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Intentional Binding is independent of the validity of the action effect's identity

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ABSTRACT

When an action produces an effect, both events are perceived to be shifted in time toward each other. This shift is called Intentional Binding (IB) effect. First evidence shows that this shift does not depend on the statistical predictability of the produced effect's identity (Desantis, Hughes, & Waszak, 2012). We confirm this result by comparing the perceived duration of action–effect intervals before valid and invalid action effects using the method of constant stimuli. The perceived duration of action–effect intervals did not differ for valid and invalid effects. This result was true for different durations of the action–effect interval (Experiments 1–4: 250 ms, Experiments 1 & 2: 400 ms), different effect modalities (Experiments 1 & 3: visual, Experiments 2–4: auditive), and two types of validity variations (Experiments 1 & 2: 80% valid, Experiments 3 & 4: 100% valid vs. random). We validated our results by using a clock paradigm and a numerical duration estimation task (Experiment 4). We conclude that the IB effect is not the result of internal prediction due to action–effect bindings, but might rely on higher-order processes.

1. Introduction

When an action reliably produces an effect, the action and the effect are perceived closer together in time than they actually are. This phenomenon has first been demonstrated in a study by Haggard, Aschersleben, Gehrke, and Prinz (2002) in which participants pressed a key that produced a tone. Participants had to judge the time of the action or the time of the effect in relation to the position of a rotating clock hand. Results showed that a key press paired with an effect is perceived later than an unpaired key press and an effect is perceived earlier than an identical sensory stimulus presented without a preceding key press (see also Haggard & Clark, 2003; Haggard, Clark, & Kalogeras, 2002; Obhi, Planetta, & Scantlebury, 2009; Wohlschläger, Engbert, & Haggard, 2003; Wohlschläger, Haggard, Gesierich, & Prinz, 2003).

1.1. Intentional Binding effect

The bias to perceive actions and effects closer in time than they actually are has been linked to intentional action contexts and is called Intentional Binding (IB) effect. Several studies provide evidence that the bias in time perception is restricted to intentional actions. First, when participants do not freely choose to press a key but perform a TMS-induced key press, the IB effect vanishes and even reverses (Haggard, Clark, et al., 2002). More precisely, voluntary key presses followed by a tone as effect are perceived later and effect tones are perceived earlier than key presses or tones presented alone. In contrast, TMS-triggered key presses are perceived earlier and following tones later than key presses or tones presented in isolation. Second, when participants do not press keys themselves but passively experience tactile stimulation of the finger, passive presses by the key are perceived later in time than active presses (Wohlschläger, Engbert, et al., 2003). Third, not only self-executed actions but also observed actions before an effect are perceived later than the same action without an effect (Wohlschläger, Haggard, et al., 2003). However, this bias is not found when participants observe a key press that is performed by a rubber hand that does not act intentionally instead of a human agent. Thus, actions are only perceived later in time whenever an agent intentionally acts and this action produces an effect (Wohlschläger, Engbert, et al., 2003).

IB effects have been demonstrated in many studies mainly with two different methods. First, the aforementioned clock paradigm showed that an action is perceived later and an effect is perceived earlier, thus providing indirect evidence that the action–effect interval is perceived shorter (Haggard & Clark, 2003; Haggard, Clark, et al., 2002; Haggard & Cole, 2007; Moore & Haggard, 2008; Moore, Lagnado, Deal, & Haggard, 2009; Obhi, Planetta, & Scantlebury, 2009; Wenke, Waszak, & Haggard, 2009; Wohlschläger, Engbert, et al., 2003; Wohlschläger, Haggard, et al., 2003). Second, verbal

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numerical duration estimates showed that participants perceived the duration of the action–effect interval shorter than the same interval between a tactile stimulation of the finger by the key and a tone (Cravo, Claessens, & Baldo, 2009; Engbert, Wohlschläger, & Haggard, 2008; Engbert, Wohlschläger, Thomas, & Haggard, 2007; Humphreys & Buehner, 2009). This method requires participants to estimate how long the interval between action and effect had lasted in milliseconds and thus directly assesses the duration of the action–effect interval. To sum up, a number of studies have shown that the IB effect both affects estimates of the time of actions or effects and influences the perceived duration of an action–effect interval in a comparable way.

1.2. Ideomotor Principle as mechanism underlying the IB effect

Yet, the mechanisms underlying the IB effect are still at debate. The effect occurs for self-executed as well as for observed action–effect episodes, that is, for intentional actions. In contrast, IB does not occur for passive actions and for observations of a rubber hand. In line with these observations, Engbert et al. (2007) suggested that IB occurs in the presence of ideomotor actions, that is, when an action is executed because the agent aims to produce an intended effect. In the current study, we wanted to investigate whether this claim holds true and IB indeed relies on acquired action–effect associations according to the Ideomotor Principle.

The Ideomotor Principle assumes that intentional behavior is based on bidirectional associations between actions and validly following effects (Herbart, 1825; James, 1890; Lotze, 1852; for more recent formulations see Greenwald, 1970; Hoffmann, 1993, 2003; Hoffmann, Berner, Butz, Herbort, Kunde, & Lenhard, 2007; Hommel, 1998; Hommel, Alonso, & Fuentes, 2003; Hommel, Müsseler, Aschersleben, & Prinz, 2001; Prinz, 1987, 1997). Whenever a motor behavior repeatedly results in the same effect people acquire action–effect associations. For example, when participants repeatedly press a key and a certain tone validly follows the key press, the participants acquire the key–tone association. Second, when people later intend to produce an effect the anticipation of the acquired effect automatically triggers the motor behavior, i.e. the action that usually brings about this effect. So, when in the aforementioned example participants intend to produce a tone stimulus, the representation or anticipation of this tone stimulus triggers the associated key press (for empirical evidence see e.g. Elsner & Hommel, 2001; Herwig & Waszak, 2009; Janczyk, Skirde, Weigelt, & Kunde, 2009; Kiesel & Hoffmann, 2004; Koch & Kunde, 2002; Kunde, 2001; Pfister, Kiesel, & Hoffmann, 2011; Pfister, Kiesel, & Melcher, 2010; Ziessler & Nattkemper, 2002).

If action–effect associations determined the IB effect, IB effects should be restricted to or should at least be stronger in conditions with stable, predictable action effects. Thus, IB effects should occur for valid action–effect associations that can be acquired by the participants. In line with this assumption, Haggard and Cole (2007, p. 212) speculated that the "'intentional binding effect' occurs for voluntary actions with predictable effects". If, however, IB effects occur to a comparable degree for predictable and unpredictable action effects, IB is probably no result of automatic effect prediction due to acquired action–effect associations but it relies on other processes that will be discussed in more detail in Section 6 (General discussion).

A first study on the mechanisms underlying IB effects has been conducted by Desantis, Hughes, and Waszak (2012) using a clock paradigm. In this study each of two possible actions (left or right key press) either validly predicted one of two possible action effects or resulted randomly in one of the two possible effects. IB effects did not differ for predictable and unpredictable action effects. In the current study, we also assessed time perception for predictable and unpredictable action effects as Desantis et al. (2012), but we tested IB with a psychophysical method of duration estimation instead of the clock paradigm. While in the clock paradigm participants' attention is directed to either the action or the effect, for duration estimates of the action–effect interval participants have to attend to both action and effect. We thus conjecture that applying a method of duration estimation directs attention to the action–effect episode as a whole (e.g. Engbert et al., 2008, 2007; Humphreys & Buehner, 2009) and thus facilitates the formation of action–effect associations. Additionally, the clock paradigm requires assessing time judgments for actions or effects in control conditions in which the action does not produce an effect or the effect stimulus is presented without a prior action that produces the effect. Because these control conditions might hinder or prevent participants to learn stable action–effect associations, a method of duration estimation might be more suitable for action–effect associations to evolve at all and it might be more sensitive to detect differences in time perception for predictable and unpredictable effects. Additionally, with a method of duration estimation we were able to test if the results are similar for action–effect intervals of 250 ms and 400 ms as methods of duration estimation have in contrast to the clock method (Haggard, Clark, et al., 2002) been shown to be more sensitive to IB effects of action–effect intervals greater than 250 ms (Humphreys & Buehner, 2009; Nolden, Haering, & Kiesel, 2012). As, in contrast to the clock method,no visual attention is needed for duration estimation tasks, we could also generalize the results to visual additionally to auditory effects.

