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Perceiving by proxy: Effect-based action control with unperceivable effects

Roland Pfister *,[1], Christina U. Pfeuffer [1], Wilfried Kunde

Julius-Maximilians-University of Würzburg, Department of Psychology III, Röntgenring 11, 97070 Würzburg, Germany

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ABSTRACT

Anticipations of future sensory events have the potential of priming motor actions that would typically cause these events. Such effect anticipations are generally assumed to rely on previous physical experiences of the contingency of own actions and their ensuing effects. Here we propose that merely imagined action effects may influence behaviour similarly as physically experienced action effects do. Three experiments in the response–effect compatibility paradigm show that the mere knowledge of action–effect contingencies is indeed sufficient to incorporate these effects into action control even if the effects are never experienced as causally linked to own actions. The experiments further highlight constraints for this mechanism which seems to be rather effortful and to depend on explicit intentions.

1. Introduction

1.1. Effect-based action control

How can we achieve what we want? Except in the land of milk and honey, we have to act in order to reach our goals; that is, we have to move our body. In order to understand how such movements are controlled, one needs to understand how potential goals are linked to the physical movements required for goal attainment. It would certainly be helpful if states that might become goals later on were directly linked to motor patterns reliably producing them. Perceiving or merely imagining an intended future state could then reactivate a motor pattern leading to its realization.

That is essentially what ideomotor theories of action control propose (Hommel, Müsseler, Aschersleben, & Prinz, 2001; Kunde, 2001; Shin, Proctor, & Capaldi, 2010). These theories even go one step further by assuming that motor patterns can only be controlled voluntarily through the mental recollection (anticipation) of the effects these motor patterns produce. Consequently, every motor action must be preceded by a recollection of the sensory effects of that action. Anticipated sensory consequences of own actions thus constitute a central aspect of human action control.

Evidence for this claim comes from studies using the *response–effect* (*R–E*) *compatibility paradigm* (e.g., Kunde, 2001; Pfister, Kiesel, & Melcher, 2010). In this paradigm, participants perform actions that produce contingent sensory effects; most importantly, employed actions and effects share certain features on a physical dimension (Kornblum, Hasbroucq, & Osman, 1990; Prinz, 1992, 1997). For instance, left vs. right actions might produce visual action effects to the left or right (Kunde, 2001; Pfister et al., 2010) or short vs. long key presses might trigger short vs. long effect tones (Kunde, 2003). In the R–E compatible condition, responses produce effects with corresponding features (e.g., short key press ▸ short effect

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* Corresponding author. Tel.: +49 931 31 81363.
E-mail address: roland.pfister@psychologie.uni-wuerzburg.de (R. Pfister).

[1] Authors contributed equally to the present manuscript.

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tone, long key press ▸ long effect tone), whereas in the R–E incompatible condition, responses produce effects with non-corresponding features (e.g., short key press ▸ long effect tone, long key press ▸ short effect tone). A consistent finding across numerous studies is that responses are faster in the R–E compatible condition than in the R–E incompatible condition (see also Badets, Koch, & Toussaint, 2013; Hubbard, Gazzaley, & Morsella, 2011; Janczyk, Pfister, & Kunde, 2012; Kunde, Pfister, & Janczyk, 2012; Rieger, 2007). Because action effects only appear after action execution, R–E compatibility effects are a straightforward measure of anticipative processes as assumed by ideomotor theory.

Before being able to exploit such effect anticipations, however, the agent clearly needs to acquire action–effect associations and current theoretical accounts widely agree that the corresponding R–E associations are built by experiencing the contingent pairing of specific actions and their respective outcomes either by oneself (e.g., Elsner & Hommel, 2001, 2004; Hoffmann, Lenhard, Sebald, & Pfister, 2009; Wolfensteller & Ruge, 2011) or through observational learning (Paulus, van Dam, Hunnius, Lindemann, & Bekkering, 2011). But is physical experience of this contingency indeed a necessary precondition to build up associations between actions and their ensuing effects? Here we propose that, even though prior experience is the most common mechanism for acquiring R–E associations, action effect associations may also be forged by knowledge of action–effect contingencies alone.

This speculation rests on two theoretical building blocks: First, human agents need to be able to build up sensory representations of events they do not actually experience themselves and, second, they need to be able to implement those representations into action control. Evidence for these two preconditions comes from two rather distinct fields of research as we describe in the following sections.

1.2. Representing non-perceived events: Imagery and empathy

Introspective experience shows that active imagery allows reliving past events quite vividly. And indeed, imagery does seem to draw on rather similar functions as actual perception (see Kosslyn, 1994, for an overview on classic theories and findings). For instance, imagining and perceiving an event seem to recruit similar mental processes (Borst & Kosslyn, 2008; Tlauka & McKenna, 1998) and they elicit neural activity in largely similar cerebral regions (e.g., Ganis, Thompson, & Kosslyn, 2004; Halpern & Zatorre, 1999; Kosslyn et al., 1993). Furthermore, imagery causes stronger neuronal responses the more vivid it is (Cui, Jeter, Yang, Montague, & Eagleman, 2007). These studies clearly suggest that merely imagined events are represented much like actually perceived ones.

A related line of research that documents sensory representations of non-perceived events is research on human empathy (Decety & Jackson, 2004; Preston & de Waal, 2002): Seeing or even imagining the state of another person inevitably elicits a representation of how this state feels for the other and this represenation motivates own behaviour. Accordingly, empathy is a “process which allows us to experience what it feels like for another person to experience a certain emotion or sensation (e.g., qualia)” (Singer, 2006, p. 856). This definition comprises both, affective components as in emotional contagion – corresponding to the use of empathy in folk psychology – as well as non-emotional sensory components. Moreover, empathy can be driven by merely anticipated future states (Batson, Early, & Salvarani, 1997; Royzman, Cassidy, & Baron, 2003) which can also include sensory experiences of other agents (Keysers et al., 2004; Schaefer, Xu, Flor, & Cohen, 2009).

