Do We See It or Not? Sensory Attenuation in the Visual Domain

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Sensory consequences of an agent's actions are perceived less intensely than sensory stimuli that are not caused (and thus not predicted) by the observer. This effect of sensory attenuation has been discussed as a key principle of perception, potentially mediating various crucial functions such as agency and the discrimination of self-caused sensory stimulation from stimuli caused by external factors. Precise models describe the theoretical underpinnings of this phenomenon across a variety of modalities, especially the auditory, tactile, and visual domain. Despite these strong claims, empirical evidence for sensory attenuation in the visual domain is surprisingly sparse and ambiguous. In the present article, the authors therefore aim to clarify the role of sensory attenuation for learned visual action effects. To this end, the authors present a comprehensive replication effort including 3 separate, high-powered experiments on sensory attenuation in the visual domain with 1 direct and 2 preregistered, conceptual replication attempts of an influential study on this topic (Cardoso-Leite et al., 2010). Signal detection analyses were targeted to distinguish between true visual sensitivity and response bias. Contrary to previous assumptions and despite high statistical power, however, the authors found no evidence for sensory attenuation of learned visual action effects. Bayesian analyses further supported the null hypothesis of no effect, thus constraining theories that promote sensory attenuation as an immediate and necessary consequence of voluntary actions.

Keywords: sensory attenuation, visual domain, action effects, perception and action, replication

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Our ability to perceive the world around us is crucial for our survival within this environment. However, what we perceive does not represent an exact reproduction of the sensory input available to our receptors, but it is an interpretation of this input—opening up our perception to influences of bottom-up and top-down processes, unconscious or conscious, implicitly or deliberately employed (e.g., Mather & Sutherland, 2011; Schwarz, Pfister, & Büchel, 2016). These interpretations in many cases help us make sense of the world around us, and they help us act and react efficiently in response to the sensory information we receive, by directly affecting the perception and processing of the stimuli in question (e.g., DeLong, Urbach, & Kutas, 2005; Long & Toppino, 2004; Riccio, Cole, & Balcetis, 2013; Schwarz, Wieser, Gerdes, Mühlberger, & Pauli, 2013; Wieser & Brosch, 2012).

One instance of such differential processing of stimuli is sensory attenuation. Sensory attenuation describes the phenomenon that

sensory consequences of an agent's actions are perceived less intensely than sensory stimuli that are not caused by the observer. As such, sensory attenuation is thought to be a prime example of how action and perception interact to guide human beings to a more efficient and focused processing of sensory stimulation. It has been described and studied primarily in the tactile and auditory domain (e.g., Baess, Widmann, Roye, Schröger, & Jacobsen, 2009; Bays, Flanagan, & Wolpert, 2006; Blakemore, Frith, & Wolpert, 1999; Haggard & Whitford, 2004; Horváth, 2015; Weiss, Herwig, & Schütz-Bosbach, 2011a; Weller, Schwarz, Kunde, & Pfister, 2017), with only few studies investigating its effects on visual perception (Cardoso-Leite, Mamassian, Schütz-Bosbach, & Waszak, 2010; Desantis, Roussel, & Waszak, 2014; Hughes & Waszak, 2011; Mifsud et al., 2016; Yon & Press, 2017).

Theoretical models attempt to explain the physiological mechanisms underlying this effect with the most influential account being the internal forward model (Hughes, Desantis, & Waszak, 2013; Waszak, Cardoso-Leite, & Hughes, 2012; Wolpert, 1997; Wolpert, Ghahramani, & Jordan, 1995). This model postulates that for any action we take, "efference copies" of this specific motor command are used to form a prediction of the sensory consequences of this action and this prediction is subsequently compared with the actual sensory input (Blakemore et al., 1999; Wolpert et al., 1995). That is, our expectations of how sensory input should or should not change due to our actions lead to a differential processing of stimuli that can be predicted by our actions. As such, sensory attenuation has been implicated as a key principle of how humans distinguish between self- and other-produced sensory input. Moreover, it has been associated with possibly related phenomena such as intentional binding and sub-

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jective feelings of agency (Haggard & Tsakiris, 2009; Waszak et al., 2012; Weller et al., 2017). In this tenor, problems to differentiate between self and other, such as in schizophrenic patients suffering from passivity experiences and delusions of influence, have been attributed to a lack of sensory attenuation due to faulty internal forward models (Blakemore, Smith, Steel, Johnstone, & Frith, 2000; Lindner, Thier, Kircher, Haarmeier, & Leube, 2005; Oestreich et al., 2016; Shergill, Samson, Bays, Frith, & Wolpert, 2005).

Many of these claims are based on the assumption that sensory attenuation is indeed a key principle of perception that similarly affects the processing of action consequences across a variety of sensory modalities. However, whereas evidence for sensory attenuation seems to accumulate for the tactile and auditory domain (although often with only small effects on behavioral scales, see, e.g., Weiss et al., 2011a, b), evidence for sensory attenuation in the visual domain is currently scarce and ambiguous: Evidence on physiological correlates of sensory attenuation has shown diverging and somewhat unexpected results (Hughes & Waszak, 2011; Mifsud et al., 2016) and studies on behavioral effects are ambiguous, reporting effects ranging from sensory cancellation to sensory facilitation for learned visual action effects (Cardoso-Leite et al., 2010; Desantis et al., 2014; Yon & Press, 2017). As argued by the respective authors, differences in behavioral effects could be related to different approaches in experimental design, contrasting stimulus detection tasks with stimulus identification or discrimination tasks. Because of this diverging evidence based on different approaches in experimental design, it remains unclear whether or not sensory attenuation for learned visual action effects really does exist—and can be studied under laboratory conditions.

The first and as-of-yet most influential study on sensory attenuation for learned visual action effects (Cardoso-Leite et al., 2010) used the psychophysical approach of signal detection theory to distinguish between perceptual sensitivity (d') and the participants' response bias (c). This distinction is important because higher detection rates for one stimulus might not actually reflect a better capability to perceive this stimulus, but rather a different response pattern to this stimulus (e.g., participants might be more inclined to answer positively to the question "Did you see a pattern?", even if they are not sure, when the pattern presented was congruent to their expectations compared to a pattern incongruent to their expectations). As sensory attenuation effects relate to an agent's capability to perceive a stimulus, not their response patterns toward this stimulus, it is important to rule out this possibility. Despite this commendable approach, several methodological issues might have limited the informative value of this study, including a limited sample size ($n = 13$ analyzed participants). To clarify the role of sensory attenuation in the perception of learned visual action effects, we therefore set out to replicate this study with a considerably increased statistical power. For this endeavor, we chose a comprehensive approach of three independent replication studies, one direct replication and two conceptual replications. The first conceptual replication was intended to address several methodological concerns of the original study, and the second conceptual replication was intended to elucidate the question of whether sensory attenuation comes into play only for certain types of tasks (i.e., stimulus detection vs. stimulus identification; Desantis et al., 2014). Both conceptual replications have been preregistered in the Open Science Framework (osf.io/k3ews and

osf.io/chhh7) to allow a maximum of transparency on this project. All three replications represent high-powered experiments based on the original study by Cardoso-Leite and colleagues (2010) with a total of 149 participants addressing the main question: is there evidence for sensory attenuation for learned visual action effects? We used traditional null-hypothesis significance testing as well as Bayesian statistics to approach this question and analyzed our data regarding commonly used behavioral measures (such as percentage of correct answers) and regarding the measures as proposed by signal detection theory. With this approach, we aim to provide informative evidence clarifying the role of sensory attenuation for learned visual action effects.