2. Experiment 1

In analogy to the study of Desantis et al. (2012) we tested whether IB effects are restricted to valid, predictable action–effect mappings or similarly occur for invalid, unpredictable action–effect mappings. To vary the predictability of an action effect, we applied an action context with valid action effects that occurred predictably and with invalid action effects that occurred unpredictably. Participants chose to press a left or a right response key. Each response key was assigned to one specific effect. For example, in 80% of all trials the left key press resulted in a red square and the right key press in a blue square (valid action–effect condition). Yet, in 20% of all trials, the action–effect mapping was reversed. Then the left key press resulted in a blue square and the right key press resulted in a red square. For these 20% of trials, participants could not predict the action effect, and we termed this condition the invalid action–effect condition.

If the IB effect occurs only for valid and thus predictable action effects and not for invalid, unpredictable action effects, the IB effect would depend on the existence of long-term binding between action and effect. If, however, the IB effect occurs for valid as well as invalid effects, the IB effect would be related to the mere perception of agency, i.e., to the sense that the actor causes an effect (irrespective of an existing action–effect association according to the Ideomotor Principle).

To assess the IB effect in terms of the actually perceived duration of the action–effect interval, we used a psychophysical method, the method of constant stimuli. The action resulted in an effect after a specific delay (250 ms or 400 ms, varied between groups). Afterwards a comparison interval was presented and participants compared the duration of the comparison interval with the duration of the standard, the action–effect interval. We chose this method over the clock paradigm because we wanted that participants attended to the action–effect episode. And we decided against verbal numerical duration estimates because with that method no reference intervals are given. Thus, the results obtained with verbal numerical duration estimates can hardly be interpreted as actually representing the absolute duration of the action–effect interval or the duration of perceived temporal differences between conditions (Engbert et al., 2008).

2.1. Method

2.1.1. Participants

32 participants (17 female, age range 18–40 years) took part in exchange for course credits or 6 Euros. 29 participants were right-

handed, and three were left-handed. Four additional participants were excluded from analyses and replaced because the number of freely chosen left and right key presses differed according to a chi-square-test ($p < .05$). One further participant was replaced as the range of the probabilities to judge the different comparison intervals as “longer” was lower than 50%.[1]

2.1.2. Apparatus & stimuli

The experiment was run on a standard PC with a 17 in. CRT screen (resolution 1024 $*$ 768 pixels, 100 Hz refresh rate). Stimulus presentation and data collection were accomplished with E-Prime (Schneider, Eschman, & Zuccolotto, 2002). For the free-choice responses two separate external response keys were fixed left of a standard keyboard. Participants pressed these two response keys with the index and middle fingers of the left hand. A red and a blue square (2.1 cm wide) presented centrally on a gray background served as action effects. Comparison intervals comprised a 440 Hz sine tone presented via VicFirth SIH-1 isolation headphones. Judgments were given with the right hand via the keys “1” and “2” on the number pad of the keyboard.

2.1.3. Procedure

At the beginning of each trial, participants freely chose one of the two response keys. Participants were asked to choose keys randomly in each trial. After an action–effect interval of either 250 ms or 400 ms an effect stimulus of the same duration was presented. Action–effect intervals varied between groups so that each participant experienced one constant action–effect interval and the same effect duration. Each key press produced a valid effect with a probability of 80%. For half of the participants, the left key produced the blue square in 80% of all trials and the red square in 20% of all trials while the right key produced the red square in 80% of trials and the blue square in 20% of trials. For the other half of participants, the assignment of red and blue squares to the left and right keys was reversed.

To judge the duration of the action–effect interval, we applied the method of constant stimuli (Gescheider, 1997; Lapid, Ulrich, & Rammsayer, 2008). The action–effect intervals of either 250 ms or 400 ms served as standard intervals. Participants were instructed to compare the action–effect interval with the duration of a tone stimulus that appeared 500 ms after the offset of the effect. The comparison duration of the tone stimulus was either 50 ms, 100 ms, 150 ms, 200 ms, 250 ms, 300 ms, 350 ms, 400 ms, or 450 ms for participants with an action–effect interval of 250 ms or 200 ms, 250 ms, 300 ms, 350 ms, 400 ms, 450 ms, 500 ms, 550 ms, or 600 ms for participants with an action–effect interval of 400 ms. After presentation of the comparison tone, participants were asked on screen if the tone had lasted shorter (option 1) or longer (option 2) than the action–effect interval. Participants made their judgment by typing the number of the option using the number pad of the keyboard. Then the screen turned blank and the participant could start the next trial by pressing either the left or right key whenever he/she felt ready to do so. Response times for the free choice actions were measured from the onset of the judgment response to the onset of the free choice action. When more than one key press was recorded before an effect occurred the error message “Bitte nicht zweimal drücken!” (German for “Please do not press twice”) appeared and the next trial started. Each of the 9 comparison intervals was paired 60 times with the valid action effect and 15 times with the invalid action effect. Altogether there were 675 trials divided into 5 blocks. In addition, participants accomplished 9 practice trials at the beginning of the experiment.

2.1.4. Data analysis

We excluded data of participants when the number of left and right key presses differed according to a chi-square-test. This was necessary to ensure that both effect stimuli were presented with similar frequencies as unequal frequencies of stimuli influence perceived stimulus durations (Ulrich, Nitschke, & Rammsayer, 2006) and we wanted to avoid a possible impact of the effect frequencies on the perception of the action–effect interval.

Additionally, we excluded data of participants when the proportion of “longer” judgments varied less than 50% across all comparison durations. A lower variation in judgments would indicate that the participant was either not able or willing to discriminate between the different interval durations.

All trials in which the participants pressed the key more than once were discarded (2.6%). We computed the proportion of “longer” judgments separately for each participant and condition (valid or invalid action effect) for each duration of the comparison tone. Proportion data per participant and condition were fitted to a logistic function using the psignifit Toolbox (Wichmann & Hill, 2001) for MATLAB (for an exemplary plot of raw proportion data and a fitted function see Fig. 1). From the fitted function we estimated the .5 percentile, i.e. the point of subjective equality (PSE). The PSE is the estimated duration of the comparison stimulus at which the participant cannot discriminate between the standard and the comparison duration. Thus, the PSE serves as a measure of how long the comparison interval would have to last to be judged as equally long as the standard interval.

2.2. Results

2.2.1. Manipulation check

To check whether participants learned the action–effect association, we compared the response times depending on whether the previous trial had contained a valid or an invalid action effect. We predicted that participants would respond slower after invalid action effects if action–effect learning had taken place because a recent study demonstrated that an invalid effect is perceived similar to negative feedback and causes the next response to be slower (Band, van Steenbergen, Ridderinkhof, Falkenstein, & Hommel, 2009). Because there was no time limit for the free choice actions we excluded response times above 2000 ms since we expect the influence of the preceding trial to vanish over time. The ANOVA with the within-subject factor previous trial (trial $n - 1$ was valid or invalid) and the between-subjects factor action–effect interval revealed longer response times after invalid trials compared to valid trials (592 ms vs. 580 ms), $F(1, 30) = 5.05$, $p = .032$, $\eta_p^2 = .144$, independent of the action–effect interval, $F < 1$.

