Subliminal evaluative conditioning? Above-chance CS identification may be necessary and insufficient for attitude learning

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Data, code, and materials necessary to reproduce the analyses reported in the article are available at https://github.com/methexp/subliminal-EC.

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Abstract

Previous research has claimed that evaluative conditioning (EC) effects may obtain in the absence of perceptual identification of conditioned stimuli (CSs). A recent meta-analysis suggested similar effect sizes for supra- and subliminal CSs, but this was based on a small body of evidence ($k = 8$ studies; Hofmann, De Houwer, Perugini, Baeyens, & Crombez, 2010). We critically discuss this prior evidence, and then report and discuss six experimental studies that investigate EC effects for briefly presented CSs using more stringent methods. Across these studies, we varied CS duration, the presence or absence of masking, the presence or absence of a CS identification check, CS material, and the instructions communicated to participants. EC effects for longer-duration CSs were modulated by attention to the CS-US pairing. Across studies, we were consistently unable to obtain EC for briefly presented CSs. In most studies, this pattern was observed despite above-chance perceptual identification of the CSs. A meta-analysis conducted across the 27 experimental conditions supported the null hypothesis of no EC for perceptually unidentified CSs. We conclude that EC effects for briefly presented and masked CSs are either not robust, are very small, or are limited to specific conditions that remain to be identified (or any combination of these).

Keywords: attitude acquisition, implicit learning, evaluative conditioning, automaticity, awareness

Subliminal evaluative conditioning? Above-chance CS identification may be necessary and insufficient for attitude learning

Evaluative conditioning (EC) is defined as an evaluative change in a generally affectively neutral conditioned stimulus (CS) following its pairing with an unconditioned (positive or negative) stimulus (US). A question that has fueled debates in EC research is whether EC effects may emerge without participants' CS-US contingency awareness. The latter question, in addition to bearing important practical implications (e.g., subliminal advertising), is of paramount theoretical importance for dual models of attitude acquisition, which propose that awareness-independent processes (assumed to be associative in nature) contribute to attitude acquisition in EC procedures in addition to awareness-dependent (e.g., propositional) processes (e.g., Gawronski & Bodenhausen, 2006). Because EC effects obtained in the presence of awareness can be attributed to both types of processes, compelling evidence for a second, awareness-independent, attitude acquisition route requires demonstrating EC effects in the absence of awareness (for recent discussion, see Gawronski & Walther, 2012; Sweldens, Corneille, & Yzerbyt, 2014). Perhaps more secondarily, EC effects obtained in the absence of awareness would allow ruling out experimental demand interpretations of these effects, therefore contributing to their theoretical validity. Finally, the question whether EC occurs in the absence of awareness is also of major theoretical importance for theories of human learning in general, as EC is one of only few examples of implicit learning phenomena for which dissociations have been claimed between learning and awareness (for reviews, see Lovibond & Shanks, 2002; Mitchell, De Houwer, & Lovibond, 2009).

The role of awareness of CS-US pairings in EC may be investigated by different means. Previous work has often focused on using measures of memory for CS-US pairings, collected after the learning phase, as a proxy for awareness at learning. Although the latter memory-based and correlational approach showed important recent developments (e.g., Hütter, Sweldens, Stahl, Unkelbach, & Klauer, 2012), it is not without problems (for a discussion, see Gawronski & Walther, 2012; Sweldens et al., 2014). Another approach consisted of manipulating awareness at encoding by interfering with the explicit encoding of the stimuli (CS or US). In the latter context, the interfering role of attentional load at encoding (Davies, El-Deredy, Zandstra, & Blanchette, 2012; Dedonder, Corneille, Yzerbyt, & Kuppens, 2010; Kattner, 2012; Pleyers, Corneille, & Luminet, 2009) and the use of parafoveal CS-US presentations (Dedonder, Corneille, Bertinchamps, & Yzerbyt, 2013) were examined. Again, however, the latter work faced interpretational limitations (for a recent discussion, see Sweldens et al., 2014). In the present study, we (indirectly) manipulated awareness of CSs by varying CS stimulus strength via mode of presentation.

Determinants of awareness

Consciousness research has identified two major factors – bottom-up stimulus strength and top-down attention – that interact to determine whether a stimulus enters consciousness (Dehaene, Changeux, Naccache, Sackur, & Sergent, 2006): A stimulus may enter consciousness if stimulation is sufficiently strong (i.e., supraliminal), and if the stimulus receives sufficient attention. Sufficiently strong stimuli, when not attended, remain in what has been called a *preconscious* state; they will nevertheless be processed to a certain degree and may affect cognition and action. Weak stimuli that remain below a certain threshold are called *subliminal*. Such stimuli do not enter consciousness. Yet, if attention is directed toward them (e.g., if participants focus their gaze on the critical screen location), subliminal stimuli can affect cognitive processes across a range of experimental paradigms (Van den Bussche, Van den Noortgate, & Reynvoet, 2009).

In the present study, we relied on an objective identification threshold: A stimulus presentation was considered subliminal if the stimulus was not correctly identified by the participant (Merikle, 1982). The use of such an *objective threshold* ensures that the present studies allow for a strict test for EC in the absence of awareness. It also implies that different operationalizations of awareness might yield different results; in particular, the present definition of awareness is not identical to a subjective definition of awareness (Henley, 1982). For instance, it may be the case that, even with above-chance identification performance, stimuli are not consciously experienced (e.g., when identification performance is driven by awareness-independent processes). In this case, stimuli are said to remain below a *subjective threshold* (Cheesman & Merikle, 1986); the question of EC with subjectively subliminal stimuli is, however, beyond the scope of the present study.

In EC, the critical stimulus is the *CS-US pairing*, and awareness of CS-US pairings can be prevented by presenting either the CS or the US stimuli subliminally. In the domain of evaluative learning, both variants of subliminal presentation have been used (Dijksterhuis, 2004; Krosnick, Betz, Jussim, Lynn, & Stephens, 1992). However, the subliminal US and subliminal CS approaches are not equally straightforward for addressing the question of awareness-independent evaluative learning. As a matter of fact, recent empirical evidence challenges the view that subliminal affective stimuli may induce affective reactions (i.e., Lähteenmäki, Hyönä, Koivisto, & Nummenmaa, 2015). As a result, an absence of EC effect in studies making use of subliminal USs may be attributed to a lack of affective reactions to the USs, instead of a lack of learning per se. The latter limitation does not apply when pairing supraliminal USs with subliminal CSs. For the latter type of stimuli, it is sufficient to demonstrate that participants were unable to report or discriminate a given CS. If the CS has been presented subliminally, and has therefore not been consciously perceived, participants were necessarily unaware of the CS-US pairing, despite being aware of the affective value of the (supraliminal) US. If EC can be demonstrated for subliminal CSs, this would be strong evidence for awareness-independent evaluative learning.

Evaluative learning with subliminal CSs

In this section, we first discuss the methodological requirements for demonstrating EC with subliminal CSs and motivate the present choice of methods for investigating subliminal EC. Next, we apply these considerations in a brief discussion of the available evidence.

Methodological considerations. In the present research, we used online, trial-based, and forced-choice assessments of CS identification to probe participants' awareness. This choice was motivated by the following reasons.

As argued by Shanks & St. John (1994), it is important to assess whether participants were aware of the relevant information needed to produce the EC effect. In the present case, the relevant information is the identity of the CS, without which a memory trace for the CS-US pairing (or an equivalent proposition; see Mitchell et al., 2009) cannot be formed. It is equally important that the awareness test is sensitive enough to fully assess this relevant knowledge; recall measures that require participants to reproduce their knowledge are typically less sensitive than recognition measures that require no reproduction (Shanks & St. John, 1994). Furthermore, yes-no recognition tasks are sensitive to effects of decision criteria that may vary across participants and are typically conservative (e.g., Hirshman, 1995); this problem can be avoided by using the forced-choice response format.