Experiment 1: Direct Replication

Introduction

Experiment 1 represents a direct replication of the main experiment reported by Cardoso-Leite et al. (2010): Participants experienced that left or right keypresses consistently produced visual action effects (left- or right-tilted Gabor patches) in an initial acquisition phase and the impact of these associations was assessed in a following test phase. In this test phase, participants again pressed either the left or the right key and their actions now produced either a visual effect (left-tilted, right-tilted or vertical Gabor patch) or no effect. As in the original experiment, the possible identity of each effect was predicted by a cue that preceded the participants' keypress.

We took great care in replicating the original study as precisely as possible with the sole difference being a considerably increased power ($n = 46/41$ instead of $n = 16/13$; the first number of each pair indicates the number of participants that were tested whereas the second number indicates the number of participants included in the statistical analysis). As such, we expected the same results as were reported in the original article, that is, we expected stimuli congruent to previously learned action-effect associations to be detected less reliably than incongruent or neutral stimuli. We further expected this to be caused by differences in the sensitivity parameter d', but not in the response criterion c (according to signal detection theory).

Method

Participants. We recruited 46 individuals (mean age 25.5 years $\pm$ 0.9 SE_M; 38 female) with normal or corrected-to-normal vision. The original study (Cardoso-Leite et al., 2010) reported an effect size of Cohen's $d = 0.79$ for the crucial comparison of incongruent and congruent trials. However, because of several methodological concerns in the original study (for more details see the discussion of Experiment 1), we opted for a more conservative approach in calculating our sample size: we assumed a medium effect size of $d = 0.50$, a power of 0.80 and a two-tailed test despite the directional hypothesis. This resulted in a sample size of $n \geq 34$, and we recruited an additional seven participants to account for possible drop-outs (drop-out rate in Cardoso-Leite et al., 2010: 18.75%). We recruited three participants more than necessary for logistical reasons; the experiment was conducted in a four-PC laboratory, and to ascertain equal conditions for all participants, we always recruited the same amount of participants

for any time slot. All participants gave written informed consent and they received payment or course credit as compensation.

Procedure. The procedure in this experiment matched the procedure of the main experiment described by Cardoso-Leite and colleagues (2010). As in the original study, visual stimuli were presented on CRT monitors with a refresh rate of 100 Hz. In a preliminary phase, the individual base detection performance of the participants was measured to ensure a comparable difficulty level of the main experiment in all individuals. To this end, Gabor patches were presented with varying contrasts and detection performance was assessed for each contrast separately. In total, 500 trials were presented in random order, consisting of 250 control trials with no Gabor patches and 5×50 trials with Gabor patches at varying contrast levels. The contrast level best suited to produce a detection sensitivity d' of 2 was then chosen for the test phase of the main experiment.

The following main experiment was split into an acquisition and a test phase (see Figure 1). The acquisition phase allowed participants to learn the association of left and right keypresses with subsequent presentations of left- and right-tilted Gabor patches. The association of keypress to Gabor patch was held constant within individuals but was counterbalanced across participants. Trials started with an empty black circle appearing on the screen. Participants could then choose to press either a left or right key with their corresponding index fingers, using the "f" and "j" keys on a standard computer keyboard. Error messages were displayed if there was more than a 5,000 ms delay between the start of the trial and the participants' keypress, or if participants pressed a key other than the two designated keys. After each correct keypress, a clearly visible left- or right-tilted Gabor patch was shown (tilt angles: 60° or −60°) for 440 ms. Gabor patches were presented following a Gaussian temporal profile, with the contrast peaking at 200 ms after the keypress. All visual stimuli were presented on a gray background (see Cardoso-Leite et al., 2010). Every 50 trials, participants received feedback on the proportion of left and right keypresses they had performed up to that point. A total of 5% of all trials in the acquisition phase consisted of catch trials; in these trials, instead of a Gabor patch, a circle surrounding a red Gaussian blob appeared. Participants were asked to react to these catch trials by pressing both keys simultaneously. Error messages were displayed if there was more than a 5,000 ms delay between the appearance of a catch stimulus and the participants' keypresses or if participants failed to press both keys. The acquisition phase consisted of 450 trials in total.

The test phase measured the participants' stimulus detection capability. Each trial started with the black outline of a circle. Two parallel line segments were attached to this circle and served as orientation cues, indicating which Gabor patch, if any, would be presented in the present trial. The line segments were tilted by 60°, 0°, or −60°, to indicate Gabor patches of the corresponding orientation. After the trial started, participants could choose to press either a left or right key with their corresponding index fingers ("f" and "j"). After each keypress, a low-contrast (see preliminary phase) Gabor patch was shown in 50% of all trials, corresponding to the previous orientation cue and irrespective of the participant's keypress. The Gabor patch was either congruent or incongruent to the previously learned action-effect association (60° or −60°) or it was considered neutral (0°, vertical stripes) if the stimulus had not been used during the acquisition phase. The three possible orientations were randomized trial-by-trial. In the remaining 50% of all trials, no Gabor patches were presented. Subsequently, participants were asked to report whether or not they had seen a Gabor patch and then to rate their confidence in their response. To this end, the question "Did you recognize a pattern?" was displayed on the screen (in German: "Hast du ein Muster erkannt?"). Below the question, the words *yes* and *no*
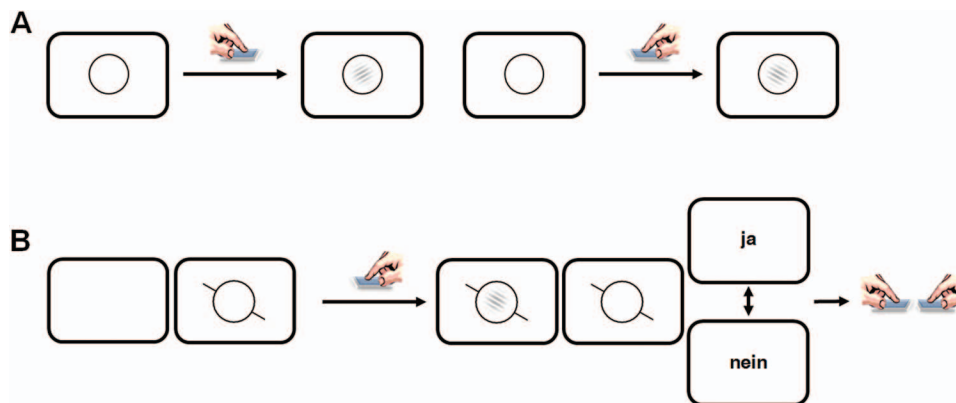

Figure 1. Design and procedure of the main experiment (Experiment 1). A: Acquisition phase. Trials started with an empty black circle. Participants then chose whether to press the left or right key which was followed by either a left- or right-tilted Gabor patch appearing on the screen. Action-effect associations were held constant within individuals, but were counterbalanced across participants. B: Test phase. Trials started with an empty black circle with two line segments attached, cueing the orientation of the possibly later presented Gabor patch. Participants chose whether to press the left or right key which was either followed by the previously cued Gabor patch (50% of all trials) or no effect (remaining trials). Participants were then asked to indicate whether they had seen a pattern or not by responding to alternatingly flashing "yes" and "no" responses on the screen. Please note that stimuli are not drawn to scale, stimulus background has been set to white and stimulus contrasts are increased for better legibility of this figure. See the online article for the color version of this figure.

(German: "ja" and "nein") were presented alternatingly in an ever-repeating loop, each message lasting for 600 ms. Participants gave their response by pressing the left and right key simultaneously when the intended response was displayed. If they failed to press both keys at the same time or it took them more than 5,000 ms to respond, participants were reminded to answer by pressing both keys. After a response was given, the question "How certain are you?" (German: "Wie sicher bist du dir?") was displayed with the options *not very certain*, *medium*, and *very certain* (German: "wenig", "mittel", and "sehr") being alternatingly presented below. Again, participants were asked to press both keys simultaneously when the intended response appeared on the screen. In total, the test phase consisted of 600 trials.