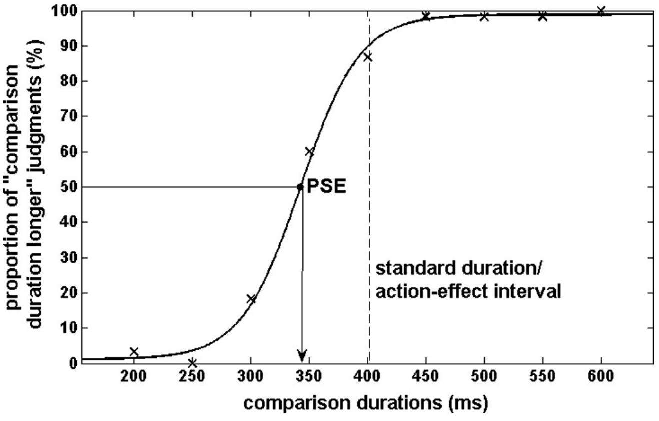

Fig. 1. Duration judgments of a prototypical participant in Experiment 1 in the valid condition with an action–effect interval of 400 ms. Crosses depict the actual proportion of “longer”-responses for the respective comparison duration, the line shows the logistic function fitted to the judgments and the point depicts the 50%-value of this function, the point of subjective equality (PSE).

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[1] We repeated all analyses including these 5 participants. These analyses yielded a similar result pattern and the same effects were significant.

Table 1
Results of Experiments 1–4. Estimated action–effect interval durations for valid and invalid/random action effects. All results are given in milliseconds. Numbers in brackets indicate the standard error.

Exp.	Method	Modality effect	Reference stimulus	Action–effect interval	Effect validity	
					Valid	Invalid/random
1	Constant stimuli	Visual	Auditory	250 ms	192.2 (12.4)	196.9 (12.1)
				400 ms	337.2 (12.4)	339.0 (12.1)
2	Constant stimuli	Auditory	Visual	250 ms	250.4 (13.3)	245.6 (13.6)
				400 ms	368.3 (13.3)	366.7 (13.6)
3	Constant stimuli	Visual	Auditory	250 ms	203.4 (11.8)	199.4 (12.3)
		Auditory	Visual	250 ms	262.2 (11.8)	253.9 (12.3)
4	Constant stimuli	Auditory	Visual	250 ms	209.8 (7.0)	215.3 (7.4)
	Numerical	Auditory		200 ms–300 ms	344.6 (21.5)	339.6 (21.5)
	Clock	Auditory		250 ms	143[a]	138[a]

[a] Please note that the clock paradigm does not yield interval estimates. Yet, for reasons of comparison we calculated the differences of perceived time of the effect and perceived time of the action in main blocks. The detailed results of the clock paradigm are presented in the main text and in Fig. 3.

2.2.2. Time perception of action–effect intervals

We computed an ANOVA with the within-subject factor action–effect mapping (valid, invalid) and the between-subjects factor action–effect interval (250 ms, 400 ms) on the PSEs (see Table 1 for the complete data pattern). There was no main effect of action–effect mapping, $F(1, 30) = 1.89$, $p = .18$, $\eta_p^2 = .059$, and action–effect mapping did not interact with interval, $F(1, 30) = 0.36$, $p = .55$, $\eta_p^2 = .012$. The interval of 250 ms was perceived 145 ms shorter than the interval of 400 ms, $F(1, 30) = 69.87$, $p < .001$, $\eta_p^2 = .70$.

To assess whether action–effect intervals were perceived shorter than they actually were, we computed additional t tests. Valid and invalid action–effect intervals were perceived shorter than they actually were with 192 ms and 197 ms for the 250 ms action–effect interval, $t(15) = -4.40$, $p < .001$, and $t(15) = -4.28$, $p < .001$, and with 337 ms and 339 ms for the 400 ms action–effect interval, $t(15) = -5.36$, $p < .001$, and $t(15) = -5.18$, $p < .001$.

2.3. Discussion

The perceived duration of the action–effect intervals was independent of the action–effect mapping. Thus, the perceived duration of the interval between action and effect is similar for predictable and unpredictable action effects.[2]

When comparing the perceived durations of the action–effect intervals with their actual durations, the size of the IB effect seems to be reduced regarding the observed IB effects of previous studies. Indeed the 250 ms interval was perceived 58 ms shorter and the 400 ms interval was perceived 63 ms shorter than it actually was. Studies applying the clock method observed IB effects (the sum of the shift of action and effect toward each other compared to baseline conditions) of 61 ms (Haggard, Clark, et al., 2002), of ca. 105 ms (Haggard & Clark, 2003; Wenke et al., 2009) or of 118 ms (Haggard & Cole, 2007).

However, one has to be cautious in interpreting the actual differences because of the applied method. First, when applying the clock paradigm, it is not liable to interpret the shifts of points in time directly as perceived duration. The perceived times of actions and effects have to be measured in different trials with the clock paradigm (Haggard & Cole, 2007), which impedes an interpretation of the perceived time as reflecting one action–effect interval. Second, in our study participants compared an empty action–effect interval with a filled tone interval. Yet, several studies have shown that duration estimations are susceptible by stimulus modality. Unfilled intervals are perceived shorter than filled ones (cf. Rammsayer & Lima, 1991; Wearden, Norton, Martin, & Montford-Bebb, 2007), which could foster that the action–effect interval, an unfilled interval, is perceived shorter than the comparison tone, i.e. a filled interval. Thus, we conjecture that our method might be unsuitable to assess the actual effect size of the IB effect. Importantly, however, the time judgments for valid and invalid effects are subject to the same bias. Thus, the method is suitable to assess that the action–effect intervals were perceived similar for both types of effects.

Experiment 1 reveals that the IB effect seems to occur to a similar degree for valid and invalid effects. Yet, before drawing conclusions on this finding, we aimed to rule out alternative explanations. Despite the fact that the analysis of response times for free choices (which was not planned a-priori) suggests that actually participants had learned the valid action–effect association, one may question whether the acquired action–effect associations in Experiment 1 were sufficiently strong to impact on IB effects because the effect followed an action only in 80% of all trials. A recent study by Band et al. (2009) demonstrated action–effect learning in a setting in which an action produced an effect in 80% of trials and another effect in 20% of trials. Nevertheless, in Experiment 2 we aimed to optimize conditions for action–effect learning.

3. Experiment 2

We introduced a learning phase during which the effects followed the actions with a probability of 100%. During learning trials no comparison stimuli were presented and no time judgments had to be given in order to assure that participants were not distracted by the duration estimation task while acquiring the action–effect association.

In addition, as existing studies on IB usually use auditory rather than visual action effects we now used auditory effects. In contrast to visual stimuli tones are automatically attended to (Posner, 1978). Thus, by using tones we also ruled out the possibility that effect stimuli were not attended to and that this prevented validity of the action–effect mapping to influence duration judgments. To further ensure that participants actually attended to the identity of action effects, participants had to indicate in some trials throughout the experiment after the duration judgment whether the high or the low tone had been presented. To avoid any overlap between effect stimulus and comparison stimulus, we used a visual stimulus of variable duration as comparison stimulus.