Pratte & Rouder (2009) explain why subliminality should be assessed during learning, instead of in a separate subliminality-check task at the end of the study: Whereas the presentation conditions and the sequence of events are typically identical in the main task and the subliminality-check task, participants are no longer asked to perform the main task (e.g., attend to supraliminal target stimuli); instead, in the subliminality-check task, they are asked to attend to and identify the subliminal stimuli. In this approach, however, although stimulus presentation is held constant across the main task and the subliminality-check task, other important aspects of the task may not be constant. For instance, task difficulty is typically much greater in the subliminality-check task than in the main task, as it is much harder to identify subliminal stimuli than it is to identify supraliminal stimuli. As a consequence of this task-difficulty difference, participants may be less motivated – and therefore may be less able to identify subliminal stimuli – in the subliminality-check task as compared to the main task. The ability to identify subliminal stimuli in the main task may therefore be underestimated by performance in the separate subliminality-check task.

In sum, participants' relevant knowledge about CS identity can be detected in a sensitive way by a forced-choice CS identification task in which participants are asked, on each learning trial, to indicate which CS stimulus they have just seen by selecting it from a set of candidate CS stimuli (Atas, Vermeiren, & Cleeremans, 2013; Pratte & Rouder, 2009). If EC effects can be found under conditions under which CS identification, as measured using such a trial-by-trial assessment during learning, does not exceed chance levels, this would be strong evidence for awareness-independent evaluative learning. This is not to suggest that on-line assessments have only advantages relative to off-line assessments of awareness. They may be more interfering, for instance by imposing a larger load on participants during task completion, or by directing participants' attention towards other pieces of information. Yet, online tasks offer a more sensitive test for subliminality, so that any subliminal EC effect found under such online probing conditions would be particularly convincing as it comes to investigating the role of awareness in EC.

Review of subliminal evaluative-learning studies. If the above criteria are required for a compelling examination of EC effects with subliminal CSs, then one is left with no published study that convincingly supported such an effect. As a matter of fact, most of the previously published studies investigating subliminal EC involved one of the following features - and generally combined several of them (for a more detailed discussion, see Lovibond & Shanks, 2002; Pleyers, Corneille, Luminet, & Yzerbyt, 2007; Sweldens et al., 2014): (1) use of subliminal USs, not CSs (e.g., Krosnick et al., 1992; Rydell & McConnell, 2006; Rydell, McConnell, Mackie, & Strain, 2006), (2) use of non-evaluative measures (e.g., motivational measures in Custers & Aarts, 2005; semantic measures in Galli & Gorn, 2011), (3) use of non-associative designs (e.g., incomplete designs or between-manipulations of US valence allowing alternative interpretations in terms of mere exposure or mood effects in Dijksterhuis, 2004; Krosnick et al., 1992; Niedenthal, 1990), (4) absence of sensitive checks for subliminal exposures (e.g., Gawronski & LeBel, 2008).

A comprehensive overview is based on Hofmann and colleagues' meta-analysis about

EC in humans (Hofmann et al., 2010) and the subliminal-EC studies that were addressed in that article. The meta-analytic review failed to find studies with subliminal (i.e., brief and masked) CS presentation that used the common self-report evaluative rating measure as dependent variable. However, a set of $k = 8$ studies was identified that used indirect measures such as affective priming (Fazio, Sanbonmatsu, Powell, & Kardes, 1986) or the IAT (Greenwald, McGhee, & Schwartz, 1998). The mean effect size obtained in this set of studies was of a magnitude comparable to the mean effect size of studies using supraliminal CSs (subliminal: $d = .49, SE = .14, k = 8$; supraliminal: $d = .46, SE = .03, k = 201$; see Hofmann et al., 2010, p. 19f).

The meta-analysis classified studies as subliminal when CS presentation duration was below 50 ms. It is clear that this criterion does not ensure that processing of CSs during learning remained below the awareness threshold for every participant and CS stimulus. For instance, under certain conditions, participants are better than chance at identifying masked stimuli with presentation durations of only 12 ms (Pratte & Rouder, 2009). Although this finding of above-chance identification does not necessarily imply that participants consciously perceived the 12ms stimuli, it leaves open the possibility that some participants may have consciously perceived them on some trials. Such a partially conscious presentation could be sufficient for EC effects to arise from awareness-dependent processes (see Pleyers et al., 2007). To conclude that the “subliminal” EC effect obtained in the meta-analysis does in fact reflect the results of awareness-independent evaluative learning, it is necessary to carefully consider the methods – and especially the results of subliminality checks – of each of the included studies.

A set of eight studies was identified from three articles that met the 50ms criterion (Custers & Aarts, 2005; Dijksterhuis, 2004; Gawronski & LeBel, 2008). Four of the six studies reported by Custers & Aarts (2005) (Expts. 1, 2A, 2B, 2C) were included in the meta-analysis. To check for awareness, the authors reported using an unspecified debriefing interview during which awareness of the subliminally presented words was assessed. From this report, it is unclear whether participants were aware of the CS stimuli during their presentation: A debriefing interview at the end of a study has been argued to lack sensitivity for assessment of awareness. As a matter of fact, the presentation conditions used in these studies were identified as clearly supraliminal in other studies (Pratte & Rouder, 2009). We cannot conclude, therefore, that the findings rely on truly subliminal CS presentations. More generally, we also have concerns about the rationale behind the inclusion of the Custers & Aarts (2005) studies in a review of EC effects. This is because (with the exception of Exp. 2B) non-evaluative measures were used as dependent variables.

The first three of the six studies reported by Dijksterhuis (2004) were also included in the meta-analysis. The reported subliminality check consisted in a debriefing at the end of the experiment in which participants reported not having "seen anything unusual during the lexical decision task", and reported not having seen any words other than the target words flashing on the screen. As argued by Shanks & St. John (1994), the sensitivity of such a debriefing procedure at the end of the experiment may be too low to capture (full or partial) awareness of CSs during CS-US pairing. Given the lack of conclusive evidence for the subliminality of CS presentation in this study, along with evidence that a masked 17ms presentation as realized here was sufficient for above-chance processing at least under some conditions (e.g., Pratte & Rouder, 2009), it is unclear whether the results reflect awareness-independent processes. Even more critically, due to problems in the designs used, it is unclear whether these findings reflect *learning* processes; the findings obtained in these studies may reflect mood effects (see Pleyers et al., 2007).

Finally, Experiment 2 in Gawronski & LeBel (2008) was included in the meta-analysis. This study, however, reports no debriefing results nor any other evidence regarding the subliminality of CS presentations. Thus, it is unclear whether and to which degree the CSs were consciously visible to participants.

Although not covered by Hofmann et al. (2010)´s meta-analysis, we deem it relevant to discuss one final study. Specifically, Verwijmeren, Karremans, Stroebe, & Wigboldus

(2012) (Study 2) conditioned briefly presented (17 ms) and post-masked beverage brand CSs with faces expressing either fear or disgust. Among participants who reported being thirsty prior to the learning phase, disgusted (goal-relevant) but not fearful (goal-irrelevant) faces decreased choices for the conditioned brand. This study involved an identification task, administered after the learning phase, to assess the subliminality of the brand (CS) presentations. In addition, effects on choices remained significant after removing participants who showed some awareness of the face-brand pairings. This study may be interpreted as bringing strong evidence for the possibility of an EC effect with subliminal CS presentations. Four comments are in order, though. First, effects were obtained on the choice measure, but the effect on the evaluative measure was not statistically significant. It thus seems that this study speaks more to goal pursuit (i.e., the wanting system) than to evaluative conditioning (i.e., the liking system), although a conceptualization in terms of wanting would hardly explain effects of negative goal conditioning/shaping (Custers & Aarts, 2010). Second, because both facial-expression USs were negative, the observed effects were apparently driven by the semantic content of the US face (i.e., disgust vs. fear), not its valence. Third, Stahl, Unkelbach, & Corneille (2009) showed that awareness checks tackling US valence have to be favored (because of their higher sensitivity) over measures of the US identity. Again, this requirement could not be met in that study. Fourth, a stronger test of awareness would have required an on-line assessment of participants´ identification of the pairings during learning (rather than an off-line assessment in a different study phase after the learning phase).[1]

In sum, this brief review suggests that when applying stringent qualification criteria for subliminal CS studies, this leave only questionable evidence for the existence of such EC effects. The present study aimed at generating more conclusive evidence to address the question whether EC can obtain in the absence of conscious CS identification during learning.

