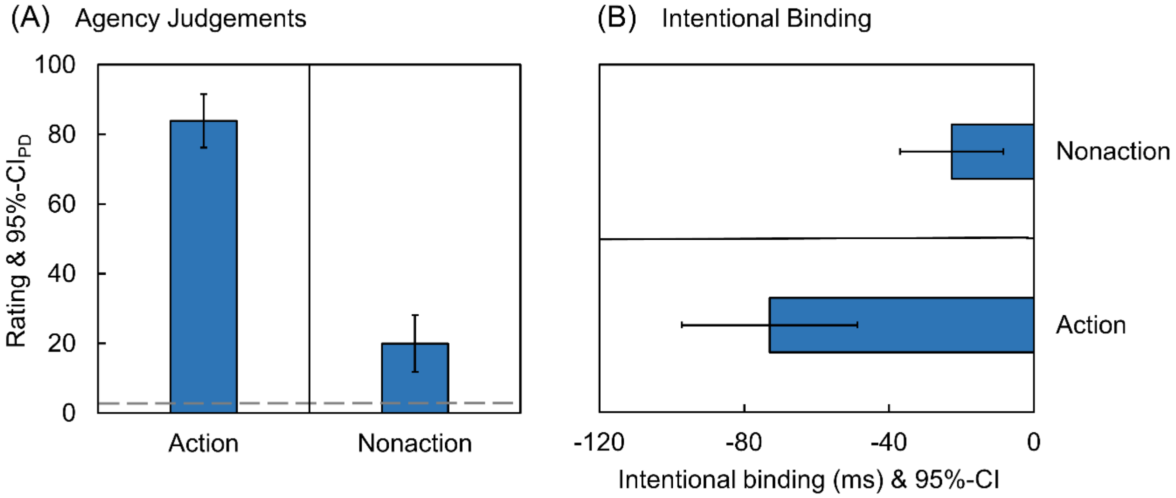

Figure 13. Results of Experiment 7. (A) Mean agency ratings. The dashed line represents the mean agency rating in the baseline condition. (B) Intentional binding for the effect tone (i.e., estimation of tone presentation minus actual time of tone presentation, relative to baseline). Error bars indicate the 95%-confidence interval of paired differences (CI$_{PD}$) for the comparison of the baseline condition with actions and nonactions, respectively (cf. Pfister & Janczyk, 2013).

Intentional binding

For the analysis of time estimations, trials deviating more than 2.5 standard deviations from the cell mean, calculated separately for each participant and condition, were excluded (1.9%). For each participant and condition, the mean estimation error was computed as participant's estimation of tone presentation minus actual time of tone presentation. Thus, a negative estimation error indicated earlier tone perception. The mean estimation errors did not differ between the two tones in the baseline condition (i.e., the tone that followed an action and the tone that followed a nonaction in the operant condition), $t(33) = 0.46$, $p = .647$, $d_z = 0.08$. Thus, estimation errors of the two tones were pooled for the baseline. To test for intentional binding, the estimation error in the baseline condition was compared to the estimation errors for action and nonactions in the operant condition, respectively. The tone was perceived earlier (i.e., the estimation error was more negative) following actions compared to the baseline condition (mean and standard error of paired differences, calculated for the operant-minus-baseline differences: $M_{PD} = -73$ ms, $SE_{PD} = 11.9$), $t(33) = 6.13$, $p < .001$, $d_z = 1.05$. The tone was also perceived earlier following nonactions compared to the baseline condition ($M_{PD} = -23$ ms, $SE_{PD} = 7.0$), $t(33) = 3.24$, $p = .003$, $d_z = 0.56$. However, tones following actions were perceived even earlier compared to tones following nonactions, $t(33) = 4.27$, $p < .001$, $d_z = 0.73$.

Agency ratings

Agency ratings did not differ between the two tones in the baseline condition, $t(33) = 0.16$, $p = .871$, $d_z = 0.03$. Thus, agency ratings were pooled for the baseline. Agency ratings were higher for action tones compared to baseline ($M_{PD} = 80.5$, $SE_{PD} = 3.8$), $t(33) = 21.34$, $p < .001$, $d_z = 3.66$ and for nonaction tones compared to baseline (M_{PD} = 16.5, SE_{PD} = 4.0), $t(33) = 4.13$, $p < .001$, $d_z = 0.71$. Agency ratings for action tones were even higher than agency ratings for nonaction tones, $t(33) = 12.49$, $p < .001$, $d_z = 2.14$.

4.3.3 Discussion

The present experiment set out to investigate whether a sense of agency is felt for nonaction effects using agency ratings and intentional binding as agency measures. Explicit agency ratings were higher for nonaction effects and action effects compared to baseline. This pattern mirrors the results of Experiment 6. Nonactions also produced significant intentional binding, that is, tones following nonactions were perceived earlier than tones in the baseline condition. Taken together, these results suggest that a sense of agency can be elicited by nonactions.

Actions also produced reliable intentional binding in line with previous studies (e.g., Haggard, Clark et al., 2002; Ruess et al., 2017) and intentional binding for actions was more pronounced than intentional binding for nonactions. Agency ratings for actions were also higher compared to agency ratings for nonactions, in line with Experiment 6. This difference between actions and nonactions will be discussed in more detail in the general discussion of this chapter.

In the present experimental setup, only outcome binding for nonactions could be assessed. Early studies on intentional binding have often subsumed action binding and outcome binding and provided one overall binding measure, assuming that the two types of binding are equal. Recently, however, it has been suggested that different mechanisms account for action and outcome binding (Waszak et al., 2012; Wolpe, Haggard, Siebner, & Rowe, 2013; Wolpe & Rowe, 2014). According to these models, outcome binding might be influenced by a pre-activation of the anticipated sensory effect, whereas action binding could be the result of a weighted cue integration process informed by different sources (see Moore, Wegner, & Haggard, 2009). Thus, action and outcome binding might be used to test different aspects of the sense of agency (Wolpe & Rowe, 2014). However, before drawing any conclusion about the potential mechanisms underlying intentional binding of nonactions from this distinction of action and outcome binding, I planned to strengthen the general finding of intentional binding for nonactions. Thus, Experiment 8a and 8b were designed to conceptually replicate the findings of Experiment 7. To that end, intentional binding was assessed using the interval estimation procedure instead of the clock procedure.