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[2] To confirm that IB effects are measurable with the method of constant stimuli we ran a control experiment comparing active and passive key presses (Haggard, Clark, et al., 2002). The active condition resembles our experimental conditions in Experiments 1–4. Specifically, participants freely chose between the left and right keys. Each key press produced a 100% key-specific visual effect (red or blue square) after 250 ms. In the passive condition participants did not choose freely, but the fingers were pulled by a motor forcing the participant to press the left or right key. Like in the active condition each key press produced the same key-specific visual effect after 250 ms. In both conditions the interval between key press and square had to be compared to a tone of varying duration. As predicted the 250 ms interval was perceived shorter when participants freely chose to press the keys than when they were forced to do so (212 ms vs. 229 ms), $t(15) = -2.53$, $p = .023$. Thus we conclude that the method of constant stimuli is suitable to detect IB effects.

3.1. Method

3.1.1. Participants

44 participants (37 female, age range 19 to 44 years) took part for course credits or 6 Euros. 40 participants were right-handed, and four were left-handed. Twelve additional participants were excluded from analyses and replaced because the number of freely chosen left and right key presses differed according to a chi-square-test ($p < .05$). Ten further participants were replaced as the range of the probability to judge the comparison interval as "longer" was lower than 50%.[3]

3.1.2. Apparatus & stimuli

Apparatus and stimuli were the same as in Experiment 1 except for the following changes. First, we changed the modality of effect stimuli and comparison stimuli. Two sine tones of 400 Hz and 800 Hz served as action effects. The comparison stimulus was a blue square (2.1 cm wide) presented centrally. Second, the effect was presented for 400 ms in both groups with action–effect intervals of 250 ms and 400 ms. Third, only one key press per trial was recorded and thus no error message concerning double key presses occurred.

3.1.3. Procedure

The procedure was the same as in Experiment 1 except for the indicated changes. First, we introduced a learning phase of 40 trials without any time judgment task and with a 100% valid assignment of actions to effects. Throughout the remaining experiment the valid effects were presented in 80% of trials, and the invalid effects in 20% of trials. Second, to ensure that the effect's identity was attended to we asked participants to press a key according to the effect's identity in ca. 18% of all trials after they had given the duration judgment.

3.1.4. Data analysis

Data analysis was equivalent to Experiment 1.

3.2. Results

3.2.1. Manipulation check

To check whether participants learned the action–effect association, we compared the response times depending on whether there was a valid or an invalid action effect in the previous trial. The ANOVA with the within-subject factor previous trial (trial n − 1 was valid or invalid) and the between-subjects factor action–effect interval showed that response times were not significantly slower after invalid compared to valid trials, $F(1, 42) = 1.79$, $p = .187$, $\eta_p^2 = .041$, and the interaction between interval and the validity of the previous trial only approached significance, $F(1, 42) = 3.31$, $p = .076$, $\eta_p^2 = .073$. To ensure that this non-significant interaction did at least numerically fit the expectation of longer response times after invalid trials, we compared for each interval the response times after valid and invalid trials with t tests. For the 250 ms action–effect intervals response times did not differ (588 ms vs. 586 ms after valid and invalid trials), $t(21) = 0.38$, $p = 710$, while for the 400 ms action–effect intervals response times tended to be longer after invalid effects compared to valid effects (696 ms vs. 683 ms), $t(21) = -2.05$, $p = .053$.

3.2.2. Time perception of action–effect intervals

To check for differences in the perceived duration of action–effect intervals, we computed an ANOVA with the within-subject factor action–effect mapping (valid, invalid) and the between-subjects factor action–effect interval (250 ms, 400 ms) on the PSEs (see Table 1 for the complete data pattern). There was no main effect for action–effect mapping, $F(1, 42) = 0.78$, $p = .383$, $\eta_p^2 = .018$, and action–effect mapping did not interact with action–effect interval, $F(1, 42) = 0.19$, $p = .663$, $\eta_p^2 = .005$. The standard interval of 250 ms was perceived 112 ms shorter than the standard interval of 400 ms, $F(1, 42) = 41.06$, $p < .001$, $\eta_p^2 = .494$.

To assess whether the action–effect intervals were perceived shorter than they actually were, we computed additional t tests. For valid and invalid effects, the action–effect intervals of 250 ms were perceived to last 250 ms and 246 ms, respectively. So, action–effect intervals were not perceived differently from their actual duration, $t(21) = 0.03$, $p = .98$, and $t(21) = -0.32$, $p = .75$. The 400 ms interval was perceived to last 368 ms for valid effects and 367 ms for invalid effects and thus it was perceived shorter than it actually was, $t(21) = -2.53$, $p = .02$ and $t(21) = -2.47$, $p = .02$.

3.3. Discussion

Experiment 2 confirms the results of Experiment 1. The perceived duration of action–effect intervals before valid effects is independent of a valid or invalid action–effect mapping. This adds further evidence to the hypothesis that for the occurrence of the IB effect no existing long-term relation between action and effect is necessary. Instead the IB effect arises in the presence of invalid action effects as well, suggesting that short-term action–effect bindings induce the IB effect.

Further, in Experiment 2 we observed shorter estimates than the actual duration only for the 400 ms action–effect interval that was perceived 38 ms shorter than it actually was. In contrast, the 250 ms interval was not perceived shorter than it actually was. We conjecture that the type of comparison, i.e. comparing an empty interval prior to a tone with the duration of a visual stimulus might not be suitable to assess the actual size of the IB effect because of a bias to judge filled intervals as longer than unfilled ones (Rammsayer & Lima, 1991). Further, the choice of the modality of the comparison stimulus influences duration judgments. Visual stimuli are usually judged shorter than auditory stimuli (Goldstone & Lhamon, 1974). This might explain why action–effect intervals were perceived shorter when compared to tones (Experiment 1) than when compared to visual stimuli (Experiment 2). However, the perceived duration of the action–effect interval is biased in the same way for valid and invalid effects. Thus, the results of Experiment 2 clearly demonstrate that duration judgments of action–effect intervals do not depend on whether the effect of the action is predictable or unpredictable.

In Experiments 1 and 2, effects were assigned to actions with a proportion of 80% valid and 20% invalid effect trials. This might have impaired the formation of long-term action–effect bindings as the assignment of one action to an effect could vary trial by trial. Indeed, the manipulation check in Experiment 2 did not reveal overall significantly longer response times after invalid compared to valid effects. Only for the action–effect interval of 400 ms response times tended to be longer after invalid compared to valid action effects in the previous trial. Thus, at least for Experiment 2 we cannot be sure whether participants indeed acquired specific action–effect associations. To further foster the acquisition of stable action–effect associations, we decided to have 100% valid action–effect mappings in Experiments 3 and 4. We now varied the validity of actions and effects between blocks so that in half of the experiment the identity of the action effect was perfectly predictable by the preceding action while in the other half the preceding action did not predict which of the two possible action effects would occur (cf. Desantis et al., 2012).

4. Experiment 3

In Experiment 3, we varied in separate blocks whether action effects were mapped validly or randomly to actions. During one half of the experiment each action produced a valid action effect in 100% of the trials.

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[3] We repeated all analyses including these 22 participants. Contrary to the analyses reported in Section 3.2 (the Results section), the estimated PSEs in the group with action–effect intervals of 400 ms (for valid and invalid action effects) were numerically, but not significantly shorter than 400 ms ($ps > .35$). The remaining pattern of results did not differ from the pattern reported in Section 3.2 (the Results section).

During the other half of the experiment, each of the two effects followed each of the actions randomly, that is with a probability of 50%. If the IB effect depended on existing action–effect associations, action–effect intervals should be perceived shorter before valid and thus predictable rather than random and thus unpredictable action effects. If, however, the mere occurrence of an effect was sufficient for IB to evolve, perceived duration of action–effect intervals should not depend on the validity of action effects.