Statistical analyses. We used separate repeated-measures analyses of variance (ANOVAs) with the factor action-effect relation (congruent vs. incongruent vs. neutral) to analyze the percentage of correct responses (PC), the sensitivity parameter d' ((d') = z(hit rate) – z(false alarm rate)) and the response criterion c ($c = -0.5$ [z(hit rate) + z(false alarm rate)]), the latter two computed as specified by signal detection theory and employed by Cardoso-Leite et al. (2010). After significant effects in the omnibus ANOVA, we further calculated paired t tests for simple effects analysis. We followed-up all nonsignificant tests by calculating directional (for directional hypotheses in case of PC and d') or nondirectional Bayes factors (BFs; for nondirectional hypotheses in case of c) with the BayesFactor package version 0.9.12–2 of the R software environment version 3.3.1 (prior = 0.707, the current default in the BayesFactor package). BFs are computed as $f(data | H_0) / f(data | H_1)$ (with the f terms denoting marginal likelihoods) and we considered BFs greater than 3 as evidence for the null hypothesis over the alternative hypothesis and lower than 1/3 as evidence for the alternative hypothesis over the null hypothesis.[1] Trials were excluded when participants failed to perform the task according to the general instruction. Participants were excluded from the analysis when any of their d' values were lower than zero, indicating a greater propensity for noise than for signal (Stanislaw & Todorov, 1999). Five participants had to be excluded because of this criterion. All data and analysis scripts can be found on osf.io/97h8k.

Results

Percentage of correct responses. Detection performance for Experiment 1 is shown in Figure 2. Participants differed in their detection performance dependent on stimulus congruency, in that they detected neutral stimuli, that is, vertically striped Gabor patches that were not previously associated with either keypress, more reliably than stimuli either congruent or incongruent (i.e., opposite) to their previously learned action-effect association, $F(2, 80) = 13.44$ $p < .001$, $\eta_p^2 = .25$, $\Delta PC_{neutral—incongruent} = 4.0\%$, $t(40) = 4.50$, $p < .001$, $d = 0.70$, $\Delta PC_{neutral—congruent} = 3.9\%$, $t(40) = 3.84$, $p < .001$, $d = 0.60$ (d is computed as $t/\sqrt{n}$ for all pairwise comparisons). However, there was no difference between stimulus detection of congruent compared to incongruent stimuli, $\Delta PC_{incongruent—congruent} = -0.2\%$, $t(40) = 0.42$, $p = .675$, $d = 0.07$; directional BF = 7.09 (computed with a null interval of]-∞; 0]; nondirectional BFs are supplied in the online supplemental material for all analyses). The slight descriptive difference of detection performance between congruent and incongruent condi-

tions was contrary to our hypothesis, and the corresponding BF indicates clear evidence for the null hypothesis ($PC_{congruent} \geq PC_{incongruent}$) over the alternative hypothesis ($PC_{congruent} < PC_{incongruent}$).

Signal detection analyses. The pattern of the participants' correct responses is reflected in the results of the signal detection parameters as shown in Figure 2B–2C and Supplementary Figure S1. Participants differed in their visual sensitivity d' dependent on stimulus congruency, $F(2, 80) = 14.68$, $p < .001$, $\eta_p^2 = .27$. This effect was again mainly driven by an improved visual sensitivity toward neutral stimuli compared to both, incongruent and congruent stimuli, $\Delta d'_{neutral—incongruent} = 0.36$, $t(40) = 5.17$, $p < .001$, $d = 1.14$, $\Delta d'_{neutral—congruent} = 0.28$, $t(40) = 3.56$, $p = .001$, $d = 0.79$. There was no difference in sensitivity between stimuli that were congruent to previously learned action-effect associations and stimuli that were incongruent to these associations, $\Delta d'_{incongruent—congruent} = -0.09$, $t(40) = 1.40$, $p = .168$, $d = 0.31$, directional BF = 13.22. In fact, the descriptive difference between the sensitivity parameter d' toward congruent compared to incongruent stimuli was contrary to our hypothesis, mirrored in a high directional BF value which demonstrates considerable evidence for the null hypothesis ($d'_{congruent} \geq d'_{incongruent}$) over the alternative hypothesis ($d'_{congruent} < d'_{incongruent}$).

Participants also differed in their response criteria dependent on stimulus congruency, $F(2, 80) = 5.59$, $p = .005$, $\eta_p^2 = .12$. This was again mainly driven by the neutral condition, $\Delta c_{neutral—incongruent} = -0.13$, $t(40) = 2.71$, $p = .010$, $d = 0.60$, $\Delta c_{neutral—congruent} = -0.15$, $t(40) = 2.70$, $p = .010$, $d = 0.60$. More positive values of c indicate a higher tendency of the participants toward "no" than "yes" responses (Stanislaw & Todorov, 1999), that is, participants had a stronger propensity toward "no" answers in response to congruent and incongruent stimuli compared to neutral stimuli. Participants showed no difference in c between congruent and incongruent stimuli, $\Delta c_{incongruent—congruent} = -0.02$, $t(40) = 0.41$, $p = .685$, $d = 0.09$; BF = 5.48. The nondirectional BF value again indicates clear evidence for the null hypothesis ($c_{congruent} = c_{incongruent}$) over the alternative hypothesis ($c_{congruent} \neq c_{incongruent}$).

Discussion

In this direct replication of Cardoso-Leite et al. (2010) with considerably enhanced sample size to increase power and therefore confidence in the experimental results, we found clear evidence for the absence of any sensory attenuation effect of congruent compared to incongruent trials as measured via BFs. Our results further showed a significant difference between neutral and either, congruent and incongruent stimuli, in that neutral stimuli were more reliably perceived and that they elicited different response patterns. This could be explained by an enhanced novelty of these stimuli as they had not appeared in the acquisition phase. Furthermore, as neutral stimuli were the same across participants (whereas congruent and incongruent stimuli were exchanged and thus counterbal-

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[1] Nondirectional BFs were computed with the command "1/ ttest.tstat(t = x.xx, n1 = xx, n2 = 0, nullInterval = NULL, rscale = 0.70710678, simple = TRUE)". Directional BFs (nullhypothesis: congruent ≥ incongruent) were computed with the command "dirBF <- ttestBF(x = [congruent_data], y = [incongruent_data], mu = 0, paired = TRUE, nullInterval = c(-Inf, 0))"; 1/dirBF[1]. All commands are based on the BayesFactor package of the R software environment, see above.
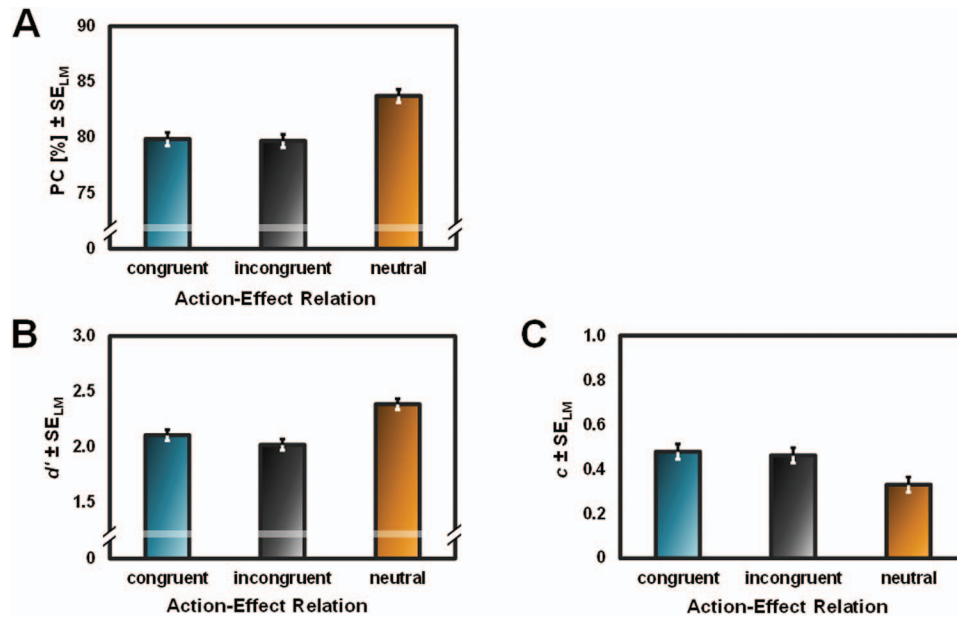
Figure 2. Results of Experiment 1. A: Percentage of correct responses (PC) for each condition. Participants detected neutral stimuli more reliably than either congruent or incongruent stimuli. Detection performance did not differ between congruent and incongruent stimuli. B: Sensitivity parameter d' for each condition. Participants showed higher sensitivity toward neutral stimuli than toward either congruent or incongruent stimuli, whereas d' did not differ between congruent and incongruent stimuli. C: Response criterion c for each condition. Participants showed a higher tendency toward "no" answers for congruent and incongruent stimuli than for neutral stimuli, whereas c did not differ between congruent and incongruent stimuli. Error bars depict within-subjects standard errors (Loftus, & Masson, 1994). For data depiction similar to the original figures reported by Cardoso-Leite et al. (2010), please see Supplementary Figure S1 in the online supplemental material. See the online article for the color version of this figure.