4.4 Experiment 8a

Experiment 8a was conducted to replicate the finding of intentional binding for nonactions using the interval estimation procedure (Engbert & Wohlschläger, 2007). To that end, the pinball setup of Experiment 6 was used. Participants' task was again to shoot the ball into the left or right arm of the pinball machine. A nonaction would shoot the ball into the pre-activated direction and an action would reverse the pre-activation and shoot the ball into the other direction. At the beginning of each trial, participants had to indicate whether they wanted to perform an action or a nonaction in the present trial. Following this decision, participants had to wait a certain time which was indicated by a progress bar. If participants had chosen an action, they were allowed to press the action key, as soon as the progress bar was filled completely. Shortly after participants' keypress, the ball was shot and participants had to indicate the interval between keypress and shot. If participants had chosen a nonaction, they heard a clicking sound (like a keypress) as soon as the progress bar was filled completely and shortly afterwards the ball was shot into the pre-activated direction. Participants had to indicate the interval between clicking sound and shot. Interval estimations in these conditions were compared to a baseline condition, which was similar to the nonaction condition, but participants could not choose the direction of ball movement and the ball was shot into one direction without participants' involvement. Interval estimation should be reduced when the ball movement results from an action or a nonaction compared to the baseline condition.

4.4.1 Method

Participants

Thirty-four participants were recruited (mean age = 28.4; *SD* = 11.2; 28 female, 2 left handed). An a priori power analysis suggested that this sample size ensured a power of at least 1–β = .80 to detect a medium effect size. All participants gave informed consent prior to the experiment and received either course credit or monetary compensation for participation. The data of three participants was replaced because the correlation between estimated and actual interval in the baseline trials was negative, suggesting that these participants had difficulties with the interval estimations. The experiment and data analyses were pre-registered on the platform of the Open Science Framework (OSF) prior to data collection (https://osf.io/y9mn8).

Stimuli and apparatus

Participants sat in front of a 24'' flat screen and used the key *C* of a standard German QWERTZ keyboard with the left index finger and the mouse with the right hand to give responses. All visual stimuli were presented on a black background. The pinball-like machine and the animated ball movement were identical to Experiment 6. Participants wore headphones and heard a pinball shooting sound of 650 ms duration whenever the ball was being shot to make the event more distinct for interval estimations. Likewise, a click sound of 200 ms duration was played in trials without keypresses to mark the start of a to-be-estimated interval.

To enter their time estimation, participants saw the question “*How long was the interval?”* (German original: *Wie lang was das Intervall?*), displayed in the upper part of the display. They responded on a visual analog scale ranging from 0 to 1000 ms with markers in steps of 100 ms by moving the mouse to the left and right. The agency question for (non)action effects was “*How strongly did you feel as causal agent for the ball movement to the left/right?*” (German original: *Wie sehr hast du dich als Verursacher der Ballbewegung nach links/rechts gefühlt?*). Participants could respond on a visual analog scale ranging from 0 (“*a little*”) to 100 (“*a lot*”) by moving the mouse to the left and right.

Experimental procedure

The experimental procedure is illustrated in **Figure 14**. At first, participants were familiarized with the instructions and completed ten baseline and ten operant practice trials. In the practice trials, all delays from 100 ms to 1000 ms in steps of 100 ms were used and participants received feedback about the accuracy of their estimation. In experimental trials, participants received no feedback and only three different delays were used (100 ms, 400 ms, 700 ms). The experiment consisted of four baseline blocks and four operant blocks presented in alternation. The order of block type was counterbalanced across participants. Baseline blocks consisted of 12 trials, operant blocks consisted of 24 trials.

Each trial started with the display of the pinball machine in the lower part of the display. The pinball was displayed on the screen throughout one trial. For 1000 ms, a red arrow was displayed above one arm to indicate the pre-activated direction. The current number of ball movements to the left and right within one block was displayed above the left and right arm of the pinball machine, respectively.