4.1. Method

4.1.1. Participants

48 students (41 female, age range 19 to 43 years) participated for course requirements. One participant was left-handed, and 47 were right-handed. Four additional participants were excluded from analysis and replaced because the number of freely chosen left and right key presses differed according to a chi-square-test ($p < .05$). Three further participants were replaced as the range of the probability to judge the comparison interval as “longer” was lower than 50%.[4]

4.1.2. Apparatus & stimuli

Apparatus and stimuli were the same as in Experiment 2 with the following exceptions. First, as Experiments 1 and 2 did not indicate any differences between the two action–effect intervals we applied only one action–effect interval lasting 250 ms for all participants. Second, participants were split into two groups either with visual stimuli as effects and tones as comparison stimuli (as in Experiment 1) or with tones as effects and visual comparison stimuli (as in Experiment 2). All effect stimuli were presented for 400 ms. Comparison stimuli were presented for the same durations as comparison stimuli used in the 250 ms condition in Experiments 1 and 2.

4.1.3. Procedure

The procedure was the same as in Experiment 1 with the following exceptions. During one half of the experiment the left key produced the low tone/the blue square and the right key produced the high tone/the red square with a validity of 100%. During the other half of the experiment each action produced each effect unpredictably in 50% of all trials (random condition). The order of experimental halves, that is, validity conditions was counterbalanced across participants. Participants were informed before each experimental half whether the actions validly produced the same effects or whether effect identity varied randomly. Each experimental half started with 20 practice trials followed by 5 of blocks of 36 trials. Altogether, each of the nine comparison intervals was presented 20 times in each condition.

4.1.4. Data analysis

Data analysis was performed in analogy to Experiment 1.

4.2. Results

We computed an ANOVA on the PSEs with the within-subject factor action–effect mapping (valid, random) and the between-subjects factor effect modality (visual, auditory), see Table 1 for the complete data pattern. We found no significant main effect of action–effect mapping, $F(1, 46) = 1.43$, $p = .239$, $\eta_p^2 = .030$. There was a main effect of modality revealing that action–effect intervals before visual effects compared to the duration of a tone were estimated shorter than intervals before tone effects compared to the duration of a visual comparison stimulus, $F(1, 46) = 12.14$, $p = .001$, $\eta_p^2 = .209$. Action–effect mapping did not interact with effect modality, $F(1, 46) = 0.18$, $p = .674$, $\eta_p^2 = .004$.

To assess whether the action–effect intervals were perceived shorter than they actually were, we computed additional t tests. For visual effects, action–effect intervals before valid (203 ms) and random effects (199 ms) were perceived shorter than 250 ms, $t(23) = -5.13$, $p < .001$, $t(23) = -5.33$, $p < .001$. For tone effects, action–effect intervals before valid (262 ms) and random effects (254 ms) were not perceived differently from their actual duration, $t(23) = 0.88$, $p = .39$, and $t(23) = .27$, $p = .79$.

4.3. Discussion

As in Experiments 1 and 2, the perceived duration of the action–effect interval was independent of action–effect mapping. Thus, IB effects do not depend on the validity of action effects.

Comparable to Experiment 1, the action–effect interval was judged shorter than it actually was when we used visual effects and the comparison stimulus was a tone. In contrast and comparable to Experiment 2, action–effect intervals of 250 ms were not judged shorter than they actually were when effects were tones and the comparison stimuli were visual stimuli. Thus, the obtained results were consistent across experiments. So far, the results reveal that IB effects do not depend on whether effects are validly mapped to the action or not. We obtained these results with a method that is commonly used for duration estimation (e.g., Lapid et al., 2008) and that seems suitable to find IB effects using active and passive key presses (Nolden et al., 2012, and see Footnote 2) but which is not yet well established in the investigation of the IB effect. Thus, in Experiment 4 we aimed to replicate the results of Experiments 1–3 and to compare results obtained with the method of constant stimuli to results of two methods that are better established to assess IB effects to verify that the absence of an influence of the validity of action–effect mapping in Experiments 1 to 3 does not depend on the applied method.

5. Experiment 4

In Experiment 4 we replicated the auditory effect condition of Experiment 3 and added two methods that are better established in IB research to validate our results, a clock paradigm to measure the perceived time of actions and effects and a numerical duration estimation task. Clock paradigms were used before in many studies to investigate the IB effect (cf. Desantis et al., 2012; Engbert & Wohlschläger, 2007; Haggard & Clark, 2003; Haggard, Clark, et al., 2002; Haggard & Cole, 2007; Obhi et al., 2009; Wohlschläger, Engbert, et al., 2003; Wohlschläger, Haggard, et al., 2003). Participants judged the time of their action or the time of a tone by indicating where a revolving clock hand had pointed to either at the time of the action or at the time of the tone. In baseline blocks, participants either performed an action or heard a tone. In main blocks, each action produced a tone as action effect and participants were informed before each block whether they should judge the time of the action or the time of the tone. In line with previous studies, we expected that actions were perceived later and effects were perceived earlier in main blocks compared to baseline blocks indicating IB effects.

Second, we applied a numerical duration estimation task (Cravo et al., 2009; Engbert et al., 2008, 2007; Humphreys & Buehner, 2009). Participants performed actions which produced tones as effects after action–effect intervals of 200 ms, 250 ms, or 300 ms. After each trial, participants estimated how many milliseconds the interval had lasted. For all applied methods, we expected that time judgments for valid and random action–effect mappings do not differ replicating the results of Experiments 1–3.

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[4] We repeated all analyses including all participants. In contrast to the reported results, the ANOVA on PSEs showed marginally longer PSEs for action–effect intervals before valid compared to random action effects ($p = .08$). Compared to the actual duration of 250 ms, the interval before valid action effect tones was estimated to be marginally longer than 250 ms ($p = .08$). Note that this result points into the opposite direction than the hypothesis that action–effect intervals should be perceived shorter before valid compared to random action effects if IB effects were a result of long-term action–effect bindings.

5.1. Method

5.1.1. Participants

60 participants (42 female, mean age 25, age range 18 to 47 years) took part for 12 Euros or course credits. 55 were right-handed, and five were left-handed. Data of two further participants were excluded and replaced as the range of the probabilities to judge the comparison interval as “longer” was lower than 50%.[5]

5.1.2. Apparatus & stimuli

Apparatus and stimuli were the same as in Experiment 2 except for the following changes. The background of the screen was white throughout the experiment. The effect tones (sine tones of 400 Hz or 800 Hz) were presented for 100 ms. For the clock paradigm, participants watched a centrally presented clock face (diameter 4.8 cm) with 12 labeled ‘minute’ intervals (see Fig. 2). The clock hand (1.7 cm) rotated with a period of 2560 ms.

5.1.3. Procedure

Experiment 4 was split into two sessions which were administered in counterbalanced order on different days within a week. At the beginning of each session participants were either informed that during the following session each key produces one of the effect tones 100% validly or that each key produces one of two effect tones randomly. In the following learning phase, there were 30 trials in which participants freely chose the key presses to produce predictable or random effect tones after an action–effect interval of 250 ms.

Three experimental parts followed in counterbalanced order across participants with the order being the same within a participant in the two sessions. Two methods, the method of constant stimuli and the numerical duration estimation task, were applied to measure the perceived duration of the action–effect interval. The third method, the clock paradigm, was used to measure the point in time when actions or effects occurred relative to a visually presented clock hand.