anced across participants) we cannot preclude an explanation based solely on the physical attributes of the stimuli. The well-known oblique effect (e.g., Appelle, 1972; Furmanski & Engel, 2000) describes this very phenomenon: human beings (as well as other animals) show greater perceptual sensitivity to patterns with vertical or horizontal orientations. In other words, performance in perceptual tasks is superior when stimuli represent vertical or horizontal gratings compared to other orientations—precisely the result pattern that we found in this experiment.

Both of these results stand in stark contrast to the results reported in the original article by Cardoso-Leite et al. (2010) which found no difference for d' between incongruent and neutral conditions, but instead between congruent and incongruent as well as between congruent and neutral conditions, and no difference regarding congruency conditions for c. This difference between the original results and our results is, of course, difficult to explain satisfactorily and all attempts at explanation are speculative at this point. Because the methodology in both studies is highly similar, the most likely explanation would be a false positive finding in the original article. Limited sample sizes as used by Cardoso-Leite et al. are known to increase variability in results and therefore to decrease data reliability based on such sample sizes (see, e.g., Bertamini & Munafò, 2012). Further circumstances (e.g., the precise instructions used to direct the participants) can also not be ruled out as possible factors, but seem unlikely, as only the most necessary information has been included in the initial instruction,

at least in the present experiment. Another explanation could lie in the possibility that in the seven years between publication of the original study and the present experiment, general differences have emerged in the population that account for the discrepancy in results, such as attentional deficits in certain tasks due to media multitasking (e.g., Ophir, Nass, & Wagner, 2009). Although we cannot preclude such explanations, we believe it unlikely that such differences solely account for the differing results in the original report and the present Experiment 1. It is, for example, noteworthy that sensory attenuation literature describes evidence from populations differing in many aspects, such as nationality, gender, age, and general cultural upbringing across several decades of publications. Although these differences might account for some of the variability in the data, the underlying patterns do not seem to be systematically different for the variety in samples used in the experiments. We therefore assume that underlying differences in the sample populations of the original report and the present experiment would not lead to such big effects in the results as observed here.

However, an interesting aspect of the original data pertains to the absence of the oblique effect as described above (Appelle, 1972; Furmanski & Engel, 2000). Based on the oblique effect, a better discrimination performance for neutral stimuli than for incongruent stimuli (and likewise for congruent stimuli, of course) would be expected, because neither incongruent nor neutral stimuli match the assumed efference copy and should therefore differ only

with regard to physical properties of the employed stimuli (in addition to the frequency of exposure to specific stimuli in the preceding experimental phases). Whereas this difference did not emerge for the original report by Cardoso-Leite and colleagues (2010), the present data yielded robust evidence for it in our replication. It thus seems possible that a potential issue with the original findings lies specifically with the incongruent condition—if performance in the incongruent condition had been lower, the original pattern of results would reflect the results of this replication and, moreover, replicate the expected oblique effect.

Which conclusions do the results of Experiment 1 indicate regarding sensory attenuation in the visual domain? We believe the informative value of both, the original experiment and the present replication to be limited because of four methodological issues relating to the experimental design. First, sensory attenuation is assumed to arise because predicted feedback ("efference copy") and actual feedback cancel each other out. Predictions were assumed to stem from the action-effect associations that were built up in an initial acquisition phase that took about 8 min (450 trials). It is tacitly assumed that the corresponding action-effect associations are held up throughout the subsequent test phase of about 47 min (600 trials). In this design, the test phase necessarily serves as an extinction phase leading the participants to actively "unlearn" the previously learned associations and recent findings suggest effect-based processes to be rather flexible and efficient (Pfister, Kiesel, & Melcher, 2010; Wolfensteller & Ruge, 2011). Second, during the test phase, participants were cued in each trial which potential stimulus to expect, irrespective of which action they performed. A second tacit assumption of the procedure is that action-based expectations still prevail in this situation even though they do not provide any information in this context whereas an explicit (action-unrelated) expectation is of far greater predictive value. It seems plausible that the explicit expectations would override the previous associations, especially because participants are informed that the previous mapping does no longer apply (Waszak, Pfister, & Kiesel, 2013). These two points may decrease the probability of observing sensory attenuation, in the original design as well as in Experiment 1, because they suggest the relevant anticipations to be absent in the test phase (or even contrary to the ones established in the learning phase). Third, the stimuli used in the acquisition phase and in the test phase further came with different contrasts. The use of physically differing stimuli in acquisition and test phase, however, clearly counteracts the chances to observe sensory attenuation because such a manipulation already introduces a confounding difference of predicted and actual sensory feedback even in the compatible condition. Fourth and finally, participants were asked to answer with "yes" or "no" to the question of whether they had observed a particular stimulus. However, the means to do this was to respond to a "yes" or "no" signal on the screen that flashed alternatingly back and forth. Subjective experience during reprogramming the study according to the descriptions given in the original article suggests that this procedure provides a considerable source for error: in a significant proportion of trials, we found ourselves to give an unintended answer merely because the keypress was performed just a little too late and the answer on the screen had already switched back to its opposite in the meantime.

The described issues leave open the possibility that sensory attenuation for learned visual action effects does generally occur,

but the reported experiment could not detect it by design. In Experiment 2, we therefore alleviated these issues via a changed experimental design to allow for a more straightforward interpretation to gather more conclusive evidence for or against sensory attenuation for learned visual action effects.