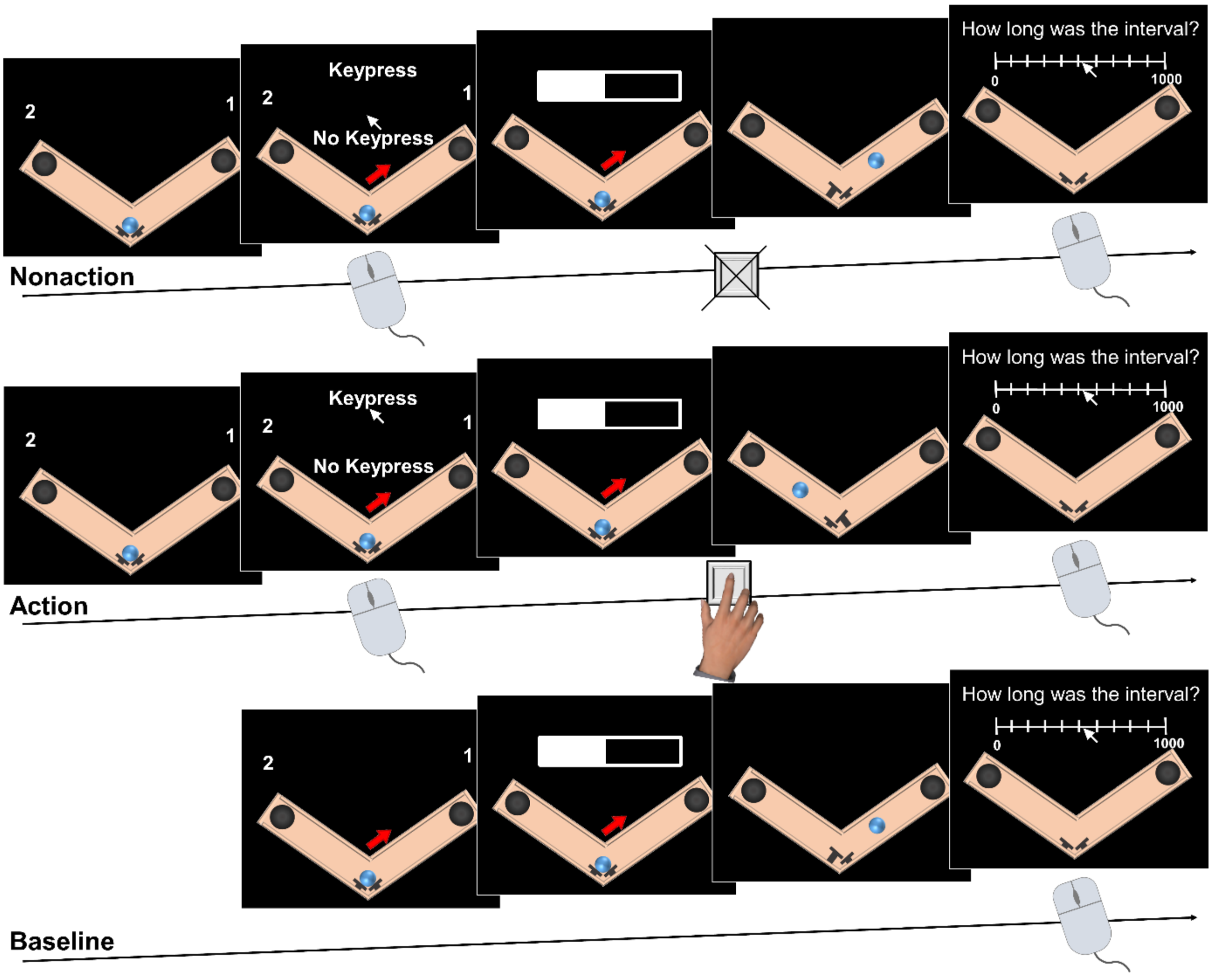

Figure 14. Illustration of the trial structure in Experiment 8a and 8b. Each trial started with the display of the pinball machine, the ball in rest and the number of shots to the left and right within a block, displayed above the respective pinball arms. Then, an arrow was displayed signifying the pre-activated direction and participants could choose between a keypress and no keypress using the mouse. After their selection, a progress bar appeared. When the progress bar was filled completely, the ball was shot into the pre-activated direction after a certain delay, if participants had chosen no keypress (nonaction, top row). If participants had chosen the keypress, they could press the key any time after the progress bar was filled completely, causing the ball to be shot in the opposite direction after a certain delay (action, middle row). In the baseline condition (bottom row), participants could not choose between a keypress and no keypress. The progress bar started automatically and when it was filled completely, the ball was shot in the pre-activated direction after a certain delay. After the ball movement, participants indicated their estimation of the delay on a visual analogue scale. In one out of four trials, the time estimation question was replaced by the agency question. Stimuli are not drawn to scale.

In operant trials, participants then saw the words "*keypress*" or "*no keypress*" (German original: *Tastendruck* or *Kein Tastendruck*) presented above each other in the upper part of the display. The mouse cursor was presented between these words and participants could select a keypress or no keypress by moving the mouse cursor onto the corresponding words, thus, selecting the direction in which the ball would be shot. When participants had chosen one option, the words were replaced by a progress bar, i.e., a white framed rectangle which was continuously filled for 1000 to 1500 ms. If participants had chosen the option “no keypress”, they heard a clicking sound as soon as the progress bar was filled completely and, following that, the ball was shot into the pre-activated direction. At the same time, participants heard a sound representing the shooting of the ball. If participants had chosen the option “keypress”, they were instructed to press the response key after the progress bar had been filled completely. Following the keypress, the ball was shot into the opposite direction of the pre-activation and participants heard the shooting sound. In both cases participants were instructed to estimate the delay between the clicking sound and the ball shooting sound or their own keypress and the ball shooting sound. Afterward, participants indicated their time estimation on the visual analogue scale. In one out of four randomly selected trials, the time estimation question was replaced by the agency question. If participants pressed a key during the filling of the progress bar or if they had chosen the option “no keypress” but pressed a key, an error message was presented for 1000 ms and the trial was aborted.

In baseline trials, participants could not choose the direction of the ball movement. The progress bar appeared in the upper part of the screen 1000 ms after trial start. When the bar was filled completely (after 1000 to 1500 ms), participants heard a clicking sound. Shortly afterward the ball was shot into the pre-activated direction and participants heard the shooting sound. Participants indicated their time estimation of the delay between the clicking sound and the ball shooting sound on the visual analogue scale. In one out of four randomly determined trials, the time estimation question was replaced by the agency question.

4.4.2 Results

For statistical analysis, all error trials (1.7%) were excluded. Intentional binding and agency ratings were investigated with a 3 x 3 repeated-measures ANOVA with the factors response type (baseline vs. action vs. nonaction) and delay (100 vs. 400 vs. 700 ms). For violations of the sphericity assumption, I report Greenhouse-Geisser corrected *p*-values along with the corresponding ε estimate for correcting degrees of freedom. Paired comparisons were analyzed with two-tailed, paired *t*-tests. Corresponding effect sizes were calculated as $d_z = \frac{t}{\sqrt{n}}$.

In the operant condition, participants chose actions ($M = 36.5\%$, $SE = 2.5$) less often than nonactions ($M = 63.5\%$, $SE = 2.5$), $t(33) = 5.48$, $p < .001$, $d_z = 0.94$. **Figure 15** (upper panels) shows the mean results of intentional binding and agency ratings.

Intentional Binding

For analysis of the interval estimations, all trials with estimations that deviated more than 2.5 standard deviation from the cell mean, calculated separately for each participant, response type (baseline, action, nonaction) and delay (0.4%) were excluded. Unexpectedly, for some participants the number of observations per cell was very low (≤ 2). This was due to a highly uneven distribution of action and nonactions. Even though not stated in the pre-registration, seven participants were therefore excluded from further analyses.