The method of constant stimuli was equivalent to the auditory effect condtion in Experiment 3. Additionally to the aforementioned general design changes the number of practice trials was reduced to 9 and the number of blocks was reduced to four blocks of 36 trials each per condition so that each of the nine comparison intervals was presented 16 times in each condition.

In the numerical duration estimation task, the action–effect interval lasted 200 ms, 250 ms, or 300 ms. Interval durations varied randomly. This trial by trial variation is necessary to prevent that participants give the same prepared judgment instead of estimating the interval duration each trial (Engbert et al., 2007). Participants typed the estimate how much time had passed between action and effect in milliseconds using the number pad of the keyboard with their right hand. Only estimates between 0 ms and 1000 ms were allowed and participants were reminded in the instruction that 1 s equals 1000 ms. Participants performed 3 blocks of 42 trials so that in total each action–effect interval was presented 42 times in each condition.

In the clock paradigm participants completed two baseline conditions in which only one event, freely chosen action or tone, was present, and two main conditions, in which each freely chosen action was followed by a tone after an action–effect interval of 250 ms. In the two main conditions participants were instructed before each block and additionally in each trial either to judge the time of the action or to judge the time of the tone. The clock hand started at a random position in each trial. In the action-only baseline and in both main conditions participants were asked to freely choose a key press. Participants were instructed to press the key only after the clock hand had at least revolved once, but not at a pre-planned time or clock-hand position. In the tone-only condition one of the tones that served as action effects throughout the remaining experiment was presented at a random time between 2560 ms and 5120 ms after the trial had started. The clock disappeared at a random time between 2 s and 3 s after the tone or, in the action-only baseline condition, between 2 s and 3 s after the action. After each trial participants were asked to judge the position of the clock hand at the moment of the key press or at tone onset in minutes (1–60) using the number pad of the keyboard with their right hand.

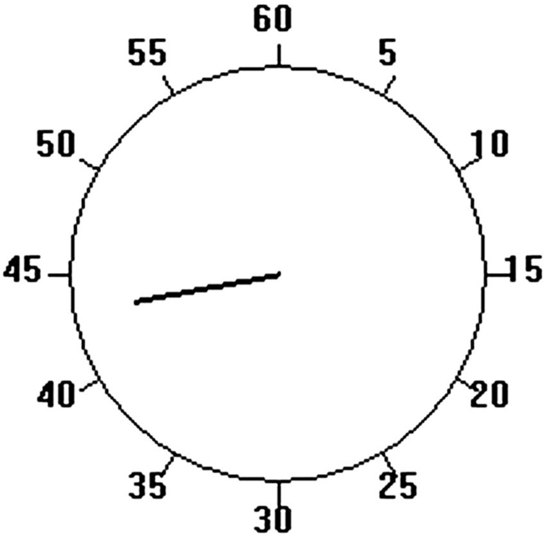

Fig. 2. Clock used in Experiment 4. Participants' task was to indicate where the clock hand had pointed to when an action was performed or when a tone was played.

In each session participants performed four main blocks and four baseline blocks of 20 trials each, resulting in 40 trials per condition (cf. Haggard & Cole, 2007). The first and the last two blocks were baseline blocks. The order of baseline conditions (action first or tone first) resembled the order of judgments in the main blocks for one participant within both sessions and was counterbalanced across participants. Participants practiced one trial of each condition before starting with the first baseline block.

5.1.4. Data analysis

Data analysis for the method of constant stimuli was equivalent to Experiment 3. However, because previous results were unaffected by whether left and right keys were chosen equally frequent, we did not test if left and right keys were chosen with equal frequency.

Estimates from the numerical duration estimation task were averaged per participant for each of the three action–effect intervals for valid and random effects. Estimates deviating more than +/− 2.5 SDs from a participant's mean in a condition were discarded (1.5%).

To analyze data gathered with the clock paradigm, we first calculated how many milliseconds the estimated and the actual positions of the clock hand differed in each trial. For each participant this difference was averaged for all combinations of the conditions judged event (action, tone) and type of block (baseline, main) separately for each session with valid and random action–effects. Estimates deviating more than +/− 2.5 SD from a participant's mean estimate in each condition were discarded (1.9%). We calculated per participant IB as the overall shift of the perceived time of actions and effects toward each other between main blocks and baseline blocks. That is, IB is indicated by a positive shift of the action and a negative shift of

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[5] All analyses reported in Section 5.2 (the Results section) were repeated including these two participants. These analyses yielded the same pattern of results as the reported analyses with one exception. The mean duration estimates derived from the method of constant stimuli revealed that estimates in the random condition were only marginally ($p < .10$), but not significantly shorter than 250 ms.

the effect in main blocks compared to baseline blocks as actions are perceived later and effects are perceived earlier in main blocks compared to baseline blocks when IB occurs. To gain a combined measure of IB for actions and effects we subtracted the shift of the tone from the shift of the action, which adds up both components to a numerically positive value.

5.2. Results

Averaged data per condition obtained for all three methods are shown in Table 1. To test for carryover effects between the three experimental tasks and the order of the valid-effects and the random-effects session and to reduce variance due to counterbalancing, we included the between-subjects factors "position of method" and "order of sessions" into all analyses.

5.2.1. Method of constant stimuli

We conducted an ANOVA with the within-subject factor action–effect mapping (valid, random) and the between-subjects factors order of sessions (valid first, random first) and position of constant-stimuli method (first, second, third). The perceived duration of the action–effect interval before valid and random action effects did not differ, $F(1,54) = 1.05$, $p = .311$. The action–effect interval was estimated to be shorter than its actual duration of 250 ms for valid effects (210 ms), $t(59) = -5.56$, $p < .001$, and random effects (215 ms), $t(59) = -4.66$, $p < .001$. Generally, participants who conducted the valid condition first perceived all action–effect intervals shorter than participants who conducted the random condition first, $F(1, 54) = 5.75$, $p = .02$, $\eta_p^2 = .096$. Importantly, the perceived duration for valid and invalid action–effect mapping did neither interact with the factor order of sessions, $F(1, 54) = 0.06$, $p = .804$, $\eta_p^2 = .001$, nor with the factor position of the method, $F(2, 54) = 1.98$, $p = .148$, $\eta_p^2 = .068$. No other main effect or interaction reached significance (all Fs < = 0.88).

5.2.2. Numerical duration estimation

We conducted an ANOVA with the within-subject factor action–effect mapping (valid, random) and the between-subjects factors order of sessions (valid first, random first) and position of numerical duration estimation (first, second, third). The perceived duration of the action–effect interval before valid and random action effects did not differ, $F(1, 54) = 0.13$, $p = .717$, $\eta_p^2 = .002$. The action–effect mapping did neither interact with the order of sessions, $F(1,54) = 1.43$, $p = .238$, $\eta_p^2 = .026$, nor with the position of the numerical time estimation method within each session, $F(2, 54) = 1.62$, $p = .208$, $\eta_p^2 = .056$. No other main effect or interaction reached significance (all Fs < = 1).

5.2.3. Clock paradigm

We performed an ANOVA with the within-subject factor action–effect mapping (valid, random) and the between-subjects factors order of sessions (valid first, random first) and position of numerical duration estimation (first, second, third) for the overall shift of the perceived time of action and effect toward each other (see Fig. 3). The analysis showed neither a difference in the mean shift of action and effect toward each other for valid (108 ms shift) or random (113 ms shift) action–effect mapping, $F(1,54) = 0.23$, $p = .632$, η_p^2 =.004, nor a two-way interaction of action–effect mapping with the order of sessions, $F(1,54) = 0.84$, $p = .363$, $\eta_p^2 = .015$, nor the position of the clock method, $F(2,54) = 1.03$, $p = .365$, $\eta_p^2 = .037$. No other main effect or interaction gained significance (all Fs < = 1.23).