[1]In an (unpublished) pre-registered replication study in our lab that implemented such an on-line identification task, we found evidence for awareness of CSs during the learning phase under similar stimulation conditions as were realized in the original study.

The present study

We investigated whether EC can be found when CSs are presented only briefly (i.e., for 30 ms) and masked, and whether EC effects under these conditions are comparable in magnitude to EC effects obtained with clearly visible CSs. In terms of the two factors that co-determine conscious perception, attention directed to the CS was held constant, whereas stimulus strength was manipulated (i.e., by varying either presentation duration or the presence versus absence of a post-mask). Furthermore, we aimed at investigating the possibility of subliminal EC under a wide variety of conditions. Towards this end, we manipulated the amount of attention directed toward the CS-US pairing (i.e., by administering different orienting instructions during learning). Thus, the main goal was to experimentally manipulate CS identifiability and to investigate, under a range of different incidental orienting conditions, whether EC can still be detected when CS identification is at (or slightly above) chance.

We sought to implement a strict and fair test of the hypothesis of EC for subliminal CSs. A strict test of subliminal EC requires a sensitive detection of CS identification. The stricter the subliminality check, the stronger the evidence for subliminal EC, if it can be found.

Sensitive assessment of subliminality. To be able to provide strong evidence for subliminal EC, we implemented a strict awareness check: We tested awareness of CS presentations using an identification task in which participants were asked, on every trial, to select the just-presented CS from a list of candidate CSs. To avoid methodological problems limiting previous research, we presented each participant with CSs under both clearly supraliminal and near-liminal conditions, and we administered an identification task after each presentation trial during the learning phase. This avoids potential motivational task-difficulty confounds (Pratte & Rouder, 2009) in the identification task because participants successfully identified the CS on at least half of the trials. In addition, because CS identification is tested on an item-by-item basis immediately after each presentation, we avoid confounds due to learning effects that increase identification of subliminal CS after repeated presentations (Atas et al., 2013).

Assessing CS identification on each trial allowed us to separate those CSs that were sometimes identified from those that were never identified correctly (a parallel approach has recently been developed by Van den Bussche et al., 2013). Subliminality, as well as EC effects, are typically assessed on a coarse level (i.e., aggregated across persons and items). The present trial-by-trial and CS-by-CS identification data would allow us to further dissect aggregated EC effects. Specifically, if we were to obtain EC for briefly presented and masked CSs, we could check for a given participant whether this effect was limited to those CSs associated with correct identification (and could therefore possibly be due to conscious processes), or whether it extended to CSs that were never correctly identified and therefore could not be explained as the result of conscious processing.[2]

Experimental conditions supportive of awareness-independent EC effects. For a fair test, we aimed at CS identification levels that were suboptimal but still, on average, slightly above chance, so as to realize conditions that would in principle allow for subliminal processing. That is, we sought to realize conditions for which there was evidence for at least partially successful processing of at least some the CSs. If EC can be obtained at all under conditions of reduced or absent awareness, such effects therefore had a fair chance of manifesting themselves in the present study.

Investigating subliminal EC under a variety of incidental learning conditions. Evaluative learning may occur under intentional as well as incidental learning conditions, and different learning processes may operate under both conditions. For instance, awareness-independent EC effects may operate only under incidental learning conditions (e.g., Olson & Fazio, 2001), whereas awareness-dependent EC may be found also under intentional learning conditions (Kattner, 2012). This is suggested by the fact that EC effects

[2]Note that — in contrast to the experimental manipulation of awareness that is in the focus of the present study — this classification of CSs by identification performance is post-hoc, and analyses involving this classification are merely correlational.

have repeatedly been obtained in the absence of memory for CS-US pairings in the incidental surveillance paradigm (Olson & Fazio, 2001), and the authors interpret these findings as the result of an implicit-misattribution process by which US valence is misattributed to the CS (Jones, Fazio, & Olson, 2009). As EC in the absence of awareness appears to be more likely under incidental encoding conditions, we implemented such incidental learning conditions in the present study. Importantly, all of these learning conditions were conceptually very similar to the surveillance paradigm (Olson & Fazio, 2001) in which participants were asked to attend to the stimuli in order to identify a pre-specified target CS by pressing a key: In the present experiments, participants were also asked to attend to and identify the CS stimuli; the only difference being that they were required to select the identified stimulus from a list of options.

The incidental orienting tasks realized in the literature differed in the amount of attention directed toward the critical information, that is, the valence of the USs associated with a given CS (e.g.; Gast & Rothermund, 2011b; Olson & Fazio, 2001). We therefore implemented different incidental orienting tasks as follows: Across experiments, we varied the attention directed toward the CS-US pair and the valence dimension in several steps (see Table 1 for an overview). In a first step, attention was maximally directed to the CS-US pair and the valence dimension in a valence-focus task (Experiments 2 & 3) in which participants were asked to attend to the CS-US stimulus pair and to evaluate their affective response elicited by the stimulus pair (Gast & Rothermund, 2011b). In a second step, the brightness-focus task asked participants to attend to and judge the CS-US stimulus pair, but the required judgment was on the perception of brightness, so that participants were no longer required to attend to the valence dimension (Experiments 3 & 4). In a third step, participants were no longer instructed to attend to the CS-US pair; instead, the stimulus identification task required them to attend to the individual CS and US stimuli (i.e., because they were asked only at the end of each trial which CS or US had been presented during the trial; Experiment 6). Finally, a fourth step removed the requirement to attend to the USs as participants were asked only to identify the CSs, whereas the USs were introduced as background pictures that participants were free to ignore (Experiments 1 & 5).

Effect size and statistical power. To be able to reliably detect EC effects with subliminal CS presentation, using a one-tailed t-test, we set $\alpha = \beta = .05$ and realized sample sizes of $N \geq 50$ that were sufficient for detecting medium effect sizes as reported in the meta-analysis for indirect measures (i.e., $0.46 < d < 0.49$) with high statistical power (i.e., $1 - \beta \geq .95$).[3] The present studies were therefore adequately powered to detect the effects of interest. In addition, we report the results of a meta-analysis across all six studies to estimate the magnitude of EC effects under the different presentation conditions. This allowed us to investigate whether EC effects with subliminal CSs are indeed of the magnitude reported in previous studies, or whether they are smaller (or even non-existent) when strict subliminality checks are used.

Overview of Experiments

In six experiments, briefly presented and masked CSs were repeatedly paired with clearly visible IAPS pictures of either positive or negative valence. We varied (1) the orienting task participants performed during the learning phase, (2) the presence versus absence of a response requirement in the orienting task, (3) the presence versus absence of an online subliminality check (i.e., CS identification task), (4) the stimulus strength manipulation (presentation duration vs. masking), and (5) the CS materials (nonwords, faces, product images).

In a first step, we realized a high-power attempt at obtaining a subliminal EC effect in an established paradigm, and tested whether the CS identification task that is central to our approach but has not been used in EC research so far interferes with evaluative conditioning: In Experiment 1 we investigated EC for briefly presented and masked CSs in an incidental paradigm parallel to our previous work (Stahl et al., 2009) in which we reliably obtained EC

[3]We assumed a correlation between measurements of $r = .5$, which is plausible given previous findings, and for which the effect size for dependent measures, d_z, is equal to the commonly used Cohen's d.

effects. In Experiment 2 we manipulated whether participants were asked to identify the CS on every trial, or whether they were merely asked to attend to the CS (without ever performing an identification task), and investigated whether EC effects were disrupted by the identification task.