Importantly, perception and imagination of another person's state seem to draw on the same mechanisms as actually experiencing this state oneself (Preston & de Waal, 2002). For example, perceiving disgusted faces automatically activates brain areas which would similarly respond to disgusting odours (Wicker et al., 2003). And what is true for emotional episodes also holds true for non-emotional sensory events: Observing someone else being touched seems to activate brain areas that are associated with the very feeling of being touched (Keysers et al., 2004). These findings suggest that human agents are able to spontaneously represent sensory experiences that they did not experience themselves. Such representations might also allow for effect-based action control if human agents are able to implement them into action control by mere intention.

1.3. Intentional control over automatic associations

Evidence for the power of intentions in forging automatic associations comes from recent studies on instruction-induced congruency effects (e.g., Cohen-Kdoshay & Meiran, 2007, 2009; Kunde, Kiesel, & Hoffmann, 2003; Liefooghe, Wenke, & De Houwer, 2012; Wenke, Gaschler, & Nattkemper, 2007). These studies indicated that usual interference effects such as flanker interference can arise even for stimuli that were simply mapped to a certain response by instruction without any actual experience. For instance, if participants are to classify bivalent stimuli according to one dimension (e.g., responding to the colour of coloured shapes), merely instructing an additional response mapping for the irrelevant dimension (e.g., shape) creates congruency effects even if the additional mapping has not been executed a single time (Wenke et al., 2007). Similarly, human agents seem to be able to counteract automatic processes by mere intentions to some degree by instantiating new intentions in terms of new task rules or specific plans (e.g., Adriaanse, Gollwitzer, de Ridder, de Wit, & Kroese, 2011; Waszak, Pfister, & Kiesel, 2013).

These findings suggest that intentions and knowledge alone have a considerable power to link representations of task-relevant events (in this case: stimuli) to motor responses. Similar processes might also take place for binding actions to their merely imagined effects, i.e., to forge bidirectional R–E associations without any physical experience of the action–effect contingency.

1.4. The present experiments

In three experiments, we investigated whether effect-based action control can exploit action effects that are merely known to result from an action but have never been experienced as a consequence of this action. We employed an R–E compatibility paradigm with short vs. long key press actions that triggered short vs. long tones (cf. Kunde, 2003). Yet, in contrast to previous experiments, the tones were not played back to the acting participant. Instead, the tones were played back via headphones to the experimenter (Experiments 1 and 2) or to an inanimate dummy (Experiment 3). Thus, even though the participants were familiarized with the potential action effects in a brief demonstration phase, they never experienced the tones as consequences of their own actions.

Based on previous findings on the co-representation of others' sensations (Keysers et al., 2004) and the ability to intentionally compile automatic associations (e.g., Kunde et al., 2003; Wenke et al., 2007), we hypothesized that the merely imagined (anticipated) action effects would still affect the corresponding actions. Such a pattern of results would expand theorizing on effect-based action control by showing that prior experiences of R–E contingencies may not be necessary for acquiring R–E associations. Rather, it would highlight a potential influence of intention and deliberate imagery as a means for compiling R–E associations. To anticipate the main results, we show that the mere knowledge of upcoming action effects may indeed be sufficient to incorporate these effects into action control. Furthermore, we show that the deliberate intention to represent the non-experienced action effect is necessary for it to affect motor actions.

2. Experiment 1

In Experiment 1, participants performed short vs. long key press actions and produced short vs. long tones. These tones were played back to the experimenter only, who sat behind a screen and wore headphones. The experimenter then gave verbal feedback about the tone duration (see Fig. 1 for a schematic of the design). At the beginning of the session, participants were explicitly instructed to imagine the tone effect the experimenter would be hearing.

In Experiment 1a, participants performed forced-choice reactions to colour stimuli throughout the session, whereas Experiment 1b also included free-choice trials in which the participants could choose which action to perform. This additional manipulation was motivated by studies suggesting that action effects might affect free-choice actions more strongly than forced-choice actions in some situations (Herwig, Prinz, & Waszak, 2007; Pfister, Kiesel, & Hoffmann, 2011; Pfister et al., 2010; Wolfensteller & Ruge, 2011). More precisely, free-choice actions seem to increase the tendency to anticipate action effects in the environment if the situation itself would not promote such anticipative processes, e.g., because R–E relations vary from trial to trial and are thus not particularly salient (Pfister et al., 2010). For more stable (blocked) relations, free response choices do not seem to promote R–E compatibility effects, however (Pfister & Kunde, 2013). Even though the present experiments employed such a block design, the use of merely imagined effects is likely to decrease participants' tendency to implement action effects into action control. We thus expected a more pronounced R–E compatibility effect for free-choice responses than for forced-choice responses.

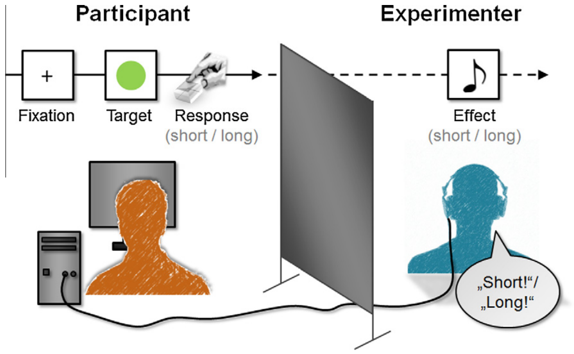

Fig. 1. Setup and trial procedure of Experiment 1. Participants responded to the colour of a target stimulus by pressing the spacebar either for a short (0–120 ms) or a long duration (121–300 ms). This response caused a short (80 ms) vs. long effect tone (240 ms) that only the experimenter was able to hear. The experimenter then gave verbal feedback about the tone effects. The trial procedure of Experiment 2 and 3 was identical except that there was no verbal feedback concerning tone identity; in Experiment 3, the tones were played back to a dummy instead of the experimenter.

2.1. Method

2.1.1. Participants

Sixteen participants (12 female, 1 left-handed, mean age = 26.1 years) were recruited for Experiment 1a and another 16 participants (11 female, 1 left-handed, mean age = 24.0 years) were recruited for Experiment 1b. We ensured that participants were naïve concerning the hypotheses by means of an open questionnaire at the end of the experiment. The data of two participants in Experiment 1a and of three participants in Experiment 1b were replaced because they correctly guessed the purpose of the study. Including these participants in the analyses did not change the overall pattern of results; the results are still reported for the naïve participants only to ensure an unbiased assessment of the effects.