Experiment 2: Conceptual Replication

Introduction

We implemented several changes to the experimental design of Experiment 1 to address the methodological issues limiting the informative value of the previous study (see the discussion of Experiment 1). First of all, instead of one short acquisition phase at the beginning of the experiment followed by a single test phase, we employed four alternating acquisition and test blocks. Previous evidence suggests that action-effect associations are evoked very quickly and efficiently (e.g., Pfister et al., 2010; Pfister, Pfeuffer, & Kunde, 2014; Wolfensteller & Ruge, 2011) which ensures stable action-effect associations even after short acquisition phases, but unfortunately also leads to possible extinction effects. These extinction effects were countered here by the alternation of acquisition and test blocks allowing participants to relearn action-effect associations throughout the experiment. We also did not include a cue indicating which Gabor patch (if any) would appear in this trial to avoid competing and therefore confounding expectations due to the cue. Such secondary expectations are likely to override expectations based on previously learned action-effect associations, due to their salience and their higher predictive value, thus inhibiting any possible sensory attenuation effect. Furthermore, we decided on using a stimulus equal in visual parameters in acquisition and test phase—also regarding their contrast to avoid inherent prediction errors even in congruent trials. Instead, we considerably shortened stimulus presentation and masked the stimuli to impede stimulus detectability. Finally, the error source of the quickly alternating "yes" and "no" responses was eliminated by presenting both answers simultaneously on the screen and allowing participants to press different keys dependent on the intended answer. Key-answer mapping was held constant within individuals but was counterbalanced across participants.

In addition to these changes, we also sought to increase the informative value of Experiment 2 by eliminating the neutral condition in the test phase. Sensory attenuation is primarily demonstrated by juxtaposing congruent and incongruent trials. An additional comparison of congruent and neutral trials does not considerably increase the amount of gained information on possible sensory attenuation effects, and there are no clear hypotheses based on sensory attenuation regarding the comparison of incongruent and neutral trials. By focusing on congruent and incongruent trials, we were able to measure detection performance in these conditions more precisely which should increase statistical power.

We also included two timing manipulations in Experiment 2 (as described in more detail in the Procedure section), that is, we included a varying interval before the participants could press a key and also systematically varied the onset of the Gabor patch (i.e., the action-effect interval) after keypress in effect trials of the test phase. The first manipulation was intended to keep participants more focused on the task by reducing the predictability of the keypress's specific timing requirements. The second manipulation

was intended to elucidate the question of changes in sensory attenuation dependent on immediate action effect appearance versus short delays as previous studies indicate that sensory attenuation may differ across action-effect intervals (e.g., Bays, Wolpert, & Flanagan, 2005; Blakemore et al., 1999; Weller et al., 2017; Yon & Press, 2017). Whereas research in the tactile domain finds the strongest sensory attenuation for immediate action effects (Bays et al., 2005), recent findings suggest that in the visual domain, immediate action effects rather lead to a facilitation of expected sensory input, whereas sensory attenuation was found for slightly longer delays (Yon & Press, 2017). To assure that any existing sensory attenuation effects can be found, we therefore opted to introduce immediate action effects as well as delayed ones.

Method

Participants. We recruited 55 individuals (mean age 20.3 years $\pm$ 0.2 SE_M; 47 female) with normal or corrected-to-normal vision for participation in this study. For power analysis, we again assumed a conservative medium effect size of $d = 0.50$ (compared to the estimate of $d = 0.79$ from the original study). We further increased the intended power to 0.90 and assumed a conservative two-paired testing approach despite the directional hypothesis. This results in a sample size of $N \geq 44$, plus an additional 9 participants to account for possible drop-outs. We recruited two participants more than necessary for logistical reasons (cf. Experiment 1). All participants gave written informed consent and they received payment or course credit as compensation.

Procedure. The study design of Experiment 2 was intended to keep the basic paradigm similar to the original study (Cardoso-Leite et al., 2010) and Experiment 1, while at the same time addressing the methodological issues discussed above. Before the

start of the experiment, participants were shown left- and right-tilted Gabor patches to familiarize themselves with this stimulus. The experiment was again split into acquisition and test blocks (see Figure 3) which alternated four times in total resulting in eight separate blocks (four acquisition blocks and four test blocks). The acquisition blocks allowed participants to learn the association of left and right keypresses with subsequent presentations of left- and right-tilted Gabor patches. The association of keypress to Gabor patch was held constant within individuals, but counterbalanced across participants. Trials started with the presentation of a fixation cross on the screen for 500 ms. After the fixation cross disappeared, participants could choose to either press a left or right key with their corresponding index fingers, using the "f" and "j" keys on a standard computer keyboard. If participants pressed a key before the fixation cross disappeared, an error message appeared on the screen. After each correct keypress, a clearly visible left- or right-tilted Gabor patch was shown (tilt angles: 60° or −60°) for 1,000 ms. Each acquisition block consisted of 36 trials, resulting in 144 acquisition trials in total across all four acquisition blocks. At the end of each acquisition block, participants received feedback on the proportion of left and right keypresses they had performed during this block.

The test blocks measured the participants' stimulus detection capability. Each trial started with the presentation of a fixation cross on the screen for 500 ms followed by a blank screen for 500, 750, or 1,000 ms (randomized across trials). An exclamation point appeared after this interval and represented a "Go" signal for the participants who could choose whether to press the left or the right key. After the keypress a left- or right-tilted Gabor patch was presented in 50% of all trials, either immediately after the keypress (i.e., with a delay of ≤20 ms due to technical limitations such as
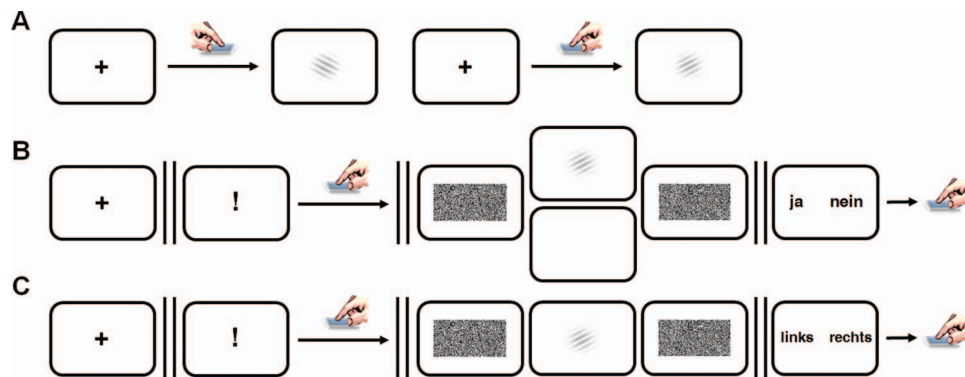

Figure 3. Design and procedure of Experiment 2 and 3. Two vertical lines indicate inserted blank screens. A: Acquisition phase of Experiment 2 and 3. Trials started with fixation cross. Participants then chose whether to press the left or right key which was followed by either a left- or right-tilted Gabor patch appearing on the screen. Action-effect association was held constant within individuals, but was counterbalanced across participants. B: Test phase of Experiment 2. Trials started with a fixation cross, followed by a short preaction interval (vertical lines). The subsequent exclamation point served as a "Go" signal for the participants to press either the left or right key (free choice) which was then followed by an action-effect interval and the presentation of a masked Gabor patch (50% of all trials) or a masked blank screen (remaining trials). Participants then were asked to indicate whether they had seen a pattern or not by responding to "yes" and "no" responses on the screen. C: Test phase of Experiment 3. The procedure was as in Experiment 2 with the exception that a masked Gabor patch was presented on every trial. Participants then were asked to indicate whether they had seen a left- or right-tilted Gabor patch responding to "left" and "right" response labels on the screen. Stimuli are not drawn to scale and stimulus contrasts are increased for better legibility. See the online article for the color version of this figure.