Experiments 3 and 4 then focused on the role of the orienting task: We varied the amount of attention directed toward the critical valence dimension by comparing an orienting task that involved a valence judgment of the CS-US pair to an orienting task requiring a brightness judgment (Experiment 3). We also varied the response requirement in the orienting task: In the brightness judgment task, participants were either required to indicate their brightness judgments on every trial, or they were never asked to indicate their brightness judgments (Experiment 4).

Experiments 5 and 6 attempt to generalize the findings to other stimuli and presentation conditions. We attempted to establish subliminal EC effects for briefly presented and masked CSs by using faces and consumer products as CS materials (Experiment 5), and by realizing a close approximation to Olson & Fazio (2001)'s surveillance paradigm (Experiment 6). Table 2 gives an overview of the experiments. To foreshadow, contrary to the current view that EC effects may emerge independent from awareness, we did not find any evidence for EC with briefly presented and masked CSs.

Experiment 1

In Experiment 1, nonword CSs were presented for either 30 or 100 ms and paired with multiple USs of the same valence, which were presented as background images during the pairing phase. After the pairing phase, participants evaluated the CSs on rating scales. Participants were asked to pay attention to the briefly presented stimuli in the center of the screen, and to try to identify them. Based on the results of a pretest, we expected CS identification to be slightly above chance in the short (30ms masked) presentation condition.

In addition to the non-words, we also used words of the German language as filler CSs.

This was done to further reduce the overall difficulty of the task of identifying the briefly flashed stimuli in order to keep up participants' motivation throughout the experiment. The word data are not reported as they are irrelevant for our hypotheses.

Method

Participants and design. A sample of $N = 131$ University of Cologne students from different majors completed the experiment in exchange for either a monetary compensation or partial course credit. Sample size was sufficient to detect small-to-medium effects ($d = .32$) with a power of $1 - \beta = .95$. We realized a 2 (US valence: positive vs. negative) x 2 (CS duration: 30 vs. 100 ms) within-participants design.

Materials. As CSs, we used 24 nonwords (taken from Brendl, Markman, & Messner, 2001; Stahl et al., 2009). In addition, 24 words were used as filler stimuli, half being of positive and half of negative valence (taken from Klauer, Eder, Greenwald, & Abrams, 2007). As USs, we used 25 positive ($M = 7.94$) and 25 negative ($M = 2.50$) IAPS pictures (Lang, Bradley, & Cuthbert, 2008). A list of IAPS stimuli is given in the Appendix.

Each CS stimulus (nonwords and filler words) was paired with 5 different US images of the same valence. Half of each of the sets of CS stimuli (24 nonwords, 12 positive words, 12 negative words) was paired with positive USs and the other half with negative USs; half of each of these sets of pairings was presented in the 100 ms condition, and the other half was presented in the 30 ms condition. Assignment of CS and US stimuli to experimental conditions was randomized for each participant anew. Forward and backward masks were generated randomly from the set of consonants for each presentation trial anew.

Procedure. The study was administered on a personal computer controlled by software written in C. Participants were seated in a cubicle and instructed that they would see words and meaningless letter strings (nonwords) very briefly, and that they should try to identify them. They were told that the task is difficult and requires concentration, and that they would not always be able to identify the word or nonword; in such cases, they were instructed to guess.

We realized simultaneous (i.e., temporally overlapping) presentations of CS and US. A trial proceeded as follows: First, the US picture was presented for 1500ms, covering the entire screen. Then a small grey rectangle was presented centrally for 500 ms as an attentional cue and replaced by the forward mask, which was presented for another 500 ms and replaced by the CS. CSs were presented for either 30 ms or 100 ms and replaced by a backward mask, which was presented either for 1400 ms (100 ms condition) or 1470 ms (30 ms condition), after which the screen was cleared. The US remained on screen during the entire CS sequence. Total trial duration was 4s. In this and the following studies, US valence as well as CS duration varied on a trial-by-trial basis, and trial order was determined randomly for each participant anew.

Following each CS-US presentation, a list of the 24 words and 24 nonwords was presented on the screen. Participants were instructed to use the computer mouse to click on the word or nonword they had just seen, and to guess if they had not seen anything. The response was followed by an inter-trial interval of 1000 ms before the next trial started. In total, 5 blocks of 48 trials were administered, such that each CS was presented five times (i.e., once with each of five different US images of the same valence).

After the presentation phase, evaluative ratings were collected for all 48 items. Participants were asked to indicate overall impression, attractiveness, and pleasantness on 8-point rating scales, with higher values reflecting more positive evaluations. Upon completing the evaluative ratings, participants were debriefed, received their compensation, and were dismissed.

Results

Evaluative ratings were highly correlated ($0.83 < r < 0.86$), and an exploratory factor analysis yielded a single factor which explained 90 % of the variance. Thus, in the studies in which three evaluative ratings were collected for each CS (i.e., Experiments 1-4), we used the mean of the three ratings as the dependent variable.[4]

CS identification. Participants' mean proportions of correct CS identifications were analyzed in a repeated-measures ANOVA with CS duration (30 vs. 100ms) as the only factor. Mean CS identification was affected by CS presentation duration, $F(1, 130) = 1182.18$, $MSE = 0.03$, $p < .001$, $\eta^2_G = .785$: As illustrated in Figure 1, masked CSs presented for 30ms were identified less often ($M = 0.07$, $SD = 0.25$) than those presented for 100ms ($M = 0.78$, $SD = 0.41$). Identification of masked CSs was better than chance (i.e., chance level: $1/48 = 0.02$), $M = 0.07$, 95% CI $[0.06, 0.08]$, $t(130) = 9.93$, $p < .001$.

Evaluative conditioning. Evaluative ratings were analyzed in a repeated-measures ANOVA with factors CS duration (30 vs. 100ms) and US valence (positive vs. negative).

We obtained a main effect of presentation duration, $F(1, 130) = 18.66$, $MSE = 0.66$, $p < .001$, $\eta^2_G = .020$, but not of US valence, $F(1, 130) = 0.00$, $MSE = 0.36$, $p = .987$, $\eta^2_G = .000$, nor did we obtain an interaction of the two, $F(1, 130) = 2.28$, $MSE = 0.32$, $p = .133$, $\eta^2_G = .001$. The main effect of presentation duration reflected the finding that CSs presented for 100ms were evaluated more positively ($M = 4.27$, $SD = 1.72$) than CSs presented for 30 ms ($M = 3.97$, $SD = 1.60$). As depicted in Figure 1, there was no EC effect for nonword CSs presented for 30ms, $M_d = 0.08$, 95% CI $[-0.05, 0.20]$, $t(130) = 1.16$, $p = .247$, nor for CSs presented for 100 ms, $M_d = -0.07$, 95% CI $[-0.23, 0.08]$, $t(130) = -0.93$, $p = .354$.

[4]We used R (3.2.0, R Core Team, 2015a) and the R-packages *BayesFactor* (0.9.12.2, Morey & Rouder, 2015), *coda* (0.17.1, Plummer, Best, Cowles, & Vines, 2006), *data.table* (1.9.6, Dowle, Srinivasan, Short, R Saporta, & Antonyan, 2015), *ez* (4.3, Lawrence, 2015), *Formula* (1.2.1, Zeileis & Croissant, 2010), *ggplot2* (2.1.0, Wickham, 2009), *grid* (3.2.0, R Core Team, 2015b), *gridExtra* (2.2.1, Auguie, 2016), *HistData* (0.7.8, Friendly, 2015), *Hmisc* (3.17.3, Jr, Charles Dupont, & others., 2016), *lattice* (0.20.31, Sarkar, 2008), *lsr* (0.5, Navarro, 2015), *MASS* (7.3.40, Venables & Ripley, 2002), *Matrix* (1.2.0, Bates & Maechler, 2015), *meta* (4.3.2, Schwarzer, 2015), *metafor* (1.9.7, Viechtbauer, 2010), *papaja* (0.1.0.9074, Aust & Barth, 2015), *plotrix* (3.5.12, J, 2006), *plyr* (1.8.2, Wickham, 2011), *reshape2* (1.4.1, Wickham, 2007), *Rmisc* (1.5, Hope, 2013), and *UsingR* (2.0.5, Verzani, 2015) for analyses and reproducible reporting.