2.1.2. Apparatus and stimuli

Participants were seated in front of a 17″ monitor and used the spacebar of a standard computer keyboard to respond. The experimenter sat behind a screen and wore headphones throughout the main experiment (Fig. 1). The headphones were soundproof to ensure that the participants themselves could not hear the tone and the headphones were clearly connected to the participant's computer.

Stimuli were red and green circles (45 mm in diameter) prompting short vs. long responses. Response durations of 0–120 ms between pressing and releasing the spacebar were counted as short, response durations of 121–300 ms were counted as long. The mapping of colours to required response durations was counterbalanced across participants. Experiment 1b additionally featured white target circles that prompted participants to choose freely between both alternatives. For these free-choice responses, participants were instructed to decide as spontaneously as possible and to avoid any specific strategy. They were also encouraged to aim for an equal distribution of short and long key presses but emphasis was placed on spontaneous choices rather than equal distributions.

Each key press triggered a sinusoidal tone (800 Hz) that was either short (80 ms) or long (240 ms). In the main experiment, this tone was only perceivable for the experimenter who gave verbal feedback by saying "kurz" (German for short) or "lang" (long) after hearing the tone and participants were explicitly instructed to imagine the tone the experimenter would be hearing.

2.1.3. Procedure

The experimental session started with 10 practice trials to familiarize the participants with the concept of "short" vs. "long" key presses (which did not yet produce any tone effects). After these practice trials, the experimenter introduced the future effect tones by playing the short tone and the long tone four times each. Crucially, the experimenter did not cause these tones by performing the short vs. long responses later on required, but by pressing a button of her choice on the keyboard without aiming for a specific duration. Accordingly, participants could not acquire the upcoming action–effect associations from model learning (Paulus et al., 2011). During this presentation, participants wore the headphones and handed the headphones to the experimenter afterwards.

After these initial demonstration trials, participants were instructed about the compatibility of their responses and the subsequent tone effects during the first half of the experiment, both orally and in writing. The experimenter ensured that subjects understood the task and had memorized the compatibility relation. Before the second half of the experiment, which employed a reversed compatibility relation, participants were instructed about the new R–E mapping and the experimenter ensured that they understood this change.

For the remainder of the session, the experimenter sat behind the screen and the participant continued with the task proper. Each trial started with a fixation cross (1000 ms) followed by the target stimulus (Experiment 1a: red vs. green circle; Experiment 1b: red vs. green vs. white circle) prompting a short or long response. The mapping of circle colour and instructed response side for forced-choice trials was counterbalanced across participants. Trials with responses prior to the target stimulus, wrong responses in forced-choice trials, and responses with a duration exceeding 300 ms were aborted and triggered an appropriate error message for 1000 ms. Correct responses triggered a short or long effect tone and the experimenter indicated which tone she had heard. The next trial started after an inter-trial interval of 1500 ms.

Participants completed 12 consecutive blocks for each R–E compatibility condition (compatible vs. incompatible) and condition order was counterbalanced across participants. The experimenter informed the participants about the changed compatibility relations after the first half of the experiment and participants were explicitly instructed to imagine the effect tones the experimenter would be hearing. In Experiment 1a, each block consisted of 8 short and 8 long responses (16 trials in total) whereas in Experiment 1b, each block additionally contained 8 free choice trials (24 trials in total).

After each block, participants received feedback concerning their mean response time (RT) and the number of errors in the preceding block and were instructed to try to respond even faster while also trying to make less mistakes. Furthermore, they were asked whether they had deliberately imagined the tone effects and whether they had experienced any difficulty with this task.

2.2. Results

2.2.1. Data treatment

The following analyses mainly focus on RTs, i.e., the time from target onset to pressing down the spacebar. Even though RTs were our main dependent variable of interest, we further analysed effective response durations (ERDs), i.e., the time between keypress onset and offset, and percentages error (PE; see Table 1 for detailed descriptive statistics for all three variables).

The first block of each condition was considered practice and did not enter the analyses. For the analyses of RTs and ERDs, we further excluded trials with responses exceeding the maximum duration of 300 ms (Exp. 1a: 1.9%, Exp. 1b: 2.5%), incorrect responses (Exp. 1a: 7.0%, Exp. 1b: 5.1%), and trials following such errors. Furthermore, we removed outliers, i.e., RTs deviating more than 2.5 standard deviations from the corresponding cell mean, calculated separately for each participant and condition (1.6% for both experiments). RTs, ERDs, and PEs of Experiment 1a were subjected to separate repeated-measures ANOVAs with the factors response duration (short vs. long) and tone duration (short vs. long). The corresponding ANOVA for Experiment 1b additionally used the factor task (forced-choice vs. free-choice).

2.2.2. Response times

Most importantly, the analysis of Experiment 1a revealed a significant interaction of response duration and effect duration, $F(1,15) = 6.88$, $p = .019$, $\eta_p^2 = .31$ (see Fig. 2, left panel). Accordingly, responses were faster when tone duration and response duration matched (short ▶ short, long ▶ long) as compared to the reverse combinations (short ▶ long, long ▶ short). Also, it took more time to initiate long responses as compared to short responses, $F(1,15) = 10.97$, $p = .005$, $\eta_p^2 = .42$, whereas the main effect of tone duration did not approach significance ($F < 1$).

The critical interaction of response duration and effect duration was also present for Experiment 1b, $F(1,15) = 11.32$, $p = .004$, $\eta_p^2 = .43$ (see Fig. 2, right panel), whereas the main effect of response duration showed a non-significant trend, $F(1,15) = 3.51$, $p = .081$, $\eta_p^2 = .19$. Furthermore, forced-choice actions were initiated faster than

Table 1
Response times (RTs), effective response durations (ERDs), and percentages error (PEs) for all three experiments as a function of the response (short vs. long) and the duration of the ensuing effect tone (short vs. long). Experiment 1a used forced choice responses throughout and the experimenter gave verbal feedback. Experiment 1b compared free and forced choice responses in this design. In Experiment 2a, we removed the verbal feedback but still instructed the participants to imagine their action effects whereas we did not use this instruction in Experiment 2b. Experiment 3 was equivalent to Experiment 2 but the experimenter was replaced by an inanimate dummy.