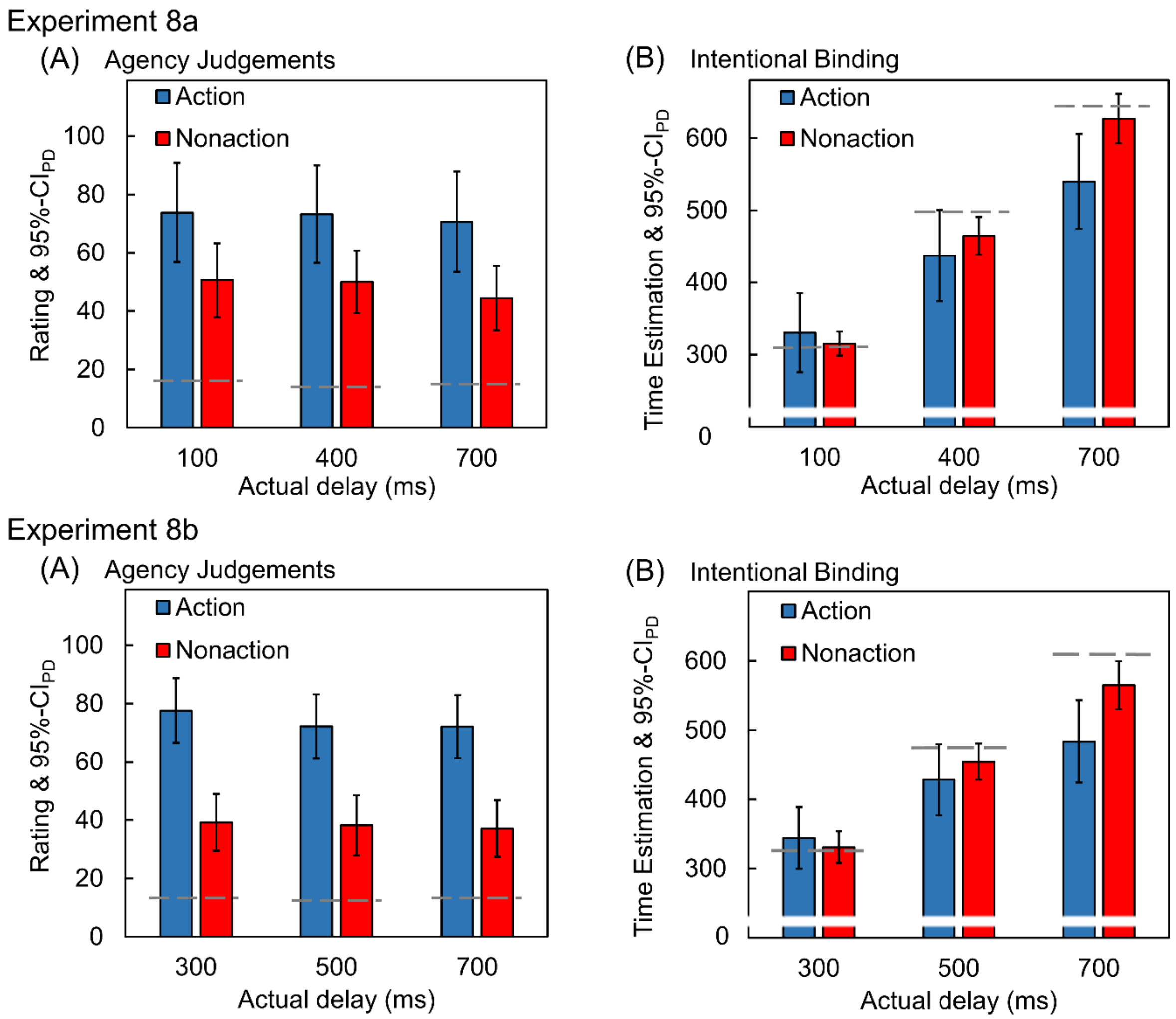

Figure 15. Results of Experiment 8a (upper panels) and 8b (lower panels). (A) Mean agency ratings. The dashed lines represent agency ratings in the baseline condition. (B) Mean time estimations. The dashed lines represent the time estimation in the baseline condition. Error bars indicate the 95%-confidence interval of paired differences (CI$_{PD}$) for the comparison of each bar with the respective baseline condition of the delay (cf. Pfister & Janczyk, 2013).

The ANOVA revealed a main effect of delay, $F(2,52) = 56.10$, $p < .001$, $\eta_p^2 = 0.68$ ($\varepsilon = .57$), and an interaction of delay and response type, $F(4,104) = 5.52$, $p = .009$, $\eta_p^2 = 0.18$ ($\varepsilon = .45$). The main effect of response type was not significant, $F(2,52) = 2.92$, $p = .091$, $\eta_p^2 = 0.10$ ($\varepsilon = .61$). To follow up on the interaction, planned *t*-tests were calculated.

For actions, the delay of 700 ms was perceived shorter compared to the baseline condition (mean and standard error of paired differences, calculated for the baseline-minus-operant differences: $M_{PD} = 104$ ms, $SE_{PD} = 31.9$),

$t(26) = 3.27$, $p = .003$, $d_z = 0.63$. This was also the case descriptively for the delay of 400 ms ($M_{PD} = 61$ ms, $SE_{PD} = 30.7$), but the t-test did not approach significance, $t(26) = 1.97$, $p = .059$, $d_z = 0.38$. The delay of 100 ms was not perceived shorter compared to the baseline condition ($M_{PD} = –20$ ms, $SE_{PD} = 26.6$), $t(26) = –0.74$, $p = .468$, $d_z = –0.14$.

For nonactions, the delay of 400 ms was perceived shorter compared to the baseline condition ($M_{PD} = 33$ ms, $SE_{PD} = 12.7$), $t(26) = 2.62$, $p = .014$, $d_z = 0.50$. This was also the case descriptively for the delay of 700 ms ($M_{PD} = 18$ ms, $SE_{PD} = 16.6$), but the t-test was not significant, $t(26) = 1.06$, $p = .299$, $d_z = 0.20$. The delay of 100 ms was not perceived shorter compared to the baseline condition ($M_{PD} = –5$ ms, $SE_{PD} = 8.1$), $t(26) = –0.56$, $p = .579$, $d_z = –0.11$. The compression effect for the delay of 400 ms did not differ between actions and nonactions, $t(26) = 0.88$, $p = .386$, $d_z = 0.17$, but the compression effect for the delay of 700 ms was more pronounced for actions compared to nonactions, $t(26) = 2.36$, $p = .026$, $d_z = 0.45$.

Agency Ratings

For analysis of the agency ratings, one additional participant had to be excluded because of empty cells. The ANOVA revealed a main effect of response type, $F(2,50) = 39.03$, $p < .001$, $\eta_p^2 = 0.61$ ($\varepsilon = .79$). Neither the main effect of delay, $F(2,50) = 1.88$, $p = .175$, $\eta_p^2 = 0.07$ ($\varepsilon = .74$), nor the interaction of response type and delay, $F(4,100) = 0.76$, $p = .526$, $\eta_p^2 = 0.03$ ($\varepsilon = .79$), were significant. Planned t-tests showed that agency ratings were higher for actions and nonactions compared to the baseline, actions (operant–baseline: $M_{PD} = 58.0$, $SE_{PD} = 7.9$): $t(25) = 7.38$, $p < .001$, $d_z = 1.45$; nonactions ($M_{PD} = 33.8$, $SE_{PD} = 4.9$): $t(25) = 6.93$, $p < .001$, $d_z = 1.36$. Agency ratings were even higher for actions compared to nonactions, $t(25) = 3.92$, $p = .001$, $d_z = 0.77$.

4.4.3 Discussion

The present experiment assessed whether a sense of agency is felt for nonaction effects using explicit agency ratings and intentional binding, measured with direct interval estimations, as agency measures. Unexpectedly, some participants had to be excluded from statistical analysis because of a low number of trials per condition. Therefore, the experiment was repeated with improvements to increase the number of trials per condition and participant, before drawing any conclusions from the results.

4.5 Experiment 8b

Experiment 8b was a replication of Experiment 8a with an increased number of trials and different delays between action, nonaction and effects. These changes were implemented to reduce the short-comings of Experiment 8a. As in the previous experiment, interval estimation should be reduced when the ball movement results from an action and a nonaction compared to the baseline condition.

4.5.1 Method

Participants

Forty participants were recruited (mean age = 26.8; *SD* = 7.6; 29 female, 2 left handed). An a priori power analysis based on the effect size for intentional binding of nonactions found in Experiment 8a suggested that this sample size ensured a power of more than 1–β = .80. All participants gave informed consent prior to the experiment and received either course credit or monetary compensation for participation. Participants were replaced when there were only five or less observation per cell available (because of an uneven choice of actions and nonactions; this applied to one participant) and when the correlation between estimated delay and actual delay in the baseline trials was negative (this applied to six participants). The experiment and data analyses were pre-registered on the platform of the Open Science Framework (OSF) prior to data collection (https://osf.io/ucwpq).