Discussion

First, results showed that participants could identify nonwords above chance level even when they were presented for only 30 ms and masked. Although identification performance was low, it was clearly above chance, indicating that some CSs could be (at least) partially identified. Previous work has shown that even clearly subliminal stimuli were able to affect cognitive processes (Van den Bussche et al., 2009), indicating that, under the present slightly supraliminal presentation conditions, awareness-independent processes should be able to operate on the CSs. A necessary condition for EC to occur was therefore clearly met.

Experiment 1 nevertheless failed to yield an EC effect: Participants' evaluation of the CSs did not vary as a function of US valence, even for clearly visible CSs. This suggests that EC is less robust than often assumed: EC was not obtained, despite the fact that the experimental paradigm was highly similar to that realized in our earlier studies in which we robustly obtained EC (e.g., Stahl et al., 2009). In sum, CS identification appears to be necessary but not sufficient for EC.

We identify three potential causes for the lack of an EC effect. First, we suspected that the instructions' focus on the CS identification task (e.g., instructions referred to identification of CSs as the main task; USs were characterized as background pictures) may have interfered with a more holistic default processing mode that is thought to be conducive to EC, or may simply have led to insufficient attention to the USs. Second, as participants were asked to evaluate both words and valenced nonwords after the learning phase using the same rating scale, the presence of words might have restricted the range of nonword evaluations, thereby masking potential EC effects. Third, the requirement to work on the CS identification task during the learning phase may have interfered with evaluative learning.

Experiment 2

In Experiment 2, we modified three features of the procedure that may have been detrimental to EC. First, to increase the amount of attention paid to the US, we explored whether EC for 30ms masked stimuli can be found when a valence focus is induced, as EC effects may depend on an attentional focus on valence during learning (Gast & Rothermund, 2011b). In the valence-focus orienting task, USs were no longer described as background pictures but were to be attended together with the CSs, and participants were instructed, on each trial, to evaluate the pleasantness of the stimulus pair. Second, words were no longer used as filler CSs. Third, and most importantly, to investigate whether the absence of EC in Experiment 1 was due to interference from the CS identification task, we manipulated the presence versus absence of that task. Thus, the CS identification task served as a manipulation in this experiment, not as manipulation check: If it interfered with EC, we should observe an EC effect for 30ms masked stimuli only for participants who did not perform this task during the learning phase. Finally, Experiment 2 also used a different stimulus-strength manipulation: Instead of manipulating presentation duration, we kept duration constant at 30 ms and manipulated the presence versus absence of the forward and backward masks.

Method

Participants and design. A total of $N = 62$ students who had not participated in any of the other studies were recruited and received either a monetary compensation or partial course credit. We implemented a 2 (US valence: positive vs. negative) x 2 (CS masking: present vs. absent) x 2 (CS identification task: present vs. absent) design, with repeated measures on the first two factors. Half of participants were randomly assigned to the CS-identification condition, the other half did not perform the CS identification task.

Materials. In Experiment 2, the same 24 nonwords as in Experiment 1 were used as CS (the words were no longer used as filler CSs because identification was clearly above chance in the 30ms condition). Each CS was paired with 5 US images of the same valence. Half of the CS stimuli was paired with positive USs and the other half with negative USs; half of each of these sets of pairings was presented in the mask-present condition, and the other half was presented in the mask-absent condition. Forward and backward masks were generated randomly from the set of consonants for each trial anew.

Procedure. Participants were told that they would see pictures and words from an unfamiliar language. They were told that words would be presented very briefly, and that they would be hidden by random letter strings on some occasions. They were instructed to attend to pictures and words and told that we were interested in their overall impression of both. As an orienting task, we induced a valence focus: After each pairing, participants were asked to indicate whether they had a pleasant or an unpleasant impression of the picture-nonword pair.

To test whether the CS identification task during learning affected EC, it was administered only to one half of participants. These participants were asked to identify the nonword they had just seen by selecting it from a list of all 24 nonwords. For the other half of participants, CS identification was never tested.

Trials were similar to Experiment 1, with the exception that presentation duration was constant at 30ms (and the duration of the backward mask was thus constant at 1470ms in the masked condition). Instead of presentation duration, the presence (vs. absence) of forward and backward masks was varied. After each trial, the valence task was presented; in the CS-identification group, it was followed by the CS identification task. After an inter-trial interval of 1000ms, the next trial started. In total, 10 blocks of 24 trials were administered (i.e., each CS-US pair was presented twice).[5] After the presentation phase, evaluative ratings were collected for the 24 nonwords as in Experiment 1.[6]

[5]Due to a programming error, identification performance in the first five blocks was lost. The remaining data from the second half of the learning phase are therefore a somewhat noisy estimate of overall CS identification performance. They may also represent a biased estimate, if CS identification increases over time due to learning, or if it decreases over time due to fatigue or decreasing motivation. However, we consider such a bias unlikely because the number of presentations (6 or 12) did not affect CS identification performance in Experiment 6 (see below).

[6]Evaluative ratings were highly correlated ($0.78 < r < 0.84$), and an explorative factor analysis yielded a single factor which explained 87.79% of the variance.

Results

CS identification. Using the data from the CS-identification group, the mean proportion of correctly identified CSs was analyzed in a repeated-measures ANOVA with masking (mask present vs. absent) as the only factor. The results are illustrated in Figure 1. Mean CS identification was affected by the mask, $F(1, 30) = 3014.99$, $MSE = 0.00$, $p < .001$, $\eta^2_G = .982$: masked items were identified less often ($M = 0.05, SD = 0.23$) than unmasked items ($M = 0.95, SD = 0.22$). Identification of masked CSs was descriptively but only marginally better than chance (chance level: $1/24 = 0.04$), $M = 0.05$, 95% CI [0.04, 0.07], $t(30) = 1.82$, $p = .079$.

Evaluative conditioning. Evaluative ratings were analyzed in an ANOVA with CS-identification group as between-participants factor, and with masking and US valence as repeated-measures factors. The between-participants CS-identification factor was not significant, $F(1, 60) = 0.01$, $MSE = 2.58$, $p = .927$, $\eta^2_G = .000$, and did not enter any significant interactions. We obtained main effects of masking, $F(1, 60) = 23.37$, $MSE = 0.46$, $p < .001$, $\eta^2_G = .036$, and of US valence, $F(1, 60) = 29.99$, $MSE = 0.90$, $p < .001$, $\eta^2_G = .086$, as well as an interaction between them, $F(1, 60) = 23.82$, $MSE = 0.82$, $p < .001$, $\eta^2_G = .064$.

The main effect of masking reflects the finding that masked CSs were rated less positively, $M = 4.27$, $SD = 1.58$, than nonmasked CSs, $M = 4.69$, $SD = 1.98$. The main effect of US valence (i.e., the EC effect, reflecting more positive ratings for CSs paired with positive than with negative USs) was qualified by the interaction with masking: An EC effect was obtained for unmasked CSs, $F(1, 60) = 31.98$, $MSE = 1.44$, $p < .001$, $\eta^2_G = .197$, but not for masked CSs, $F(1, 60) = 1.10$, $MSE = 0.27$, $p = .299$, $\eta^2_G = .003$ (see Figure 1).