Experiment	RT Response				ERD Response				PE Response			
	Short Effect tone		Long Effect tone		Short Effect tone		Long Effect tone		Short Effect tone		Long Effect tone	
	Short	Long	Short	Long	Short	Long	Short	Long	Short	Long	Short	Long
1a	408	451	475	434	78	84	202	209	6.2	9.2	9.3	3.7
1b (forced)	421	462	466	431	75	76	213	218	5.4	7.3	10.4	7.9
1b (free)	459	505	509	475	82	82	210	210	–	–	–	–
2a	392	431	464	419	73	72	198	212	3.5	5.0	9.7	5.4
2b	352	357	388	376	82	81	212	212	7.3	6.1	4.9	5.3
3a	352	389	408	374	74	79	212	219	5.3	6.5	8.7	4.1
3b	328	324	350	355	71	71	206	206	4.1	4.9	6.2	7.9

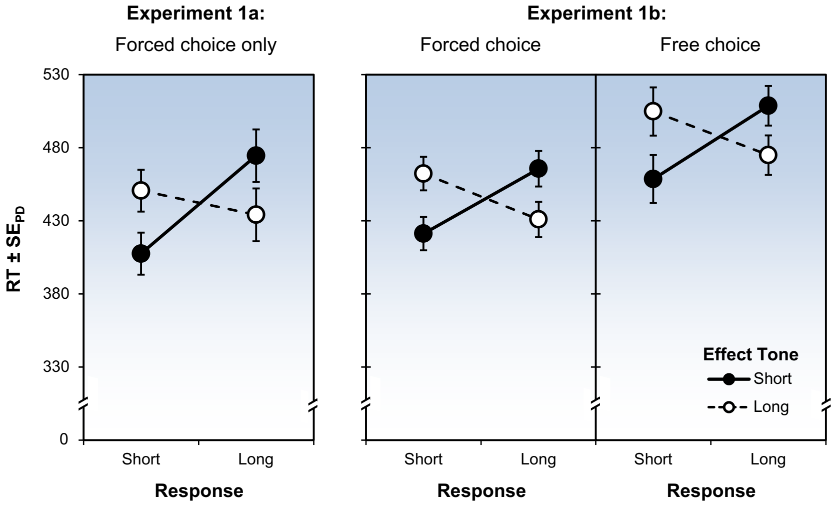

Fig. 2. Response times (RTs) for the four critical conditions in Experiment 1. Initiating a response was faster when this response would predictably produce an effect tone of corresponding duration. This was true even though the effect tone was only perceivable for the experimenter who gave verbal feedback. Standard errors of paired differences (SE_{PD}; Pfister & Janczyk, 2013) were computed separately for short and long responses.

free-choice actions as qualified by a significant main effect of the factor task, $F(1,15) = 18.55$, $p < .001$, $\eta_p^2 = .55$. None of the remaining main effects and interactions was significant, $ps > .282$, $\eta_p^2 < .08$.

2.2.3. Additional analyses

By design, ERDs depended mainly on the intended duration of a “short” vs. “long” keypress and accordingly, the analysis of ERDs in Experiment 1a showed a main effect of response duration, $F(1,15) = 283.88$, $p < .001$, $\eta_p^2 = .95$, but also a main effect of tone duration, $F(1,15) = 4.71$, $p = .047$, $\eta_p^2 = .24$, with longer ERDs for long effect tones than for short effect tones. The interaction term was not significant ($F < 1$). For Experiment 1b, the effect of response duration was also significant, $F(1,15) = 607.47$, $p < .001$, $\eta_p^2 = .98$, and this effect was more pronounced for forced-choice responses than for free-choice responses, $F(1,15) = 18.32$, $p < .001$, $\eta_p^2 = .55$. None of the remaining effects was significant, $ps > .210$, $\eta_p^2 < .11$.

For PEs, a similar analysis showed a significant interaction of response duration and tone duration in Experiment 1a, $F(1,15) = 17.29$, $p < .001$, $\eta_p^2 = .54$, with higher PEs if response duration and tone duration were incompatible rather than compatible (Table 1). Neither main effect approached significance, $ps > .328$, $\eta_p^2 < .07$. For Experiment 1b, we restricted the analysis to forced-choice trials, because errors obviously could not occur in the free-choice condition. This analysis yielded non-significant trends for both, the main effect of response duration, $F(1,15) = 4.03$, $p = .063$, $\eta_p^2 = .21$, and the interaction, $F(1,15) = 3.16$, $p = .096$, $\eta_p^2 = .17$, again with higher PEs if response duration and tone duration were incompatible as compared to R–E compatible mappings. The main effect of tone duration was not significant ($F < 1$).

2.3. Discussion

Experiment 1 investigated whether predictable consequences of own actions can be used for action control even when these consequences exclusively apply to a social partner and not to the acting agent. To this end, we employed an R–E compatibility paradigm with short vs. long key press actions that triggered short vs. long tones with either a compatible mapping or an incompatible mapping. Crucially, participants never experienced the consequences of their actions directly but only in terms of verbal feedback by the experimenter who heard the effect tones via headphones. The study's main question was whether the purely imagined effect tones would still give rise to R–E compatibility effects. The results confirmed that the action effects were included into action control: Initiating an action was faster and more accurate when it triggered compatible sensory effects for the experimenter as compared to incompatible effects. Merely imagining sensory action outcomes indeed seems to have a direct effect on action control just like experiencing self-induced action consequences (Kunde, 2001, 2003).

A second notable result of Experiment 1b is that the impact of imagined consequences was equally pronounced for free- and forced-choice actions. This finding is in line with recent reports suggesting the distinction of free and forced-choice actions to be mostly relevant for circumstances that do not give rise to reliable R–E compatibility effects themselves (Pfister & Kunde, 2013; Pfister et al., 2010). Obviously, the explicit instruction to imagine the effect tones was sufficient for the effects to be integrated into action control (see also Ansorge, 2002). We consequently used forced-choice actions only in the following experiments.