Stimuli, apparatus and experimental procedure

Stimuli, apparatus and experimental procedure were identical to Experiment 8a except for two modifications. The delays were increased to 300, 500, and 700 ms. Furthermore, the number of trials per block was increased. The baseline blocks now consisted of 18 trials, the operant blocks consisted of 36 trials.

4.5.2 Results

For statistical analysis, all error trials (1.2%) were excluded. Intentional binding and agency ratings were investigated with a 3 x 3 repeated-measures ANOVA with the factors response type (baseline vs. action vs. nonaction) and delay (300 vs. 500 vs. 700 ms). For violations of the sphericity assumption, I report Greenhouse-Geisser corrected *p*-values along with the corresponding ε estimate for correcting degrees of freedom. Paired comparisons were analyzed with two-tailed, paired *t*-tests. Corresponding effect sizes were calculated as $d_z = \frac{t}{\sqrt{n}}$.

In the operant condition, participants chose actions ($M = 37.4\%$, $SE = 1.8$) less often than nonactions ($M = 62.6\%$, $SE = 1.8$), $t(33) = 6.84$, $p < .001$, $d_z = 1.08$. **Figure 15** (lower panels) shows the mean results for intentional binding and agency ratings.

Intentional Binding

For analysis of the interval estimations, all trials with estimations that deviated more than 2.5 standard deviation from the cell mean, calculated separately for each participant, response type (baseline, action, nonaction) and delay were excluded (1.0%). The ANOVA revealed a main effect of delay, $F(2,78) = 99.40$, $p < .001$, $\eta_p^2 = 0.72$ ($\varepsilon = .56$), and an interaction of delay and response type, $F(4,156) = 12.64$, $p < .001$, $\eta_p^2 = 0.25$ ($\varepsilon = .71$). The main effect of response type did not reach significance, $F(2,78) = 3.48$, $p = .056$, $\eta_p^2 = 0.08$ ($\varepsilon = .66$). To follow up on the interaction, planned *t*-tests were calculated.

For actions, the delay of 700 ms was perceived shorter compared to the baseline condition (mean and standard error of paired differences, calculated for the baseline-minus-operant differences: $M_{PD} = 124$ ms, $SE_{PD} = 29.5$), $t(39) = 4.22$, $p < .001$, $d_z = 0.67$. This was also the case descriptively for the delay of 500 ms ($M_{PD} = 46$ ms, $SE_{PD} = 25.4$), but the test did not reach significance, $t(39) = 1.81$, $p = .079$, $d_z = 0.29$. The delay of 300 ms was not perceived shorter compared to the baseline condition ($M_{PD} = –15$ ms, $SE_{PD} = 22.0$), $t(39) = –0.69$, $p = .494$, $d_z = –0.11$.

For nonactions, the delay of 700 ms was perceived shorter compared to the baseline condition ($M_{PD} = 43$ ms, $SE_{PD} = 17.0$), $t(39) = 2.52$, $p = .016$, $d_z = 0.40$. This was also the case descriptively for the delay of 500 ms ($M_{PD} = 20$ ms, $SE_{PD} = 13.0$), but the test did not reach significance, $t(39) = 1.52$, $p = .137$ $d_z = 0.24$. The delay of 300 ms was not perceived shorter compared to the baseline condition ($M_{PD} = –2$ ms, $SE_{PD} = 11.3$), $t(39) = –0.16$, $p = .870$, $d_z = –0.03$. Even though the delay of 700 ms was perceived shorter following actions and nonaction, this compression effect was more pronounced following actions compared to nonactions, $t(39) = 2.88$, $p = .006$, $d_z = 0.46$.

Agency Ratings

For analysis of the agency ratings, three participants were excluded because of empty cells. The ANOVA revealed a main effect of delay, $F(2,72) = 3.18$, $p = .047$, $\eta_p^2 = 0.08$, hinting at higher agency ratings for shorter delays. Furthermore, there was a main effect of response type, $F(2,72) = 83.83$, $p < .001$, $\eta_p^2 = 0.70$. The interaction of response type and delay was not significant, $F(4,144) = 1.53$, $p = .208$, $\eta_p^2 = 0.04$ ($\varepsilon = .79$). Planned *t*-tests showed that agency ratings were higher for actions and nonactions compared to the baseline; actions (operant–baseline: $M_{PD} = 60.6$, $SE_{PD} = 5.1$): $t(36) = 11.88$, $p < .001$, $d_z = 1.95$; nonactions ($M_{PD} = 24.8$, $SE_{PD} = 4.7$): $t(36) = 5.32$, $p < .001$, $d_z = 0.87$. Agency ratings were even higher for actions compared to nonactions, $t(37) = 8.58$, $p < .001$, $d_z = 1.41$.

4.5.3 Discussion

Experiment 8a and 8b assessed whether a sense of agency is felt for nonaction effects using explicit agency ratings and intentional binding, measured with direct interval estimations. Experiment 8a and 8b yielded converging results. Agency ratings were higher for nonaction effects compared to the baseline condition. Furthermore, effect delays were judged shorter following nonactions compared to the baseline condition. These results are in line with Experiment 7 and indicate that a sense of agency for nonaction effects can be found with explicit, as well as implicit agency measures.

Agency ratings for actions were also higher compared to the baseline condition and effect delays were judged shorter following actions compared to the baseline condition. As for nonactions, however, only some action-effect delays were judged shorter compared to the baseline. To date, it is still not clear how intentional binding is influenced by the duration of action-effect delays (Ruess, Thomaschke, & Kiesel, 2018). In the present study, intentional binding was absent (even descriptively) for the smallest intervals of 100 ms and 300 ms. In contrast, some previous studies have shown intentional binding for such short delays (e.g., Engbert et al., 2008; Ruess et al., 2017). Other studies suggest that intentional binding (at least as measured with the interval estimation procedure) is reduced for these short delays and increases only with longer delays (Humphreys & Buehner, 2009; Vastano, Pozzo, & Brass, 2017), which is in line with the present results. The results of Experiment 8a and 8b further indicate that not only the absolute value of a delay influences interval estimations and intentional binding, but rather the interpretation of the delay within its context. For instance, the delay of 300 ms was the shortest delay in Experiment 8b, whereas it would have been a medium delay in Experiment 8a. This may be the reason why the mean estimation of the 100ms-delay in Experiment 8a is almost as high as the mean estimation of the 300ms-delay in Experiment 8b. Previous studies were also able to show that the temporal context of a delay influences its time perception (Jazayeri & Shadlen, 2010). Thus, the absence of intentional binding for actions and nonactions for smaller delays might be due to the temporal context. In addition, the temporal separation of response decision and execution in the present setup might have further altered the temporal context. To what degree this temporal separation influences intentional binding needs further clarification. However, the results of Experiment 8a and 8b yield converging evidence that nonactions can produce intentional binding as measured with the interval estimation procedure.