Discussion

In this experiment, CS identification was high for unmasked CSs and was slightly but not significantly above chance for masked CSs (note here that the power of the CS identification test was lower than that of the test for EC, as only half of the participants entered the former analysis). A robust EC effect was obtained for unmasked and clearly identifiable CSs; this finding support the view that inducing a valence focus promotes EC for identifiable CSs. In contrast, there was no evidence for EC in the masked condition. Hence, under valence-focus instructions EC appears to vanish when reaching the boundaries of conscious perception. Perhaps most importantly, this study revealed that the CS identification task did not affect EC: both groups had comparable levels of EC for unmasked CSs.

Experiment 3

Experiment 3 realizes another attempt at demonstrating EC for briefly presented and masked CSs, and to test whether such an EC effect is modulated by processing goals or, put differently, the attentional requirements of the orienting task. That an EC effect was obtained under valence-focus instructions in Experiment 2 but was not obtained under CS-identification instructions in Experiment 1 is consistent with the view that EC depends on the goal of processing US valence (Corneille, Yzerbyt, Pleyers, & Mussweiler, 2009; Gast & Rothermund, 2011b). We examined the role of processing goals in EC by directly manipulating this factor: Experiment 3 compared the valence-focus instruction to a brightness-judgment orienting task. In the brightness-focus condition, we eliminated the valence-processing requirement of the orienting task while maintaining the requirement that attention be directed toward the CS and US as a pair.

Method

Participants and design. For Experiment 3, $N = 57$ students who had not participated in any of the other studies were recruited; they received either a monetary compensation or partial course credit. We implemented a 2 (US valence: positive vs. negative) x 2 (CS masking: present vs. absent) x 2 (orienting task: valence vs. brightness) design, with repeated measures on the first two factors. Half of participants were randomly assigned to the valence orienting task, the other half performed the brightness orienting task.

Materials and procedure. The same materials were used as in Experiment 2.

Procedure was largely identical to that used in Experiment 2, with the following exceptions: Participants were instructed to attend to the picture-word pair and told that we were interested in their perceptual impression. Half of participants performed the valence-focus task (i.e., after each pairing, they were asked to indicate whether they had a pleasant or an unpleasant impression of the stimulus pair). The other half performed a brightness task: They were asked to indicate whether their perceptual impression of the pair was better described as "bright" or as "dark". After the perceptual-impression orienting task, all participants were asked, on every trial, to identify the nonword they had seen by selecting it from a list of all 24 nonwords.[7]

Results

CS identification. Mean proportion of correct CS identifications was analyzed in a masking (mask present vs. absent) by orienting task (valence vs. brightness) ANOVA with repeated measures on the first factor. Mean CS identification was affected by the mask, $F(1, 55) = 2768.75$, $MSE = 0.01$, $p < .001$, $\eta^2_G = .951$, but neither by orienting task, $F(1, 55) = 2.08$, $MSE = 0.01$, $p = .155$, $\eta^2_G = .023$, nor the interaction, $F(1, 55) = 0.04$, $MSE = 0.01$, $p = .847$, $\eta^2_G = .000$. The main effect of masking is illustrated in Figure 1; it reflected the fact that masked CSs were identified less often ($M = 0.09, SD = 0.29$) than non-masked CSs ($M = 0.93, SD = 0.25$). In both groups, identification of masked CSs was above chance; valence-focus: $M = 0.08$, 95% CI [0.06, 0.09], $t(27) = 5.19$, $p < .001$; brightness-focus: $M = 0.10$, 95% CI [0.07, 0.13], $t(28) = 4.26$, $p < .001$.

Evaluative conditioning. Evaluative ratings were analyzed in an ANOVA with orienting task as between-participants factor, and with masking as well as US valence as repeated-measures factors. The between-participants orienting-task factor was not significant and did not enter any interactions, $F \leq 1.02$. We obtained main effects of masking,

[7] Evaluative ratings were highly correlated ($0.81 < r < 0.83$), and an exploratory factor analysis yielded a single factor which explained 88.09 % of the variance.

$F(1,55) = 16.10$, $MSE = 0.88$, $p < .001$, $\eta^2_G = .048$, and of US valence, $F(1,55) = 13.48$, $MSE = 0.82$, $p = .001$, $\eta^2_G = .038$, as well as an interaction between them, $F(1,55) = 12.33$, $MSE = 0.70$, $p = .001$, $\eta^2_G = .030$.

The main effect of masking again reflected a preference for unmasked CSs ($M = 4.53, SD = 1.97$) over masked CSs ($M = 4.03, SD = 1.68$). The main effect of US valence (i.e., the EC effect, reflecting more positive ratings for CSs paired with positive than with negative USs) was qualified by the interaction with masking: As shown in Figure 1, an EC effect was obtained for unmasked CSs, $F(1,55) = 15.56$, $MSE = 1.26$, $p < .001$, $\eta^2_G = .099$, but not for masked CSs, $F(1,55) = 0.28$, $MSE = 0.26$, $p = .597$, $\eta^2_G = .001$. The lack of an EC effect for masked CSs also holds when analyzed separately for the orientation task groups; valence: $F(1,27) = 0.12$, $MSE = 0.29$, $p = .732$, $\eta^2_G = .001$; brightness: $F(1,28) = 1.36$, $MSE = 0.23$, $p = .253$, $\eta^2_G = .006$.

Discussion

The brightness task was successful in inducing an EC effect for unmasked CSs. We replicated the finding that EC does not occur under conditions of reduced CS identification: Whereas EC was observed for unmasked stimuli, we did not find EC for masked stimuli. The type of learning instruction did not matter: Identical patterns were obtained under both orientation conditions, suggesting that attention to the valence dimension is not required for EC to obtain. This finding is consistent with previous findings suggesting that stimulus valence may be processed spontaneously (Olson, Kendrick, & Fazio, 2009).

Experiment 4

In Experiment 4, we attempted to further generalize the findings to the stimulus-strength manipulation used in Experiment 1, that is, by manipulating presentation duration instead of masking. The CSs were presented either for 30, 50, or 100 ms, and all CSs were masked.

We also attempted to reduce the orienting task's potential for interference with EC: By manipulating the presence versus absence of a response requirement in the orienting task, we investigated whether requiring an orienting response during learning disrupts EC in the present paradigm. All participants were instructed to form an impression of the brightness of the stimuli, with one half reporting their impression after each trial, and the other half never being asked to report their impression.

Method

Participants and design. For this study, $N = 52$ participants were recruited from the same population who had not taken part in any of the other studies reported herein; participation was compensated by a small monetary amount or partial course credit. We implemented a 2 (US valence: positive vs. negative) x 3 (CS duration: 30ms, 50ms, 100ms) x 2 (orienting response: present vs. absent) design with repeated measures on the first two factors. Half of participants were randomly assigned to the orienting-response condition, the other half did not give responses in the brightness orienting task.

Materials and procedure. The same nonwords (CSs) and IAPS pictures (USs) were used as in Experiments 1-3.

The same procedure was used as in Experiment 3, with the following exceptions: First, all participants performed the brightness task, whereas only one half gave a brightness judgment after each trial, while the other half was instructed to perform but not to report any brightness judgments. Second, CS stimuli were always masked. As in Experiment 3, the CS identification task was administered to all participants. Breaks were introduced after each block of 40 trials to allow participants to rest and to remind them of the brightness task.[8]

Results

[8] Evaluative ratings were highly correlated ($0.75 < r < 0.78$), and an exploratory factor analysis yielded a single factor which explained 84.61 % of the variance.

CS identification. Participants' mean proportion of correctly identified CSs was analyzed in a CS duration (30, 50, 100 ms) by orienting response (present vs. absent) ANOVA with repeated measures on the first factor. Results are depicted in Figure 2. Mean CS identification was affected by duration, $F(2, 100) = 487.43$, $MSE = 0.02$, $p < .001$, $\eta^2_G = .808$, but not by orienting response, $F(1, 50) = 0.17$, $MSE = 0.04$, $p = .683$, $\eta^2_G = .002$, nor their interaction, $F(2, 100) = 0.02$, $MSE = 0.02$, $p = .977$, $\eta^2_G = .000$. Masked CSs presented for 30ms were identified in less than one out of ten trials ($M = 0.08, SD = 0.27$), reflecting above-chance (i.e., $1/24 = 0.04$) performance in both the response-present and response-absent groups, $t(24) = 3.69$, $p = .001$, and $t(26) = 5.13$, $p < .001$, respectively. The CSs presented for 50ms were identified in one out of four trials ($M = 0.24, SD = 0.43$), and those presented for 100 ms were identified in approximately four out of five trials ($M = 0.83, SD = 0.38$).