It should be noted, though, that there is no direct experimental control over the exact point in time participants imagined the sound effects to occur at. On closer inspection, this question entails two different aspects: First, the way that participants construed the situation, i.e., whether they indeed believed their responses to produce a certain effect tone for the experimenter, and, second, the point in time that features of the effect tone actually became represented in the course of a given trial. Regarding the first point, the present explicit instructions to imagine the sound effects as *consequences* of own actions should have ensured high internal validity of the experimental design. This conclusion is also in line with verbal reports of the participants during debriefing. The second point cannot be addressed in terms of the employed design, but the answer is evident in the results instead: Because R–E compatibility did have an impact on RTs and PEs, and because tone duration further influenced ERDs in a similar way as physically experienced effect tones (Kunde, 2003), it seems safe to conclude that the effect tones did indeed become represented prior to response initiation (following the logic of R–E compatibility studies in general). A possible alternative explanation for the results of Experiment 1, however, could be that the experimenter's feedback (the words "long" and "short") acted as a purely semantic action effect. Several studies suggest that R–E compatibility effects can indeed be observed for purely semantic relations between actions and following effects (Badets et al., 2013; Hubbard et al., 2011; Koch & Kunde, 2002). The observed influence could thus be based purely on semantic R–E compatibility without actually being based on the tone duration itself. This possible confound is addressed in Experiment 2.

3. Experiment 2

To address the possible confound of a semantic relation between actions (short vs. long key presses) and ensuing perceivable action effects (Koch & Kunde, 2002), the experimenter no longer provided tone-specific feedback after each trial. Instead, in Experiment 2a, the experimenter counted the number of committed errors (as indicated by a separate tone following error trials). At the end of each block, the experimenter informed the participants about the number of such errors to reinforce the participant's belief that the experimenter actually heard the tones.

Experiment 2b further addressed whether the explicit instruction to imagine the tones was critical for obtaining R–E compatibility effects in the current setting. This hypothesis is motivated by studies on cognitive perspective taking (Decety & Jackson, 2004) that employed somewhat similar designs as Experiment 1. More precisely, these studies showed that imagining a situation from the position of somebody else is a rather effortful process (Batson et al., 1997; Royzman et al., 2003). Similarly, maintaining a representation of the (imagined) effect might be expected to be just as effortful. This effort, in turn, would render participants unlikely to represent the arbitrary effects without being encouraged to do so. On the other hand, several studies suggested the co-representation of sensory states of others to occur automatically (Keysers et al., 2004; Schaefer et al., 2009), which could promote effect anticipations also in the current setup. We put these speculations to test by removing the instructions to imagine the tone effects in Experiment 2b. Here, the experimenter gave verbal feedback concerning the correctness of the response in each trial without referring to the tones that appeared in the headphones.

3.1. Method

Sixteen new participants (9 female, 1 left-handed, mean age = 23.0 years) were recruited for Experiment 2a and another sixteen new participants (14 female, 1 left-handed, mean age = 20.6 years) were recruited for Experiment 2b. For Experiment 2a, the data of five participants were replaced because they correctly guessed the purpose of the study; again, including these participants in the analyses did not change the overall pattern of results.

The general procedure was identical to Experiment 1a. Furthermore, for Experiment 2a, we introduced a rating (0–100) of the participant's ability to imagine the tone at the end of each block to increase participants' tendency to actually imagine the tones. Preliminary analyses of the rating data indicated that there was no correlation between participants' self-judged ability to imagine the tones and the corresponding R–E compatibility effects. To further support the manipulation of Experiment 2b, the experimenter was no longer covered by a room divider.

3.2. Results

3.2.1. Response times

As in Experiment 1, we excluded trials with responses that exceeded the maximum duration of 300 ms (Exp. 2a: 1.9%, Exp. 2b: 2.0%), wrong responses (Exp. 2a: 5.8%, Exp. 2b: 6.0%), outliers (Exp. 2a: 1.7%, Exp. 2b: 1.2%), and trials following errors. The remaining RTs were subjected to separate repeated-measures ANOVAs with the factors response duration (short vs. long) and tone duration (short vs. long).

The corresponding results for Experiment 2a are plotted in the left panel of Fig. 3, and the interaction was significant again, $F(1,15) = 14.07$, $p = .002$, $\eta_p^2 = .48$. Moreover, this analysis replicated the main effect of response duration, $F(1,15) = 8.64$, $p = .010$, $\eta_p^2 = .37$, and the non-significant main effect of tone duration ($F < 1$). We also compared the results of Experiment 2a to the data of Experiment 1a using a split-plot ANOVA with the within-subjects factors response duration and tone duration and the between-subjects factor experiment (1a vs. 2a). Crucially, this analysis did not show any interactions of experiment with any of the other factors ($Fs < 1$).

By contrast, the analysis of Experiment 2b (Fig. 3, right panel) only yielded a significant main effect of response duration, $F(1,15) = 10.29$, $p = .006$, $\eta_p^2 = .41$, whereas neither the main effect of tone duration, $F(1,15) = 1.54$, $p = .234$, $\eta_p^2 = .09$, nor the interaction ($F < 1$) were significant. Moreover, a direct comparison of Experiment 2a and Experiment 2b with a split-plot ANOVA showed the three-way interaction of response duration, tone duration and experiment to be significant, $F(1,30) = 5.76$, $p = .023$, $\eta_p^2 = .16$. Neither two-way interaction including the factor experiment approached significance ($Fs < 1$), whereas a non-significant trend for the main effect of experiment was driven by faster responses in Experiment 2b as compared to Experiment 2a, $F(1,30) = 3.75$, $p = .062$, $\eta_p^2 = .11$.