screen refresh time), or after an action-effect interval of 250 ms or 500 ms during which a blank screen was shown. Both possible orientations of the Gabor patch were randomized trial-by-trial and presented irrespective of which key was pressed. In the remaining 50% of all trials, a blank screen was displayed instead. Prior to the presentation of the Gabor patch or the blank screen, a rectangular pixel cloud was shown for 50 ms (premask) and right after the presentation of the Gabor patch or the blank screen two rectangular pixel clouds were shown successively for 50 ms and 70 ms (postmasks), respectively. The stimulus masks were intended to impede easy stimulus detection. Furthermore, to adapt the difficulty of stimulus detection to the individual participants, the timing of stimulus presentation across all conditions was adjusted according to a staircase algorithm to allow for an overall success rate of 75% on average. The staircase algorithm provided the participants with slightly more or less time to perceive the Gabor patch (by adjusting the stimulus duration by 10 ms) if the success rate of 75% was not met at the respective point of the experiment. That is, if participants had more difficulty in detecting the stimuli, the stimuli were presented longer than for participants who had less difficulty in detecting the stimuli. This procedure corresponded to the individual difficulty setting by adjusting the contrast level of the test stimuli in the original design (Cardoso-Leite et al., 2010, and Experiment 1). After another short pause (blank screen for 500 ms), participants were then asked to report whether or not they had seen a Gabor patch. To this end, the question "Did you see a Gabor patch?" (German: "Hast du einen Gabor Patch gesehen?") was displayed on the screen. Below, the words *yes* and *no* (German: "ja" and "nein") were presented on the left and right side of the screen, respectively. Key-answer mapping was counterbalanced across participants with "yes" being presented on the left side of the screen for half of the participants and on the right side of the screen for the other half (and "no" vice versa). Participants gave their response by pressing the left or right key to indicate their answer. Each test block consisted of 72 trials, resulting in 288 test trials in total across all four test blocks. At the end of each test block, participants received feedback on the proportion of left and right keypresses they had performed during this block.

Statistical analyses. We used a paired-samples t test to compare the percentage of correct responses (PC) between congruent and incongruent action-effect relations in the test phase. We fur-

ther employed a repeated measures ANOVA with the factors action-effect relation (congruent vs. incongruent) and action-effect interval (immediate presentation vs. 250 ms vs. 500 ms) to test for any interactions of congruency with action-effect delays. For data analysis according to signal detection theory, we calculated paired-samples t tests to compare the sensitivity parameter d' and the response criterion c between congruent and incongruent action-effect relations. In case of nonsignificant tests, we employed directional (for directional hypotheses in case of PC and d') or nondirectional BFs (for nondirectional hypotheses in case of c) as in Experiment 1. Trials were excluded when participants failed to perform the task according to the general instruction. No participants had to be excluded (all individual d's > 0). All data and analysis scripts can be found on osf.io/97h8k.

Results

Percentage of correct responses. Detection performance for Experiment 2 is shown in Figure 4A. Detection rates did not differ between congruent and incongruent action effects, $\Delta PC_{incongruent—congruent} = 0.5\%$, $t(54) = 0.61$, $p = .544$, $d = 0.08$; directional BF $= 3.94$. The directional BF value indicates evidence for the null hypothesis ($PC_{congruent} \geq PC_{incongruent}$) over the alternative hypothesis ($PC_{congruent} < PC_{incongruent}$). The ANOVA targeting the effects of action-effect interval on sensory attenuation revealed no significant effects, $ps > .270$.

Signal detection analyses. The results of the signal detection parameters confirm the results of the PC data as shown in Figure 4B-C. Participants showed no difference in the sensitivity parameter d' toward congruent or incongruent stimuli, $\Delta d'_{incongruent—congruent} = 0.03$, $t(54) = 0.56$, $p = .578$, $d = 0.08$; directional BF $= 4.14$. The directional BF value again indicates evidence for the null hypothesis ($d'_{congruent} \geq d'_{incongruent}$) over the alternative hypothesis ($d'_{congruent} < d'_{incongruent}$).

Participants also did not differ in their response patterns toward congruent and incongruent stimuli, $\Delta c_{incongruent—congruent} = -0.01$, $t(54) = 0.58$, $p = .563$, $d = 0.08$; BF $= 5.79$. The nondirectional BF indicates clear evidence for the null hypothesis ($c_{congruent} = c_{incongruent}$) over the alternative hypothesis ($c_{congruent} \neq c_{incongruent}$).
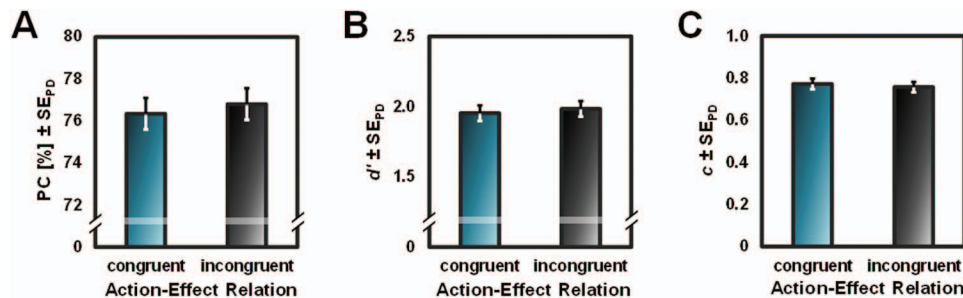

Figure 4. Results of Experiment 2. A: Percentage of correct responses (PC) for each congruency condition. Detection performance did not differ between congruent and incongruent stimuli. B: Sensitivity parameter d' for each congruency condition. d' did not differ between congruent and incongruent stimuli. C: Response criterion c for each congruency condition. c did not differ between congruent and incongruent stimuli. Error bars depict standard errors of paired differences (Pfister & Janczyk, 2013). See the online article for the color version of this figure.

Discussion

In this conceptual replication based on Cardoso-Leite et al. (2010), we implemented several changes to the experimental design to counteract methodological pitfalls possibly affecting the informative value of the original study. These changes, however, did not lead to different results than the direct replication (Experiment 1): We found no evidence for sensory attenuation effects for learned visual action effects—in fact, we found again evidence for the absence of such effects. Moreover, stimulus detection was also not affected by the duration of the action-effect interval, that is, lack of sensory attenuation effects cannot be attributed to timing specifications.

So far, we have only investigated sensory attenuation for learned visual action effects in detection tasks, that is, tasks, during which participants had to decide whether or not a stimulus was present in the preceding trial. However, many studies on sensory attenuation do not rely on detection tasks but on discrimination or identification tasks. In these paradigms, participants are asked to either judge a stimulus by comparing it to another stimulus regarding a specific property (e.g., loudness or brightness; Weiss et al., 2011a; Yon & Press, 2017) or to identify which stimulus (among two or more) was presented (Desantis et al., 2014). In both cases, the question lies not in whether or not a stimulus is perceived; rather the studies ask whether the stimulus is perceived similarly or differently than another stimulus. Interestingly, in this context, sensory attenuation in the auditory domain seems to be found routinely (e.g., Weiss et al., 2011a, b). Other authors, in contrast, argue that intensity judgments and stimulus identification should be facilitated, not impaired, for congruent stimuli (Desantis et al., 2014).

In the next experiment, we therefore aimed to tackle this question with a conceptual replication of Experiments 1 and 2 in which we exchanged the detection task for an identification task (Desantis et al., 2014).

Experiment 3: Stimulus Identification

Introduction

Experiment 3 represents a further conceptual replication and extension of the previous experiments, designed to target the question of whether sensory attenuation can be found for stimulus identification of learned visual action effects. To this end, we slightly adapted Experiment 2 by exchanging the detection task with an identification task, but kept all other parameters and stimuli constant to allow for optimal comparison of the results.

Method

Participants. We recruited 62 individuals (mean age 26.5 years $\pm$ 0.9 SE_M; 51 female) with normal or corrected-to-normal vision for participation in this study. As previous studies report effects for attenuation as well as for facilitation of stimulus processing (two opposing effects), we again opted for a medium effect size of $d = 0.50$ and a nondirectional analysis for power analysis. We further increased study power to 0.95 to ensure a maximum of power. This results in a sample size of $N \geq 54$. Previous studies differ in their reports of drop-outs, therefore we simply opted for

an additional 8 participants similarly to Experiments 1 and 2. All participants gave written informed consent and received payment or course credit as compensation.