4.6 General discussion and interim conclusion

The experiments of this third empirical part tested whether effects that result from a nonaction evoke a sense of agency. In four experiments, participants reported enhanced agency for nonaction effects compared to respective baseline conditions. Because explicit agency ratings might be susceptible to demand effects (Moore, 2016), the sense of agency was also investigated using intentional binding. Experiments using the clock procedure and the interval estimation procedure showed intentional binding for nonaction effects. Taken together, these results suggest that nonaction effects can elicit a sense of agency.

At this point, one can only speculate about the potential mechanisms generating the sense of agency for nonactions and their effects. On the one hand it is likely that inferential processes are involved, e.g., when people believe that they can control and influence nonaction effects. A systematic manipulation of these believes should thus influence the sense of agency. On the other hand, predictive processes might also be involved in the sense of agency for nonactions. This claim is tentatively supported by results from Experiment 7, which revealed outcome binding for nonaction effects. It has been suggested that predictive process might be particularly involved in outcome binding (as compared to action binding; Waszak et al., 2012; Wolpe et al., 2013; Wolpe & Rowe, 2014). However, a direct test for the differential involvement of predictive and inferential processes in the sense of agency for nonactions remains to be conducted.

Agency ratings were consistently enhanced for nonactions compared to respective baseline conditions. However, they seemed to differ in their absolute size between experiments. Agency ratings appeared reduced in the clock paradigm (Experiment 7; mean rating: 20), compared to the experiments using the pinball machine (Experiment 6, 8a, 8b; mean ratings: 38–58). This difference might be due to the slightly more naturalistic setting of the pinball experiments. In the more naturalistic setting, participants' belief that they can control the pinball might have been enhanced, pointing to an involvement of inferential processes in the sense of agency for nonactions.

Even though a sense of agency for nonaction effects was found in the present experiments, it was still reliably reduced compared to the sense of agency for action effects. This difference could reflect a general difference between actions and nonactions. Specific motor patterns are involved when performing an action but not when performing a nonaction. Furthermore, while actions generally are the unique cause of an effect, nonactions cannot cause nonaction effects directly, but only if the situation permits it. Nonactions might therefore only be able to elicit a reduced sense of agency.

However, in the present experimental setups, nonactions and actions also differed with respect to the timing of effect presentation. For actions, the effect was always presented after a specific interval following the keypress. However, for nonactions the timing of the effect presentation had to be inferred from participant's actions, as the exact time of participants' decision not to act could not be measured (Experiment 6 and 7) or participant's decision was deliberately temporally separated from the nonaction effect (Experiment 8a and 8b). Previous studies, however, suggest that agency decreases if the interval between an action and the resulting effects is prolonged (e.g., Shanks, 1989; van Elk, Salomon, Kannape, & Blanke, 2014; Weller et al., 2017). Thus, reduced agency for nonaction effects may also partly stem from the fact that the delay between nonaction and effect was larger compared to the delay between action and effect.

Further studies might explore the range and the limits of a sense of agency for nonaction effects. For instance, sensory attenuation of nonaction effects might be investigated as another implicit marker of the sense of agency. It has previously been suggested that nonactions might cause sensory attenuation of the nonaction effects (Weller et al., 2017). Such a finding could provide further evidence for the involvement of predictive processes in the sense of agency for nonactions, since sensory attenuation seems to be particularly influenced by predictive rather than inferential processes (Bays, Flanagan, & Wolpert, 2006; Weller et al., 2017). It is also conceivable that a sense of agency for nonaction effects might be absent in specific situations where this is beneficial for self-evaluation. For instance, no sense of agency might be felt if not acting results in a negative outcome (as e.g., for the bank customers in Essen, who did not help the man in need; Burger, 2017, September 18). While further studies need to explore these advanced approaches, the experiments of this third empirical part substantiate the notion that a sense of agency for nonactions can emerge.

5. Principles of not acting

The present dissertation set out to provide a deeper understanding of nonactions and to investigate whether nonactions share essential characteristics with actions. It was proposed that nonactions, just like actions, are represented in terms of their sensory effects. This approach offered new possibilities to study nonactions and allowed for specific predictions that were tested in three empirical parts.

In the first empirical part (Experiments 1–3, Chapter 2), I investigated whether nonactions and the resulting effects can be bound to each other in a bidirectional manner. The experiments showed that bidirectional associations between nonactions and effects can be formed. Furthermore, these associations did not seem to differ from associations between actions and their effects.

In the second empirical part (Experiments 4 and 5, Chapter 3), I tested whether planning a nonaction includes an anticipation of nonaction effects. The experiments showed small effects of anticipated nonaction effects and suggested that a representation of the nonaction effects is activated when a nonaction is planned. However, they also showed that planning an action and planning a nonaction can have opposite consequences on subsequent actions, indicating that while actions and nonactions can employ the same mechanisms (such as effect anticipation), they are clearly distinct instances.

Lastly, in the third empirical part (Experiments 6–8, Chapter 4), I targeted the question whether effects resulting from a nonaction are perceived as self-produced effects and elicit a sense of agency. Converging evidence from these experiments indicated that a sense of agency for nonaction effects can emerge.

These results confirm the predictions that were set forth in the beginning and provide evidence for the idea that nonactions are represented in terms of their sensory effects. The findings of these experiments further suggest that the same mechanisms can be recruited by actions and nonactions, even though there are still critical differences between actions and nonactions. Taken together, the results demonstrate that nonactions should be seen as an integral part of goal-directed behavior.

5.1 Qualities of not acting

The results of the present experiments showed that actions and nonactions share essential characteristics and that the same mechanisms can be involved in actions and nonactions. The main difference between actions and nonactions is that actions are characterized by a specific motor pattern whereas nonactions are characterized by the absence of the respective motor patterns. However, both involve an anticipation of certain desired effects. It seems that this anticipation accounts for a range of findings in voluntary actions, whereas the presence of a distinct motor pattern might often be negligible.

The experiments also showed that nonactions (i.e., intentional decisions not to act) need to be distinguished from mere inactivity. Even though for an outside observer inactivity and nonactions look alike (e.g., a person is sitting still), nonactions might in fact be more similar to actions than to inactivity, as they can involve a representation of the anticipated effects and elicit a sense of agency. Consequently, it seems that intentional nonactions recruit similar brain circuits as intentional actions (Kühn & Brass, 2009), whereas that is not the case for mere inactivity (Kühn, Bodammer, & Brass, 2010). This needs to be considered in an experimental context where results might differ if participants perform a nonaction instead of just being inactive (see Weller et al., 2017, for an example). The peculiarity of nonactions (as opposed to mere inactivity) has also been highlighted by previous studies which showed that trying not to act can, under specific circumstances, be even more resource-demanding than acting (Huestegge & Koch, 2014; Langhanns & Müller, 2018; Raettig & Huestegge, 2018). In one study, for instance, participants were instructed to “stay rock-still”, which produced a higher cortical load compared to situations where participants were instructed to relax or move easily (Langhanns & Müller, 2018).