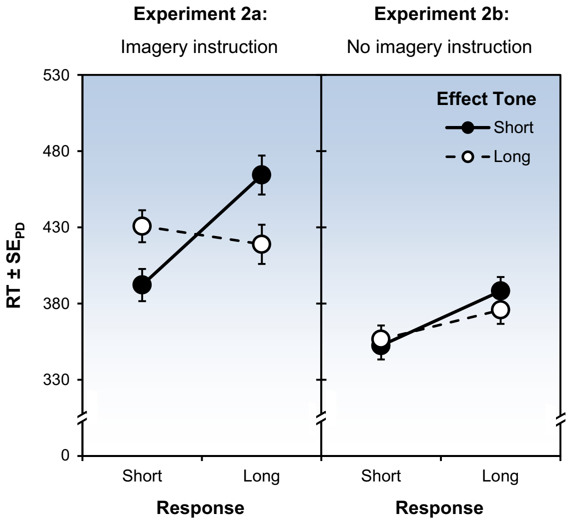

Fig. 3. Response times (RTs) for the four critical conditions in Experiment 2. When participants were instructed to imagine the tone effects (Exp. 2a), initiating a response was again faster when this response would produce an effect tone of corresponding duration, even though the experimenter did not give direct verbal feedback. Without explicit instructions (Exp. 2b), the impact of the effect tones was no longer present. Standard errors of paired differences (SE$_{PD}$) were computed separately for short and long responses.

3.2.2. Additional analyses

For ERDs, the analysis of Experiment 2a replicated the main effect of response duration, $F(1,15) = 451.19$, $p < .001$, $\eta_p^2 = .97$, and the main effect of tone duration, $F(1,15) = 10.21$, $p = .006$, $\eta_p^2 = .41$. Furthermore, a significant interaction emerged, indicating that tone duration did only affect long but not short responses, $F(1,15) = 8.93$, $p = .009$, $\eta_p^2 = .37$ (see Table 1). For Experiment 2b, only the main effect of response duration was significant, $F(1,15) = 443.64$, $p < .001$, $\eta_p^2 = .97$ (both other $Fs < 1$). A direct comparison of Experiment 2a and 2b yielded a significant interaction of effect duration and experiment, $F(1,30) = 443.64$, $p < .001$, $\eta_p^2 = .97$, as well as between response duration and tone duration, $F(1,30) = 4.64$, $p = .039$, $\eta_p^2 = .13$. The three-way interaction showed a non-significant trend, $F(1,30) = 3.62$, $p = .067$, $\eta_p^2 = .11$. Finally, overall ERDs were slightly shorter in Experiment 2a than in Experiment 2b, $F(1,30) = 4.46$, $p = .043$, $\eta_p^2 = .13$.

For PEs, the analysis of Experiment 2a again yielded a significant interaction of response duration and tone duration, $F(1,15) = 10.16$, $p = .006$, $\eta_p^2 = .40$, with lower PEs when response duration and tone duration corresponded than when they did not correspond (see Table 1). Furthermore, the main effect of response duration was significant, $F(1,15) = 11.16$, $p = .004$, $\eta_p^2 = .43$, and the main effect of tone duration showed a non-significant trend, $F(1,15) = 3.15$, $p = .096$, $\eta_p^2 = .17$. The corresponding analysis of Experiment 2b showed the main effect of response duration to be significant, $F(1,15) = 4.82$, $p = .044$, $\eta_p^2 = .24$, whereas the remaining effects were not, $ps > .260$, $\eta_p^2 < .09$. Accordingly, a direct comparison of the PEs in Experiment 2a and 2b yielded a significant three-way interaction, $F(1,30) = 10.78$, $p = .003$, $\eta_p^2 = .26$, as well as an interaction of response duration and experiment, $F(1,30) = 15.93$, $p < .001$, $\eta_p^2 = .35$.

3.3. Discussion

Experiment 2a did not use any tone-specific feedback to address potential alternative explanations in terms of semantic R–E compatibility (Koch & Kunde, 2002). Yet, participants were again faster when the duration of their response and the duration of the ensuing tone effects matched than when they did not match. Because the tones were only heard by the experimenter, participants were obviously imagining the consequences of their actions for the passive experimenter. This was only true, however, when participants were explicitly instructed to imagine the tone effects; without such instructions (Exp. 2b), the tone effects were no longer incorporated in action control. These findings stand in contrast to typical R–E compatibility effects that also emerge without any instructions relating to possible action effects (Kunde, 2001, 2003), indicating that the representation of unperceivable action effects is indeed rather effortful.

4. Experiment 3

Experiment 1 and 2 converge on the notion that human actors are able to represent unperceivable effects that their actions cause for social partners. The experiments do not address, however, whether a social setting is necessary to trigger such anticipations (for the impact of social cues on action coding, see Becchio, Sartori, & Castiello, 2010; Gonzalez, Studenka, Glazebrook, & Lyons, 2011; Pfister, Dolk, Prinz, & Kunde, in press; Ray & Welsh, 2011; Sato & Itakura, 2013). This was the main question of Experiment 3 in which we replaced the experimenter by an inanimate dummy that was placed behind the room divider. In Experiment 3a, participants were still explicitly instructed to imagine the effect tones whereas no such instructions were given in Experiment 3b. If a social function of own (unperceived) action effects is necessary to include them into action control, R–E compatibility effects should be reduced or even absent for both, Experiment 3a and 3b. As a consequence of using inanimate dummy partners, verbal error feedback could not be provided in Experiment 3a and 3b.

4.1. Method

Sixteen new participants were recruited for Experiment 3a (11 female, 4 left-handed, mean age = 22.5 years) and Experiment 3b (9 female, 3 left-handed, mean age = 21.2 years). The data of four participants in Experiment 3a and of three participants in Experiment 3b were replaced because they correctly guessed the purpose of the study; again, including these participants in the analyses did not change the overall pattern of results.

The design of Experiment 3a was identical to Experiment 2a with the only exception that the headphones were now worn by a dummy instead of the experimenter. Accordingly, no verbal feedback was given at all, but participants were explicitly instructed to imagine the effect tones and had to rate their ability to imagine the tones after each block. In Experiment 3b we again did not use any explicit instructions in terms of action effects and did not use the rating procedure at the end of each block.