Procedure. The study design of Experiment 3 was similar to Experiment 2 with the following changes during the test phase (Figure 3C): The test blocks were intended to measure the participants' capability to correctly identify the presented stimuli. To this end, all keypresses elicited the presentation of a masked Gabor patch, either left-tilted (50% of all trials) or right-tilted (remaining 50% of all trials). Both possible orientations were randomized trial-by-trial and presented irrespective of which key was pressed. At the end of each trial, participants were asked to indicate whether they had seen a left- or right-tilted Gabor patch (Question: "Were the stripes of the Gabor patch left-tilted or right-tilted?", German: "Waren die Streifen des Gabor Patches links-diagonal oder rechts-diagonal?", with the possible answers "left", German: "links", and "right", German: "rechts", presented on the left and right side of the screen, respectively). To avoid confusion, the answer "left" was always presented on the left side and the answer "right" always on the right side. Participants gave their response by pressing the left or right key depending on whether they intended to answer "left" or "right". Each test block consisted of 72 trials, resulting in 288 test trials in total across all four test blocks.

Statistical analyses. We used a paired-samples t test to compare the percentage of correct responses (PC) between congruent and incongruent action-effect relations in the test phase. We further employed a repeated-measures ANOVA with the factors action-effect relation (congruent vs. incongruent) and action-effect interval (immediate vs. 250 ms vs. 500 ms) to test for any interactions of congruency with action-effect delays. For data analysis according to signal detection theory, we calculated paired-samples t tests to compare the sensitivity parameter d' and the response criterion c between congruent and incongruent action-effect relations. In case of nonsignificant tests, we used nondirectional BFs (for nondirectional hypotheses) as in Experiments 1 and 2. Trials were excluded when participants failed to perform the task according to the general instruction. Nine participants had to be excluded because they had d' values lower than zero in at least one condition. All data and analysis scripts can be found on osf.io/97h8k.

Results

Percentage of correct responses. Stimulus identification performance for Experiment 3 is shown in Figure 5A. Stimulus identification did not differ between congruent and incongruent action effects, $\Delta PC_{incongruent—congruent} = 0.6\%$, $t(52) = 0.35$, $p = .730$, $d = 0.05$; nondirectional BF = 6.30. The BF value demonstrates clear evidence for the null hypothesis ($PC_{congruent} = PC_{incongruent}$) over the alternative hypothesis ($PC_{congruent} \neq PC_{incongruent}$). The analysis targeting the effects of action-effect interval on sensory attenuation revealed no significant effects, $ps > .202$.

Signal detection analyses. Participants did not show different sensitivity depending on congruency, $\Delta d'_{incongruent—congruent} = 0.04$, $t(52) = 0.30$, $p = .765$, $d = 0.04$; nondirectional BF = 6.40, nor did they differ in their response patterns toward congruent and incongruent stimuli, $\Delta c_{incongruent—congruent} = 0.01$, $t(52) = 0.22$, $p = .824$, $d = 0.03$; BF = 6.53. The nondirectional BF values again indicate clear evidence for the null hypotheses ($d'_{congruent} = $
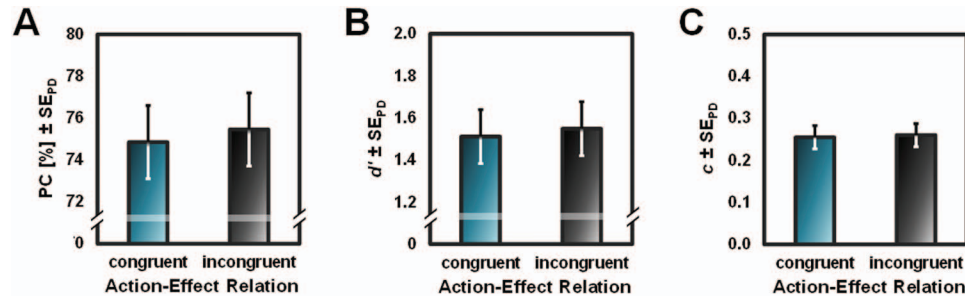
Figure 5. Results of Experiment 3. A: Percentage of correct responses (PC) separately for each congruency condition. Stimulus identification performance did not differ between congruent and incongruent stimuli. B: Sensitivity parameter d' for each congruency condition. d' did not differ between congruent and incongruent stimuli. C: Response criterion c for each congruency condition. c did not differ between congruent and incongruent stimuli. Error bars depict standard errors of paired differences (Pfister & Janczyk, 2013). See the online article for the color version of this figure.

$d'_{incongruent}$ and $c_{congruent} = c_{incongruent}$) over the alternative hypotheses ($d'_{congruent} \neq d'_{incongruent}$ and $c_{congruent} \neq c_{incongruent}$).

Discussion

Experiment 3 targeted the question of whether sensory attenuation, that is, cancellation of sensory input, or facilitation of sensory input might be elicited by stimulus identification as previous studies argue for both possibilities (Desantis et al., 2014; Weiss et al., 2011a; Yon & Press, 2017). In a high-powered experiment, we implemented an identification task similar to the detection task employed in Experiment 2 to allow for optimal comparison of the results. Our results in Experiment 3 mirror the results of Experiment 1 and 2: there was no evidence for sensory attenuation in our data, nor for facilitation of sensory input. In contrast, our data indicate the absence of any such effects. Moreover, as in Experiment 2, stimulus identification was not affected by the action-effect delay, thereby confirming that lack of sensory attenuation was not caused by timing specifications. These results suggest that the absence of sensory attenuation effects in Experiment 1 and 2 cannot be attributed to task differences such as stimulus detection versus stimulus identification tasks.

It should be noted that the adapted designs of Experiment 2 and 3 also come with several limitations that could have possibly decreased sensory attenuation effects. First, expectations of sensory consequences could have been diminished due to differences in stimulus presentation between the acquisition and test phases (unmasked vs. masked stimulus presentation). Even though the eventually employed Gabor patches were similar in both phases, pre- and postmasks arguably also introduce a change in visual stimulation. Second, sensory attenuation could be hampered by the introduction of varying delays between action and action-effect. Previous evidence suggests, however, that varying delays are not enough to obliterate sensory attenuation effects (e.g., Bays et al., 2005; Yon & Press, 2017), especially given the sample size of the present experiment. Moreover, we have not included catch trials into our acquisition phases as has been done in Experiment 1 (and the original experiment by Cardoso-Leite et al., 2010). We can therefore not rule out that participants simply did not pay any attention to the Gabor patches elicited by their keypresses. However, previous evidence suggests that even without such catch

trials during the acquisition phase, effect-based processes can be detected robustly and reliably in action effect research (e.g., Pfister, Heinemann, Kiesel, Thomaschke, & Janczyk, 2012). It seems thus unlikely that the absence of sensory attenuation effects in this experiment is based on a lack of control mechanism in the acquisition phase to measure the participants' focus on the task at hand. Lastly, a possible limitation could stem from spatial interference effects in Experiments 2 and 3 because stimulus-triggering actions and ratings were made with the same set of response keys. That is, participants could show preference for the same or for different keypresses (repetition or alternation biases) within one trial which could bias the detection or identification response. To ascertain that our results were not driven by such effects, we reanalyzed the data of Experiment 2 and 3 and excluded all participants who showed a preference for either same or different keypresses within one trial (i.e., in over 60% of all test trials). The results of this analysis confirmed the previous results: we found no evidence for sensory attenuation effects in either experiment (Experiment 2—PC: $t(53) = 0.65$, $p = .522$; d': $t(53) = 0.59$, $p = .559$; c: $t(53) = 0.41$, $p = .686$; Experiment 3—PC: $t(46) = 0.54$, $p = .589$; d': $t(46) = 0.71$, $p = .481$; c: $t(53) = 0.40$, $p = .695$). We thus believe it unlikely that spatial interference effects are responsible for the lack of sensory attenuation effects in our data.