Evaluative conditioning. Evaluative ratings were analyzed in an ANOVA with orienting response as between-participants factor, and with CS duration as well as US valence as repeated-measures factors. We obtained a main effect of CS duration, $F(2, 100) = 25.04$, $MSE = 1.39$, $p < .001$, $\eta^2_G = .144$, a non-significant main effect of US valence, $F(1, 50) = 3.70$, $MSE = 0.92$, $p = .060$, $\eta^2_G = .008$, as well as their significant interaction, $F(2, 100) = 4.41$, $MSE = 0.61$, $p = .015$, $\eta^2_G = .013$. The presence versus absence of an orienting response did not affect ratings, $F(1, 50) = 2.05$, $MSE = 3.33$, $p = .158$, $\eta^2_G = .016$, nor did it interact with US valence, $F(1, 50) = 2.80$, $MSE = 0.92$, $p = .100$, $\eta^2_G = .006$, or CS duration, $F(2, 100) = 0.78$, $MSE = 1.39$, $p = .459$, $\eta^2_G = .005$; the three-way interaction was also not significant, $F(2, 100) = 0.05$, $MSE = 0.61$, $p = .953$, $\eta^2_G = .000$.

The main effect of CS duration reflected the finding that CSs presented for 30 and 50 ms ($M = 4.28, SD = 1.69$, and $M = 4.60, SD = 1.72$, respectively) were rated less positively than CSs presented for 100 ms ($M = 5.40, SD = 1.78$). The main effect of US valence (i.e., the EC effect) was qualified by the interaction with CS duration: As can be seen in Figure 2, an EC effect was obtained only for CSs presented for 100 ms, $F(1, 50) = 8.76$, $MSE = 0.99$,

$p = .005$, $\eta^2_G = .055$, but not for CSs presented for 30 ms, $F(1, 50) = 0.00$, $MSE = 0.53$, $p = .982$, $\eta^2_G = .000$, or 50 ms, $F(1, 50) = 0.12$, $MSE = 0.62$, $p = .733$, $\eta^2_G = .001$.

Discussion

We again obtained EC effects only for clearly identifiable CSs but failed to find EC for briefly presented and unidentified CSs. This extends our previous findings, which relied on manipulations of the presence versus absence of a mask, to a stimulus-strength manipulation of CS duration when all stimuli were masked. In contrast to the notion that a response in the orienting task interferes with EC, we found no effect of manipulating the presence versus absence of the response requirement in the orientation task on CS identification or EC. Given this finding, it seems unlikely that orienting responses interfered with CS identification or EC effects in the present studies.

Experiment 5

In Experiment 5, we attempted to replicate and extend the previous findings to different stimulus materials. So far, the stimuli used as CSs were taken from the same set of nonwords. Here, we used a different set of nonwords, and we additionally used faces (e.g., Hütter et al., 2012) and product images (e.g., Pleyers et al., 2007) as CSs. We realized the conditions used in Experiment 1 but also included longer presentation conditions that more closely resemble those realized in previous EC research. Participants viewed nonwords, faces, and products either for 30, 100, or 1000 ms; all CSs were masked.

Method

Participants and design. For Experiment 5, $N = 61$ University of Cologne students were recruited in exchange for either a monetary compensation or partial course credit; none of the participants took part in a previous study of this experimental line within the last year. We implemented a 3 (CS duration: 30, 100, 1000 ms) by 3 (CS material:

nonwords, faces, product images) by 2 (US valence: positive vs. negative) repeated-measures design.

Materials. We used a new set of 24 pronouncable nonwords that were generated from entries of the CELEX database by replacing the vowels; 24 faces were taken from previous work (e.g., Hütter et al., 2012); 24 unknown product images were generated from existing but unfamiliar consumer products by erasing brand names and logos. Contrast settings for all stimuli were adjusted and pretested in order to obtain low visibility in the 30ms condition but high visibility in the 100ms condition. The same IAPS pictures (USs) were used as in the previous experiments. Each of the 72 CSs was paired with 4 different USs of the same valence, resulting in a learning phase of 4 blocks of 72 trials each. As masks, we presented four different random checkerboard patterns.

Procedure. Participants were instructed to attend to and try to identify the centrally presented CS stimulus. As in Experiment 1, they did not receive specific instructions regarding the background pictures (i.e., USs). CS material was varied on a trial-by-trial basis (i.e., all trials were presented in a new random order for each participant). All participants performed the CS identification task on all trials: they were to select the CS from a list of 12 options (taken from the same material as the CS on that trial). In contrast to previous experiments, CS stimuli were only post-masked in this study (with post-mask duration determined as 1500 ms minus CS duration; the US, presented in the background, served as a pre-mask), and a single evaluative rating was collected on a continuous scale ranging from -100 to +100 (internally, the scale was coded as ranging from 0 to 200).

Results

CS identification. Mean proportion of correctly identified CSs was analyzed in a CS duration (30, 100, 1000 ms) by CS material (nonword, face, product) repeated-measures ANOVA. Mean CS identification was affected by duration, $F(2, 120) = 2061.73$, $MSE = 0.01$, $p < .001$, $\eta^2_G = .910$, and by material, $F(2, 120) = 18.79$, $MSE = 0.01$, $p < .001$, $\eta^2_G = .034$, as well as their interaction, $F(4, 240) = 15.59$, $MSE = 0.01$, $p < .001$, $\eta^2_G = .055$.

The main effect of CS duration is illustrated in Figure 2; it reflects the finding that the CSs presented for 30ms were identified less frequently ($M = 0.25, SD = 0.43$) than those presented for 100ms ($M = 0.89, SD = 0.32$) and those presented for 1000 ms ($M = 0.99, SD = 0.12$). The main effect of material reflects the fact that faces ($M = 0.73, SD = 0.44$) were better identified than the other two materials (nonwords: $M = 0.69, SD = 0.46$, products: $M = 0.70, SD = 0.46$). The interaction reflected the finding that this face advantage was especially prominent in the 30ms condition (faces: $M = 0.32$, nonwords: $M = 0.23$, products: $M = 0.20$), and attenuated in the 100ms (faces: $M = 0.91$, nonwords: $M = 0.85$, products: $M = 0.90$) and 1000ms conditions (faces: $M = 0.98$, nonwords: $M = 0.99$, products: $M = 0.98$).

CS identification was above chance (i.e., $1/12 = 0.08$) even in the 30ms condition, $M = 0.25$, 95% CI [0.22, 0.28], $t(60) = 13.07$, $p < .001$, and this was true across all materials (all $p < .001$).

Evaluative conditioning. Evaluative ratings were analyzed in an ANOVA with CS duration, CS material, and US valence as repeated-measures factors. We obtained main effects of US valence (i.e., an EC effect), $F(1, 60) = 8.25$, $MSE = 629.51$, $p = .006$, $\eta^2_G = .007$, and of material, $F(2, 120) = 11.63$, $MSE = 997.50$, $p < .001$, $\eta^2_G = .030$, but not of CS duration, $F(2, 120) = 0.11$, $MSE = 362.80$, $p = .897$, $\eta^2_G = .000$. As illustrated in Figure 2, the US-valence effect was qualified by an interaction with CS duration, $F(2, 120) = 6.40$, $MSE = 390.41$, $p = .002$, $\eta^2_G = .007$: EC was obtained only for CSs presented for 1000 ms, $F(1, 60) = 17.57$, $MSE = 544.51$, $p < .001$, $\eta^2_G = .035$, but not for CSs presented for 30 ms, $F(1, 60) = 0.02$, $MSE = 269.44$, $p = .895$, $\eta^2_G = .000$, or 100 ms, $F(1, 60) = 1.04$, $MSE = 596.38$, $p = .313$, $\eta^2_G = .002$. The EC effect was not modified by the material factor, $F(2, 120) = 0.02$, $MSE = 352.43$, $p = .980$, $\eta^2_G = .000$.