As different manipulations might affect the perceived time of the action and the effect differently, we repeated the ANOVA with the within-subject factor action–effect mapping (valid, random) and the between-subjects factors order of sessions (valid first, random first) and position of numerical duration estimation (first, second, third) separately for the shift of perceived time of actions and perceived time of effects. The shift in the perceived time of actions between baseline and main blocks did not differ before valid or random effects (22 ms vs. 17 ms), $F(1, 54) = 0.18$, $p = .67$, $\eta_p^2 = .003$. The action–effect mapping did neither interact

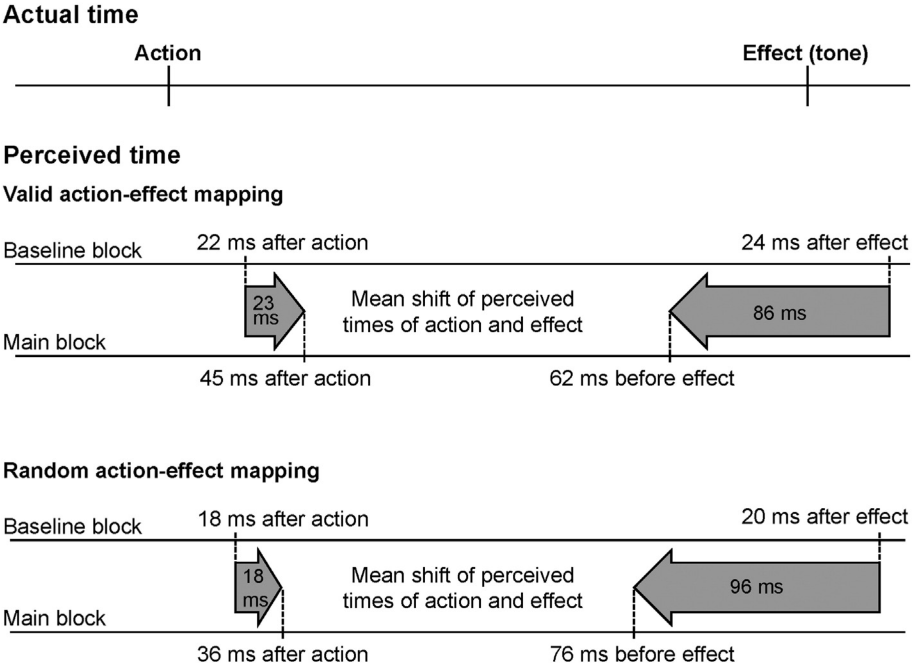

Fig. 3. Schematic overview of the results of the clock paradigm. The solid upright lines show actual points in time (upper timeline), and the dotted lines show perceived points in time in the baseline and main condition in the session with valid action–effect mapping (upper timelines) and in the session with random action–effect mapping (lower timelines). Arrows show the shift for actions and effects when comparing time perception in baseline and main blocks.

with the order of sessions, $F(1, 54) = 0.004$, $p = .95$, $\eta_p^2 < .001$, nor with the position of the clock paradigm, $F(2, 54) = 0.32$, $p = .73$, $\eta_p^2 = .012$. No other main effect or interaction reached significance (all $Fs < 1.40$).

The shift in the perceived time of effects between baseline and main blocks did not differ before valid or random effects either (−86 ms vs. −96 ms), $F(1,54) = 0.84$, $p = .365$, $\eta_p^2 = .015$. The action–effect mapping did neither interact with the order of sessions, $F(2, 54) = 0.92$, $p = .342$, $\eta_p^2 = .017$, nor with the position of the clock paradigm, $F(1, 54) = 1.23$, $p = .301$, $\eta_p^2 = .044$. No other main effect or interaction reached significance (all $Fs < 1.58$).

5.3. Discussion

Replicating the results of Experiments 1–3, Experiment 4 revealed no impact of the action–effect mapping on the perceived duration of the action–effect interval. This holds true for the method of constant stimuli as well as for the numerical duration estimation task. In addition, the clock paradigm shows the predicted IB effect. That is, actions are perceived later when paired with effect tones and effect tones are perceived earlier when paired with actions compared to temporal perception of actions or tones presented alone (Haggard & Cole, 2007; Lagnado, Haggard, & Moore, 2008; Moore & Haggard, 2008). Importantly, the IB effect is not modulated by the validity of the effects, irrespective of the method applied. Furthermore, the IB effect is not modulated by the order of the applied methods and independent of the order of the action–effect mapping, i.e. whether the participant experienced a valid or a random action–effect mapping first.

Results obtained with the numerical duration estimation task replicate results obtained with the method of constant stimuli in Experiments 1–4. Additionally the perceived points in time of valid or random actions and effects estimated with the clock method are in line with both our results gained with methods of duration estimation and with the findings of Desantis et al. (2012). This removes any doubts that the method of constant stimuli might not be suitable or sensitive enough to reveal measurable differences between valid and random action–effect mappings. So the lack to find a difference between the valid and the random action–effect mapping is not owing to the methods used.

6. General discussion

Our aim was to disentangle if the IB effect, that is the bias to perceive actions and effects closer in time than they actually are, depends on the existence of stable action–effect associations. In four experiments we observed that the perception of the action–effect interval is independent of the validity of the action–effect mapping. The action–effect interval was perceived similar for valid, predictable and for invalid or for random, unpredictable action effects. This result was shown using the method of constant stimuli and two established methods to measure the IB effect, namely the clock paradigm and the numerical duration estimation task. Thus, our results replicate and strengthen recent findings by Desantis et al. (2012) that IB is independent of the predictability of an action effect's identity and thus of stable action–effect associations.

6.1. Experiments 1 to 4 — power analyses

To further validate that the perceived duration of action–effect intervals is independent of whether the action effect is predicted or not by the action, we aimed to increase the power of the analyses. For this we reanalyzed the perceived duration of the action–effect interval gained with the method of constant stimuli including all participants from Experiments 1–4. We performed an ANOVA on the obtained PSEs with the within-subject factor action–effect mapping (valid, invalid[6]) and the between-subjects factor experimental group (250 ms visual effect, Experiment 1; 400 ms visual effect, Experiment 1; 250 ms auditive effect, Experiment 2; 400 ms auditive effect, Experiment 2; 250 ms visual effect, Experiment 3; 250 ms auditive effect, Experiment 3; 250 ms auditive effect, Experiment 4). This analysis yielded that the factor action–effect mapping had no impact on PSEs, $F(1, 177) = 0.13$, $p = .72$, $\eta_p^2 < .001$ (mean estimate before valid effects: 260.5 ms, before invalid effects: 259.5 ms). The interaction with experimental group, $F(6, 177) = 0.75$, $p = .61$, $\eta_p^2 = .025$, was not significant, either. Of course, experimental groups differed regarding the average perceived durations, $F(6, 177) = 33.71$, $p < .001$, $\eta_p^2 = .53$ (see Table 1).

In addition, we calculated the power to show an effect of at least the size f = .05 for all 184 participants (Faul, Erdfelder, Lang, & Buchner, 2007) based on our data. The power is 0.99 showing that we would have most probably found a difference between valid and invalid effects if such a difference existed.