General Discussion

In three experiments with a total of 149 analyzed participants, we addressed the question of whether or not sensory attenuation occurs for learned action effects in the visual domain. We used both stimulus detection and identification tasks and computed standard statistical analyses as well as BFs for a better interpretation of nonsignificant results. The paradigms we chose were closely related to the experimental design of the original study targeting this question (Cardoso-Leite et al., 2010). That is, the first experiment represented a direct replication of the original study, whereas the second and third experiments represented conceptual replications featuring a couple of modifications to rule out several methodological issues and extend the informative value of our approach. We further took care to calculate standard measures of performance such as the participants' PC as well as distinguish between perceptual sensitivity and response bias according to

signal detection theory. This latter approach is important because changes in measures such as PC could be based on changes in sensitivity or response bias or both. However, hypotheses regarding sensory attenuation only predict changes in sensitivity, whereas changes in response bias are not theoretically informative regarding sensory attenuation.

In all three experiments, we found no evidence for sensory attenuation, neither in stimulus detection nor in stimulus identification tasks. Indeed, we found evidence for an *absence* of sensory attenuation effects via BFs. This stands in contrast to previous studies reporting sensory attenuation for visual learned action effects (Cardoso-Leite et al., 2010; Yon & Press, 2017; but see Desantis et al., 2014). However, some of these studies employed rather limited sample sizes ($N = 13$ or $N = 16$ participants) limiting the statistical reliability of these approaches. Moreover, even among the few studies addressing the question, results were ambiguous with some studies even showing facilitation of sensory stimuli and others the expected cancellation of sensory stimuli if stimulus appearance was congruent with previously learned action-effect associations. Only one recent study demonstrated reliable effects of first stimulus facilitation and then cancellation across two or three studies (Yon & Press, 2017). Interestingly, the first two experiments in this article demonstrating sensory attenuation (i.e., stimulus cancellation) did not exclude response bias as a possible factor—and the third experiment which attempted to resolve this issue did not show a significant sensory attenuation effect. Our results suggest that sensory attenuation effects might simply not occur for learned visual action effects—at least not in experimental paradigms as were used in the present experiments (and have been similarly used in past studies).

These findings clearly show that sensory attenuation does not necessarily and automatically occur for all predictable effects of a voluntary action. Sensory attenuation thus appears to be less general a principle of human perception than often assumed.

However, there might be a distinction between sensory attenuation based on newly learned action effects, such as studied in the present experiments (as well as in many other studies on sensory attenuation, e.g., in the auditory domain, Baess et al., 2009; Horváth, 2015; Weiss et al., 2011a, b; Weller et al., 2017), and action effects that do not have to be trained in a study context as they naturally occur in all respective actions, such as tactile or proprioceptive feedback during acting (e.g., Shergill, Bays, Frith, & Wolpert, 2003) or oculomotor reafference (von Holst & Mittelstaedt, 1950). Of course, our experiments can only give conclusions regarding newly learned action effects, that is, our data indicate that sensory attenuation for visual action effects might not occur for action effects that have only just been acquired.

Another factor that might mediate the occurrence of sensory attenuation effects is an agent's intention and his or her focus of attention: Only if agents intend to produce certain effects and only if they pay attention to their actions and the subsequent action effects will they anticipate the effects during action control (e.g., Janczyk, Yamaguchi, Proctor, & Pfister, 2015; Wirth, Pfister, Brandes, & Kunde, 2016). In turn, because sensory attenuation builds on a cancellation of predicted (i.e., anticipated) action consequences and actual reafferences, sensory attenuation might only occur if an agent focuses on the action effects in question. A possibility to test this would be by increasing the salience and relevance of the participants' expectations for the task at hand.

However, as the relevance of expectations does not seem to play a role in the tactile or auditory domain, such an explanation would distinguish sensory attenuation effects in the visual domain from those in other sensory modalities. A further possibility would be to use a paradigm that tests the participants' anticipation efforts via other tried behavioral effects, such as response-effect compatibility effects (Ansorge, 2002; Chen & Proctor, 2013; Kunde, 2001; Pfister et al., 2010; Pfister & Kunde, 2013). This approach might prove difficult, however, as anticipation effects usually only reliably manifest when action effects can be clearly perceived—a situation that is naturally contrary to the parameters in stimulus detection paradigms.

On a related note, all three of the present experiments as well as the original study of Cardoso-Leite et al. (2010) allow the participants to decide for a particular action in advance (e.g., during an intertrial interval), plan this particular movement and execute it later on. Such a decoupling of decision and action initiation might diminish the effects of sensory attenuation. Studies in the response-effect compatibility paradigm, however, suggest that the impact of anticipated action effects on action control processes prevails even if participants are specifically instructed to prepare their responses ahead of time (Kunde, Koch, & Hoffmann, 2004; Shin & Proctor, 2012; Wirth et al., 2016). Participants in these studies were instructed which key to press by a cue stimulus, but they had to withhold this action for a variable cue-go interval. Response-effect compatibility effects decreased with increasing cue-go interval but, importantly, were still present even for long intervals that allowed for ample preparation time. These findings suggest that a possible decoupling of decision and action initiation might indeed reduce the strength of possible sensory attenuation effects, but they also indicate that sensory attenuation should be detectable even if the majority of participants opted to preselect their responses.

It is noteworthy that beyond the scope of sensory attenuation, the possibility of any top-down process to affect perception—including action-based changes in perceptual accuracy as postulated by the theory of sensory attenuation—has been called into question recently. In their review of articles pertinent to top-down effects on perception, Firestone and Scholl (2016) reported a multitude of methodological shortcomings and subsequent misinterpretations leading to ambiguous results and denying a true test of whether perception can be altered by top-down influences, inspiring a debate that is still ongoing. Our results regarding sensory attenuation in the visual domain agree with their conclusions: in three high-powered experiments, we could not find evidence for an effect of action on the perception of subsequent stimuli but instead we found evidence for the absence of such an effect. Of course, the last word on sensory attenuation for learned visual action effects is not yet spoken and more methodologically sound studies have to be conducted to scrutinize which situations and circumstances might or might not give rise to sensory attenuation in the visual domain. Such studies will also help to elucidate possible commonalities and differences to related areas of research such as action-induced blindness that occurs while planning rather than executing a specific movement (Müsseler & Hommel, 1997; Kunde & Wühr, 2004; Pfister et al., 2012). However, our experiments strongly emphasize that sensory attenuation is not a process that reliably occurs under all circumstances and in all sensory modalities. We therefore believe it prudent to be cautious about

sensory attenuation effects and their potential downstream consequences, behaviorally and physiologically, at least when it comes to learned visual action effects. For example, it seems premature to base such important and high-level concepts as agency partly on effects of a process that usually elicits small—if any—behavioral effects in some, but not all, sensory modalities (especially considering notable differences in occurrence of both phenomena, e.g., Weller et al., 2017).

Conclusions

In this article, we have reported three high-powered experiments designed as direct and conceptual replications of the original study targeting sensory attenuation effects for learned visual action effects (Cardoso-Leite et al., 2010). None of the present experiments could show evidence of sensory attenuation for learned visual action effects, neither in stimulus detection nor in stimulus identification tasks; in fact, our results systematically show evidence for the absence of sensory attenuation across all experiments. Even though these findings do not preclude the possibility that sensory attenuation of learned visual action effects may exist under certain circumstances, it emphasizes that sensory attenuation is not a process that occurs automatically and universally and that evidence for its occurrence in the visual domain is limited at best.

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