These findings demonstrate that the absence of an action does not allow the simple conclusion that people are inactive. It rather requires a detailed inspection of what people are doing and how they process the potential effects that result from this absence of action. Further research that takes this principle into account will broaden our understanding of nonactions. The methods used in the present experiments show that nonactions can be investigated under specific circumstances, especially if their influence on the subsequent processing of actions or effects can be measured. In addition, neuroimaging methods could be used in the future to provide further understanding of nonactions.

5.2 Effect-based representation of nonactions

5.2.1 Components of nonaction-effect associations

Initial formulations of ideomotor theory suggested that the perceptual effects of actions are bound to the motor patterns that produced these effects (e.g., James, 1890/1981). However, the findings of the present experiments suggest that ideomotor theory also applies in situations where people do not act.

Previous studies on goal-directed actions have been able to show that the execution of a motor activity is in fact not necessary to form associations. Action-effect associations can also be learned by observing another person's actions (Paulus, van Dam, Hunnius, Lindemann, & Bekkering, 2011) or when action-effect associations are verbally instructed (Eder & Dignath, 2017). In this case, associations can still consist of a specific motor pattern and the effects, because the particular motor pattern can be planned or simulated. However, for nonactions there should be no specific motor pattern altogether. This raises the question, what exactly can be bound to the nonaction effects in nonaction-effect associations.

As suggested in the introduction, effect-based representations of nonactions should be context-specific, since nonactions can only produce desired effects where the circumstances permit. Nonaction-effect associations thus need to consist of an association between a certain situation where people decide not to act and the resulting effects.

Nonaction-effect associations, however, might consist of additional components, apart from the nonaction situation and the resulting effects. For instance, a nonaction might cause a general suppression of all movements. Such a global suppression of the motor system has been observed when an action is stopped. Stopping the hand can, for instance, also reduce corticospinal excitability in the leg (Badry et al., 2009) and stopping a saccade can reduce corticospinal excitability in the hand (Wessel, Reynoso, & Aron, 2013). Nonaction-effect associations might thus consist of the nonaction situation, a general suppression of motor activity and the resulting nonaction effects.

It is also conceivable that nonaction-effect associations comprise a specific motor pattern and are comparable to action-effect associations after all. Previously it has been suggested that such a specific motor pattern could be the activation of an opposing action (e.g., deciding not to press down a key with the index finger could result in the contrary action of lifting the index finger; Kühn & Brass, 2010a). However, the motor pattern could also be unrelated to the omitted action. For instance, people might deliberately choose to strengthen the omission of an action by executing a different action (like omitting a certain keypress action, while stomping with the foot at the same time). Furthermore, trivial motor patterns, like blinking or breathing, might also become bound to nonaction effects, if they are coincidentally carried out at the same time as the nonaction. Nonaction-effect associations might thus also exist of the nonaction situation, a certain motor pattern and the resulting nonaction effects.

Taken together, there are different possibilities of what can become bound to nonaction effects in nonaction-effect associations and the exact composition of these associations needs to be investigated by future research. The above considerations represent potential avenues for this investigation. However, the data of the present experiments also allows for some tentative conclusions. In Experiment 4 and 5, participants performed actions slower and committed more errors after having performed a nonaction rather than a (different) action. These problems to re-initiate an action after a nonaction (in contrast to the initiation of one action after another action) suggests that nonactions differ from actions.

This finding speaks against the involvement of specific (action-like) motor patterns in nonaction-effect associations, suggesting that nonaction-effect associations are distinct from action-effect associations.

5.2.2 Nonactions and intentional inhibition

Nonactions have been defined as instances where people intentionally omit an action. This definition resembles the definition of intentional *response inhibition*, a term that refers to the omission and suppression of no longer required or inappropriate actions (e.g., Verbruggen & Logan, 2008). Both concepts, nonactions and (intentional) response inhibition, focus on instances where actions are deliberately omitted. The concept of response inhibition further emphasizes the possibility to stop ongoing motor activity and the main goal of response inhibition is to prevent certain effects (namely the effects of the action that is to be stopped).

The concept of response inhibition has motivated a considerable amount of research studies, which have used different paradigms to study response inhibition. A widely used paradigm is the stop signal paradigm (Logan & Cowan, 1984; see e.g., Verbruggen & Logan, 2008, for a review), in which participants perform a go task, e.g., by responding to the identity of a stimulus with a specific keypress. Occasionally, a stop signal is presented after the stimulus and participants have to withhold their response in these trials. Another frequently used paradigm to study response inhibition is the go/no-go paradigm (for an early study on such designs, see Donders, 1969). In this case, participants only observe one stimulus per trial and the stimulus can either tell them to execute a response (go stimulus) or to withhold a response (no-go stimulus).

In these paradigms, participants do not act (i.e., inhibit an action) and this behavior has specific, foreseeable consequences, like specific proprioceptive and visceral changes and also distal effects, like the absence of an error message and the start of a new trial. It has therefore recently been suggested that intentional response inhibition is represented in terms of these effects and is also controlled in an effect-based way (Ridderinkhof, van den Wildenberg, & Brass, 2014). This idea provides a parsimonious theoretical rationale for action control, as it assumes essentially the same mechanisms for actions and response inhibition. Furthermore, a distinction between nonactions and response inhibition is no longer pertinent and necessary. The only difference between what has been termed “response inhibition” and a nonaction is whether the anticipated effects relate predominantly to proprioceptive feedback or to distal, environmental feedback, but the anticipation of these effects generally leads to the suppression of an action. A comprehensive model of action and nonaction control would therefore suggest that all actions and nonactions are controlled in terms of their anticipated effects. For instance, the desire of a bright room generally activates an action to press the light switch when entering a room. In specific situations, however, a nonaction might be activated, for example, when a motion detector turns on the light. Occasionally, desired effects might change due to situational circumstances. For instance, if an action to press the light switch has been activated, but the light suddenly turns on by itself, a desire not to execute the activated action might arise. An anticipation of the respective proprioceptive effects should again activate the nonaction, thus preventing the action (see **Figure 16** for an illustration of such a model).