All reported statistical analyses and the power analysis favor the null-hypothesis that perceived duration of action–effect intervals before valid and invalid effects does not differ. We further calculated a Bayes-Factor to gain a measure of the probability of the null hypothesis over the non-directional alternative hypothesis that any difference exists between the perception of action–effect intervals before valid and invalid effects. To do so we compared all PSEs for the valid and invalid action–effect mappings across all methods and participants, $t(183) = -0.0019$, $p = .99$. Assuming we expect a small effect, we calculated a scaled JZS-factor which was suggested for within-subject comparisons (for more detail see Rouder, Speckman, Sun, Morey, & Iverson, 2009) with a scale r = 0.2. The resulting JZS-factor suggests that the null hypothesis is 3.8 times more probable than the non-directional alternative hypothesis that a small difference exists between perceived durations in the valid and the invalid action–effect mappings. Thus, taken together, we conclude that the IB effect occurs to no different degree for valid and invalid action–effect mappings.

6.2. IB effect and action–effect learning

IB effects do not depend on the validity of the action–effect mapping indicating that acquired action–effect associations are not necessary to induce IB effects. This gives rise to speculations on the mechanisms behind and the functional relevance of the IB effect. The IB effect has been suggested to be closely related to the sense of agency (Ebert & Wegner, 2010; Haggard, 2005; Moore, Dickinson, & Fletcher, 2011; Moore, Wegner, & Haggard, 2009). These findings combined with our results suggest that sense of agency, i.e. the feeling to produce an action effect, does not require that the identity of the effect is automatically anticipated before the effect actually appears. So while knowing *that* the action will produce an effect changes IB (Moore & Haggard, 2008; Moore, Lagnado, et al., 2009), the prediction on *which specific effect* will occur does not matter. This is in line with studies that have suggested that the causal relation between an action and its effect and the IB effect is linked to each other (Buehner & Humphreys, 2009; Cravo, Claessens, & Baldo, 2011; Cravo et al., 2009; Eagleman et al., 2005; Haggard, 2005; Humphreys & Buehner, 2009).

At first sight, one may conjecture that our and Desantis et al.'s (2012) results contradict studies on the IB effect which varied the contingency *whether* an action produced an effect. It has been shown that the IB effect is larger when an action reliably produced an effect, that is in a highly contingent condition, compared to a condition in which an action rarely produced an effect, that is in a low contingent condition (Moore & Haggard, 2008; Moore, Lagnado, et al., 2009). Thus, the expectation whether the action produces any effect modulates the bias in time perception in action contexts. We conjecture that contingency manipulations, that is, varying the probability *whether* an action will cause an effect, alter the sense of agency. We, in contrast, did not vary the probability *whether* an action will cause an effect (actions produced

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[6] For brevity we subsume the conditions invalid (Experiments 1 & 2) and random (Experiments 3 and 4) action effect conditions under the term “invalid”.

effects in 100% of the trials), but the probability *which* effect the action will produce and this seems not to alter sense of agency.

Thus, our data in line with Desantis et al.'s (2012) results demonstrate that the sense to have caused an effect does not require the anticipation of a specific action effect that follows the action validly. Instead, to induce a sense of agency and thus to elicit IB effects the contingent production of any effect seems to be sufficient.

Yet, this does not mean that sense of agency is always independent on the specific action–effect relation. Indeed, Moore, Dickenson, and Fletcher (2011) recently reported that sense of agency was altered in an outcome blocking procedure (Moore et al., 2011). In an initial pre-training phase, participants learned that left and right key presses resulted in visual effects, red and pink squares, respectively. In a second compound-training phase, each action produced a visual effect and an auditive effect. Crucially, one of the actions was followed by a new visual effect (a black square), while the visual effect of the other action stayed the same as during pre-training. In a test phase, only the tones were presented as action effects and participants judged the perceived times of actions and effects. The test phase was subdivided in two halves and, unexpectedly by the authors, time judgments for the tones differed only in the second half of the test phase. In the second half of the test phase, there was a smaller IB effect for the tone that was paired with the expected visual effect in the compound training phase. The authors assume that the sense of agency was reduced for this tone, because for this tone the acquisition of an action–effect relation was blocked (for a detailed description of the rationale of outcome blocking see Flach, Osman, Dickinson, & Heyes, 2006). Participants expected the visual stimulus that resulted from the key press in the pre-training and the compound-training phase and therefore did not acquire a key press–tone association for the tone that was paired with the expected visual effect. Interestingly, the authors observed that their results depended on the amount of schizotypy of their participants. Time judgments for the two tones differed only for participants with low scores on schizotypy scales. Thus, this result implies that IB effects can be influenced by enduring changes in action–effect associations. Currently, we do not know why more “direct” manipulations for the acquisition of action–effect relations do not show any impact on IB effects, while rather “indirect” manipulation by means of a blocking protocol does. One might conjecture that the blocking procedure does not only change strength of the action–effect association but also impacts on higher order processes relating to causality judgments. Yet more research is needed to elucidate the differences of our study and the study of Desantis et al. (2012) on the one hand, and the study of Moore et al. (2011) on the other hand.

If IB does not rely on automatic internal prediction of the action effect based on long-term action–effect associations (see also Hughes, Desantis, & Waszak, 2013, for review), it remains an open question which processes cause the IB effect. We assume that IB relies on higher order processes that infer how likely an occurring stimulus was the effect of the preceding action. This can explain why IB is affected by the probability *whether* an action causes an effect (Moore & Haggard, 2008; Moore, Lagnado, et al., 2009), but not by the probability *which* effect is caused (see also Desantis, Hughes & Waszak, 2012). In the latter case there is, whichever stimulus occurs, no other possible cause for whichever effect present.

This inferential account is also in line with other results showing that IB is not in any case independent of the action effect's identity. For example, Moore, Wegner, and Haggard (2009) found stronger IB effects when the action has been preceded by a prime stimulus that is congruent rather than incongruent to the action effect. Similarly, Ebert and Wegner (2010) showed that IB effects were stronger when an action (a pull or push joystick movement) was congruent to the effect it produced (an object on the screen coming closer or moving away) than when action and effect movement were incongruent. In addition to IB effects, Ebert and Wegner assessed authorship ratings and, in line with the IB effects, participants felt more authorship for congruent rather than incongruent action effects. Thus, the identity of an action effect can impact on the amount of IB when participants infer more authorship based on the occurrence of a specific effect. Also in line with such an inferential account are results showing that participants judge effects they believe to be caused by themselves as earlier than effects believed to be caused by another person (Desantis, Roussel, & Waszak, 2011; Haering & Kiesel, 2012). This is in line with interpretations of IB as a mechanism that facilitates an action effect to be perceived as caused by the own action (Cravo et al., 2009; Eagleman & Holcombe, 2002; Greville & Buehner, 2010). As IB effects seem independent of the internal prediction of the effect's identity, we speculate that IB might rather serve learning of specific action–effect associations. Thus IB would support the flexibility of the nervous system to adapt to new action–effect associations instead of depending on the existence of such associations.

7. Conclusion

Taken together, we demonstrated in four experiments that IB effects are independent of the predictability of the effects' identity. This result was confirmed with different methods to assess IB, with different probability variations, for effects in visual and auditory modalities and for different action–effect intervals. Thus, IB effects, that is, the bias to perceive action and effect closer to each other, are not modulated by whether there are specific long-term action effect associations.IB is, in contrast, influenced by the effect's identity when the identity is “meaningful” in the task context as for example when effects are congruent to prior primes (Moore, Wegner, et al., 2009) or to the action (Ebert & Wegner, 2010) or when effects are believed to be the own rather than another person's effect (Desantis et al., 2011; Haering & Kiesel, 2012). We assume that IB effects are modulated due to inferential processes. Those processes might themselves foster the acquisition of long-term action–effect associations and thus support action control according to the Ideomotor Principle.

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