4.2. Results

4.2.1. Response times

We excluded trials with responses exceeding the maximum duration of 300 ms (Exp. 3a: 1.9%, Exp. 3b: 1.7%), incorrect responses (Exp. 3a: 6.1%, Exp. 3b: 5.7%), trials following these errors, and outliers (1.4% for both experiments). The remaining RTs were subjected to separate repeated-measures ANOVAs with the factors response duration (short vs. long) and tone duration (short vs. long).

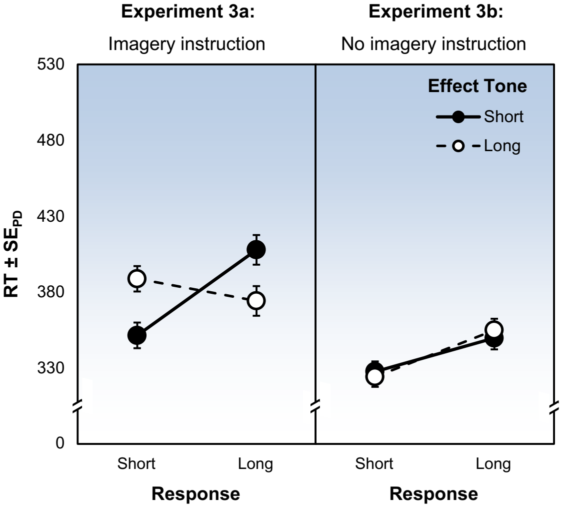

Fig. 4. Response times (RTs) for the four critical conditions in Experiment 3. In Experiment 3a, initiating a response was again faster when this response would predictably produce an effect tone of corresponding duration that was played back to an inanimate dummy. This influence disappeared when we no longer instructed the participants explicitly to imagine the effect tones (Experiment 3b). Standard errors of paired differences (SE_{PD}) were computed separately for short and long responses.

For Experiment 3a, this analysis fully replicated the pattern of Experiment 2a (Fig. 4, left panel). Accordingly, the main effect of response duration was significant, $F(1,15) = 8.13$, $p = .012$, $\eta_p^2 = .35$, whereas the main effect of tone duration was not ($F < 1$). These effects were again qualified by a significant interaction, $F(1,15) = 15.92$, $p < .001$, $\eta_p^2 = .51$. For Experiment 3b (Fig. 4, right panel), the main effect of response duration was also present, $F(1,15) = 10.78$, $p = .005$, $\eta_p^2 = .42$, whereas neither the main effect of tone duration nor the interaction approached significance (Fs < 1). A direct comparison of Experiment 3a and Experiment 3b with a split-plot ANOVA showed the three-way interaction of response duration, tone duration and experiment to be significant, $F(1,30) = 12.88$, $p = .001$, $\eta_p^2 = .30$. Neither two-way interaction including the factor experiment approached significance (Fs < 1), whereas a significant main effect of experiment was driven by faster responses in Experiment 3b as compared to Experiment 3a, $F(1,30) = 4.39$, $p = .045$, $\eta_p^2 = .13$.

4.2.2. Additional analyses

For ERDs, the analysis of Experiment 3a replicated the main effect of response duration, $F(1,15) = 1000.62$, $p < .001$, $\eta_p^2 = .99$, and the main effect of tone duration, $F(1,15) = 6.26$, $p = .024$, $\eta_p^2 = .29$. As in Experiment 2a, a significant interaction indicated that tone duration affected long responses more strongly than short responses, $F(1,15) = 0.29$, $p = .009$, $\eta_p^2 = .02$ (see Table 1). For Experiment 3b, only the main effect of response duration was significant, $F(1,15) = 575.40$, $p < .001$, $\eta_p^2 = .97$ (both other Fs < 1). A direct comparison of Experiment 3a and 3b again yielded a significant interaction of tone duration and experiment, $F(1,30) = 3.04$, $p = .092$, $\eta_p^2 = .09$, whereas neither the interaction of response duration and tone duration nor the three-way interaction were significant (Fs < 1). Overall ERDs did not differ between Experiment 3a and 3b, $F(1,30) = 2.61$, $p = .117$, $\eta_p^2 = .08$.

For PEs, the analysis of Experiment 3a again yielded a significant interaction of response duration and tone duration, $F(1,15) = 6.59$, $p = .021$, $\eta_p^2 = .31$ (see Table 1), with incompatible mappings yielding a higher PE than compatible mappings. The main effect of response duration was not significant ($F < 1$), whereas the main effect of tone duration again showed a non-significant trend, $F(1,15) = 4.09$, $p = .061$, $\eta_p^2 = .21$. The corresponding analysis of Experiment 3b showed the main effect of response duration to be significant, $F(1,15) = 6.68$, $p = .021$, $\eta_p^2 = .31$, whereas the remaining effects were not, $ps > .091$, $\eta_p^2 < .18$. A direct comparison of the PEs in Experiment 3a and 3b yielded a significant three-way interaction, $F(1,30) = 5.71$, $p = .023$, $\eta_p^2 = .16$. The interaction of response duration and experiment was not significant, $F(1,30) = 2.54$, $p = .121$, $\eta_p^2 = .08$, whereas the interaction of tone duration and experiment was, $F(1,30) = 7.33$, $p = .011$, $\eta_p^2 = .20$.

4.3. Discussion

The findings of Experiment 3 corroborate and extend the conclusions drawn from Experiment 2. Again, R–E compatibility effects only emerged if participants were explicitly instructed to imagine the effect tones, indicating that such purely imagined effects are not incorporated in action control automatically. Still, R–E compatibility effects were present in Experiment 3a in which the effect tones were played back to an inanimate dummy instead of a social partner as in Experiment 2a. These results indicate that deliberate imagination of the effect tones alone is sufficient to induce such R–E compatibility effects in both, social and non-social settings.

5. General discussion

The present experiments explored whether human action control is affected by sensory action effects even if these effects are never actually experienced to result from own actions, but are only implemented into action control by outcome imagination and anticipation. Participants performed either short or long key presses that predictably triggered short or long effect tones. In separate blocks, the mapping of actions and effects was either compatible (e.g., short responses ▶ short effect tones) or incompatible (e.g., short responses ▶ long effect tones). The effect tones were not played back to the participant but rather to the experimenter, who gave verbal feedback in Experiment 1 and did not give any tone-related feedback in Experiment 2. Accordingly, the participants never actually experienced the tones as consequences of their actions at any time during the experiment, but they were explicitly instructed to imagine the effects that their actions would cause instead. The crucial question was whether the mere imagination of one's own action effects may affect action control.