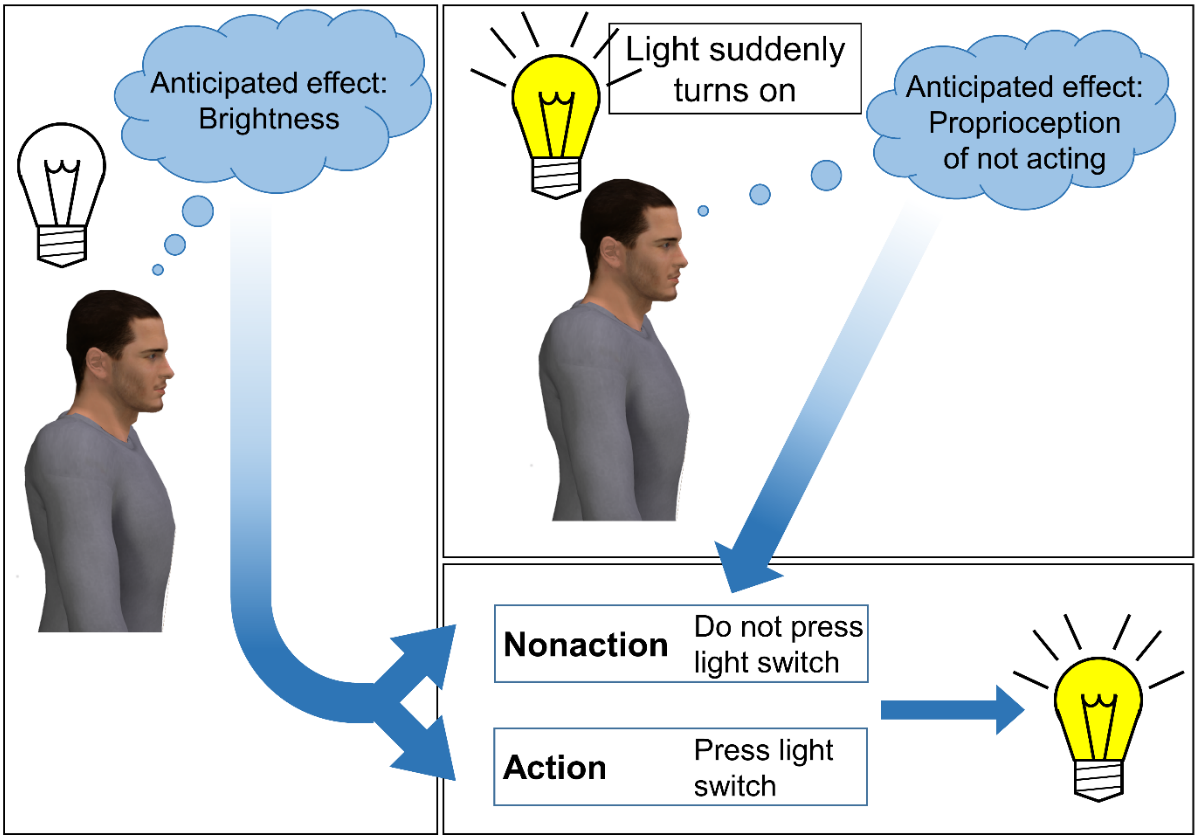

Figure 16. Model for an effect-based control of actions and nonactions. The anticipation of a desired effect activates the appropriate action or nonaction in a given situation. If the situation changes in the course of this sequence, a desire stop an activated action might occur. Anticipating the respective proprioceptive effects then triggers the nonaction, thus preventing the action.

5.3 Further instances of nonactions

So far, the effect-based representation of nonactions has been studied in the manual domain, but the results of this research might be transferrable to the oculomotor domain. Previous studies have indeed suggested that eye movements can also be controlled in an effect-based manner (Herwig & Horstmann, 2011; Huestegge & Kreutzfeldt, 2012; Riechelmann, Pieczykolan, Horstmann, Herwig, & Huestegge, 2017). Thus, nonactions might also be performed in anticipation of a certain change of the retinal input. For instance, when an object moves in the field of vision, one might decide not to move the eyes and not to follow the object, but to remain still to let the object disappear from the field of vision and see what lies behind the object. Such situations might provide a good testbed for future research of nonactions.

Instances of not acting, while taking the resulting effects into account, cannot only be investigated from the viewpoint of action control. Peculiarities of nonactions have also been highlighted by other branches of psychological research, for example, in the context of dishonest behavior. Here, active lying, i.e., telling an untrue fact, can be distinguished from omitting the truth. For example, a physician might actively lie to a cancer patient by telling the patient that there is a good chance to cure the disease even though that is not the case. On the other hand, the patient could be convinced that there is good chance to cure the disease and the physician might decide not to correct the patient. Interestingly, the lying person generally judges the overt telling of a lie to be worse than omitting information, whereas the receiver of the lie often believes that omitting information is even worse than telling a lie (Levine et al., 2018). Recent research about the cognitive mechanisms of active lying has found that telling a lie is normally more difficult and takes longer than telling the truth (see

Suchotzki, Verschuere, van Bockstaele, Ben-Shakhar, & Crombez, 2017, for a meta-analysis), even though the process can be modulated, e.g., when people invent a false alibi (Foerster, Wirth, Herbort, Kunde, & Pfister, 2017) or when they have recently told a lie (Foerster et al., 2018). It would be interesting to investigate whether similar mechanisms are recruited when the dishonest behavior is a nonaction, i.e., an omission of the truth, rather than the active telling of a lie.

Nonactions have also been investigated in some well-known psychological paradigms. For instance, in some moral dilemmas, such as the trolley dilemma (Thomson, 1976), people have to weigh the costs and benefits of acting (like sacrificing the life of one person to save many) against not acting (and letting the group of people die; e.g., Navarrete, McDonald, Mott, & Asher, 2012). Furthermore, nonactions are involved in the still face paradigm, in which parents cease interaction with their infant and maintain a neutral face. The nonaction of the parents has specific effects on the infants, who will stop smiling, look away and show negative facial expression (Tronick, Als, Adamson, Wise, & Brazelton, 1978; see Mesman, van IJzendoorn, & Bakermans-Kranenburg, 2009, for a meta-analysis). In these situations, not acting can have grave consequences. Findings from the present experiments would still suggest that people have a sense of agency for these consequences, but this needs to be investigated in detail.

The nonactions in these examples have consequences with an affective, mostly negative, value. Importantly, previous research suggests that these affective components of effects are also involved in action control (Beckers, Houwer, & Eelen, 2002; Eder et al., 2015; Eder, Pfister, Dignath, & Hommel, 2017). Future research should therefore place an emphasis on the involvement of affective components in nonaction control. What is certain is that the above examples of nonactions show that not acting has specific and at times severe effects, highlighting the power that a nonaction can hold.

5.4 Concluding remarks

Pursuing goals and changing the world around us to reach these goals is an essential part of our human self. Generally, we assume that we have to act to reach our goals. However, sometimes not acting can even be more constructive. The present experiments showed that even though nonactions lack a specific motor pattern, they still share essential characteristics with actions. Thus, it seems that nonactions are represented in terms of the effects they produce, and a sense of agency for these nonaction effects can emerge. These findings demonstrate that behavior that looks like inactivity from the outside can consist of complex processes and can even influence subsequent overt responses. The absence of an action therefore does not allow the simple conclusion that people are doing nothing.

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