The results are straightforward: Responses were initiated faster and more accurately when they would produce compatible effect tones compared to incompatible effect tones. We thus demonstrate an R–E compatibility effect (Kunde, 2001, 2003) purely driven by imagined action effects that are known to result from own actions but that are not experienced physically. A relatively consistent influence of effect duration was also visible on the effective response durations, replicating previous work on temporal R–E compatibility (Kunde, 2003) and the duration of action effects in general (Kiesel & Hoffmann, 2004). This pattern of results did not depend on verbal feedback (as suggested by Experiment 2), ruling out alternative explanations in terms of experiencing semantic R–E relations (Badets et al., 2013; Hubbard et al., 2011; Koch & Kunde, 2002). The present findings thus extend basic assumptions of current accounts for effect-based action control by suggesting that action–effect associations can be acquired by mere knowledge and imagination of the expected action effects. As mentioned in the introduction, R–E compatibility effects are assumed to arise from bidirectional associations between actions and contingently following effects. Theories on effect-based action control usually hold that these associations require extensive practice (Elsner & Hommel, 2001, 2004; Hoffmann et al., 2009; Pfister et al., 2011) or at least a few instances (Wolfensteller & Ruge, 2011). Instead, the present results suggest that associations between actions and effects can be set up by mere intention, indicating that prior learning experience is not necessary for the acquisition of a specific action–effect association.

It should be noted, however, that the above conclusion emerged from samples of adult participants who have already acquired considerable knowledge of basic action–effect pairings they might encounter. Matters might be entirely different for infants who may well have to rely on actual experience for setting up initial action–effect associations. Especially the acquisition of bidirectional R–E associations is a developmental challenge that is overcome only after extensive practice (Eenshuistra, Weidema, & Hommel, 2004; Hauf, Elsner, & Aschersleben, 2004; Verschoor, Weidema, Biro, & Hommel, 2010) and that is preceded by only unidirectional use of R–E associations (Verschoor, Spapé, Biro, & Hommel, 2013). The emergence of purely intentional formations of R–E associations in ontogenetic development certainly is an interesting topic for future inquiry, as are possible effects of actual experiences on R–E associations that were built by mere intention (and vice versa). For instance, additional experiential learning might also strengthen action–effect associations acquired by instruction and further accentuate R–E compatibility effects.

By contrasting human and inanimate agents as experimental partners, we were further able to show that the instructed action–effect learning, as demonstrated in our experiments, is a general phenomenon that does neither rely on a social setup in general, nor on empathic processes in particular. This interpretation is supported by the fact that similar compatibility effects were obtained for human partners and inanimate dummies and compatibility effects were only modulated by the presence and absence of an explicit imagery instruction (Experiment 2a vs. 3a and Experiment 2b vs. 3b). If participants were merely informed about the occurrence of sound effects without an imagery instruction, compatibility effects were neither observed for human partners nor dummies. Therefore, our results indicate that instructed R–E learning in a social context is not necessarily automatic and social in nature, but based solely on the presence of the imagery instruction, as R–E compatibility effects only emerged when participants were explicitly instructed to imagine the effects. Furthermore, imagery conditions gave rise to overall slower responses than conditions without explicit instruction, which might be taken to reflect the increased cognitive effort to represent unperceivable effects.

Our findings also seem to resemble a recent discussion in the context of the social Simon effect (Sebanz, Knoblich, & Prinz, 2003). Even though first explanations for this effect focused mainly on the presence of a co-actor and the co-representation of his or her task, the social Simon effect was suggested not to depend on the presence of actual co-actors by several later studies. For instance, these studies found social Simon effects also when replacing a human co-actor with inanimate objects in the agent's surroundings (Dolk et al., 2011). Accordingly, the effect was suggested to reflect referential coding relative to salient objects rather than an actually social phenomenon (Dolk, Hommel, Prinz, & Liepelt, 2013). Such an account, however, does not seem to account for all findings in the literature. For instance, social Simon effects for humanoid robots as interaction partners depend on whether the agent is led to believe that one and the same robot is controlled by an active artificial intelligence or not (Stenzel et al., 2012). This finding might be taken to indicate that higher-level cognitions may indeed moderate the effect. Similar to the social Simon effect, the present R–E compatibility effects might be moderated if social aspects of the interaction are highlighted. Increasing the relevance of action outcomes for either the actor or the social partner might promote the automatic formation of R–E associations without explicit imagery instructions. Using action consequences of high relevance for human partners, such as highly aversive stimuli, might possibly also lead to a moderating effect of empathy. Future research will have to explore this question in detail (for more discussions of effect-based action control in shared tasks, see Kiernan, Ray, & Welsh, 2012; Pfister, Dignath, Hommel, & Kunde, 2013; Pfister et al., in press). Therefore, given appropriate circumstances, social partners might increase the tendency to anticipate corresponding sensory events – a hypothesis that seems to be a fruitful candidate for future research.

Whatever the outcome of such research will be, we can already make an important point here. Whereas empathy might automatically prompt different kinds of mental states we believe a social partner to have (Preston & de Waal, 2002; Singer & Lamm, 2009; Sonnby-Borgström, 2002), states that have the power to trigger own motor actions do not occur automatically but require mental effort. This makes sense for various reasons. If we automatically retrieved motor actions compatible to the mental states of social partners this would render joint action with complementary motor responses almost impossible (e.g., using a big saw). This also applies to competitive action. A boxer could barely punch an opponent effectively, if the anticipated effect of that action at the opponent (e.g., the force exerted on his chin) triggered already an action compatible to that effect in the puncher (e.g., some kind of avoidance behaviour).

Overall, the present experiments expand the literature on the impact of effect-based action control by showing that associations can be forged between actions and their known but entirely unperceivable effects. Thus, merely imagining certain consequences of one's actions, even without ever perceiving them, can have a similar impact on action control as actually experiencing these events as contingently following one's own actions.

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