The main effect of CS material reflects the fact that, overall, products ($M = 111.87, SD = 40.25$) were rated more positively than faces ($M = 105.80, SD = 38.95$), which were in turn rated more positively than nonwords ($M = 100.62, SD = 38.27$). The two-way interaction between CS material and CS duration, $F(4, 240) = 2.61$, $MSE = 265.44$, $p = .036$, $\eta^2_G = .004$, was also significant and reflected the finding that the preference differences across levels of the material factor were attenuated in the 30ms condition in which the latter two materials were evaluated similarly (faces: $M = 104.70, SD = 35.63$, nonwords: $M = 103.84, SD = 34.91$).

Discussion

The absence of EC for briefly presented and masked CSs was replicated for a different set of nonwords, as well as extended to faces and product images. CS identification was greater in this study due to the different presentation conditions (i.e., images instead of words, pixel pattern masks instead of random consonant strings, only backward-masking); however, there were again no EC effects in the 30ms masked condition.

Experiment 5 did obtain an EC effect for CSs presented for longer durations (i.e., 1000ms) that are typical of EC studies. However, as in Experiment 1, there was no EC in the 100ms condition. This difference in findings between the 100ms and 1000ms conditions may be explained by the notion that 100ms CS presentations may have resulted, under the current presentation conditions, in a weaker memory trace of the CS-US episode, which subsequently failed to support evaluative learning. Alternatively, EC may have failed to obtain for the 100ms conditions in Experiments 1 and 5 because the difference in presentation onset of CS and US did not allow for implicit misattribution effects to operate. We addressed this possibility in a final study. #Experiment 6

In a final study, we attempted to realize an incidental learning situation closely resembling the surveillance paradigm (Olson & Fazio, 2001). In this paradigm, participants are typically told to view a (supposedly random) stream of stimuli and are instructed to pay attention to the identity of all of the stimuli. This is an incidental learning instruction in that it does not focus participants‘ attention on valence or on pairings; yet, it ensures that both CS and US stimuli are attended. In this paradigm, EC effects have repeatedly been obtained in the absence of memory for CS-US pairings, and these findings have been interpreted as the result of an implicit-misattribution process by which the affective response elicited by the US is misattributed as being caused by the CS (Jones, Olson, & Fazio, 2010). According to this account, an EC effect in the absence of awareness should occur when source confusability for the evaluative response to the US is maximized; procedural features thought to maximize source confusability (and, by implication, an awareness-independent EC effect) are simultaneous presentation onset of CS and US, spatial contiguity or proximity of CS and US, the pairing of a CS with (a sufficient number of) different USs, and high CS (relative to US) salience. These features were realized in Experiment 6, in which we focused participants' attention on both the CS and the US by asking them to identify the CS on some trials and to identify the US on other trials, with participants not knowing until after the trial which stimulus they would have to identify. We also realized a common presentation onset of CS and US. To increase CS salience, participants were asked to always attend to the CS first, which was presented centrally and cued by the forward mask. CS and US stimuli were approximately of the same size, which also increased CS salience compared to the previous experiments. As in previous studies, we paired each CS with several different USs of the same valence.

Method

Participants and design. A total of $N = 60$ students who had not participated in one of the other studies within the last year participated in the experiment and received either a monetary compensation or partial course credit. We implemented a 2 (US valence: positive vs. negative) x 3 (CS-duration: 30ms/6 pairings, 30ms/12 pairings, 100ms, 900ms) repeated-measures design, with a third factor (6 vs. 12 CS-US pairings) nested in the 30ms CS-duration condition.

Materials and procedure. This study was implemented using the OpenSesame software (Mathôt, Schreij, & Theeuwes, 2011). We used the product pictures from Experiment 5 as CSs. The same IAPS pictures (USs) were used as in the previous experiments, but we added positive ($M = 7.79$), negative ($M = 2.62$), and neutral ($M = 5.26$) IAPS pictures to the US set (see Appendix), for a total of 36 USs at each valence level. The USs elicited moderate levels of arousal that were comparable for positive ($M = 4.84$) and negative USs ($M = 4.83$), but somewhat lower for neutral USs ($M = 3.92$).

Participants were instructed to attend to and try to identify the pictures, and were told to focus initially on the briefly presented central stimulus (CS). On the trials with a valent (positive or negative) US, they were later asked to select the CS from a list of six options. In about one third of trials, CSs were presented with neutral USs; in these trials, participants were asked to identify the US from a list of six options at the end of the trial. In other words, participants were informed about which stimulus they had to identify on that trial only when the set of identification options was presented, so they had to pay attention to both stimuli.

According to Jones et al. (2010), a small number of pairings of a CS with different USs may not be sufficient to produce an awareness-independent EC effect. Therefore, we manipulated the number of pairings for a given CS: Of the 16 CSs which were presented for a duration of 30ms, eight were paired with six different USs during learning (i.e., 48 trials), and another set of eight CSs were paired with 12 USs (i.e., 96 trials). In addition, there were four CSs (two for each US valence) that were presented for 100ms and 900ms, respectively; each was paired with 12 USs (resulting in another 96 trials). This also implies that, assuming comparable variability, statistical power was greater for detecting EC effects for the (larger number of) briefly presented CSs than for the (smaller number of) CSs presented for longer durations.

Each trial started with a 500ms preparation period, followed by a pre-mask stimulus that was presented for 500ms and served as a fixation goal. It was replaced by the joint presentation of CS and US that shared a common onset. The CS was replaced by a post-mask stimulus, which was presented together with the US for a total duration of 1000ms. It was followed by the identification task in which either CS or US had to be identified. In total, there were 336 trials, divided into six blocks of 56 trials each. In between blocks, participants were asked to take a break and to press a key to resume the experiment as soon as they were ready. After the presentation phase, evaluative ratings were collected on a 19-point rating scale with endpoints labelled as “unpleasant” (0) and “pleasant” (18).

Results

CS identification. Mean proportion of correct CS identifications was analyzed in an ANOVA with CS type (30ms/6 pairings, 30ms/12 pairings, 100ms, 900ms) as repeated-measures factor. Figure 2 shows that, as expected, mean CS identification was affected by CS duration, $F(3, 177) = 2258.07$, $MSE = 0.00$, $p < .001$, $\eta^2_G = .960$. The identification performance for briefly presented (i.e., 30ms) CSs was clearly above chance (chance-level: 1/6) and comparable for the CSs with 6 pairings, $M = 0.22$, 95% CI [0.20, 0.24], $t(59) = 4.81$, $p < .001$, and those with 12 pairings, $M = 0.20$, 95% CI [0.18, 0.22], $t(59) = 3.16$, $p = .003$, indicating that increasing the number of CS-US pairings did not affect CS identification. CS identification was very good in the 100ms condition ($M = 0.91, SD = 0.29$) and nearly perfect in the 900ms condition ($M = 0.98, SD = 0.14$).

Evaluative conditioning. Evaluative ratings were analyzed in an ANOVA with CS type (30ms/6 pairings, 30ms/12 pairings, 100ms, 900ms) and US valence as repeated-measures factors. We obtained a main effect of CS type, $F(3, 177) = 9.94$, $MSE = 7.39$, $p < .001$, $\eta^2_G = .051$, but only a marginally significant effect of US valence, $F(1, 59) = 3.15$, $MSE = 7.11$, $p = .081$, $\eta^2_G = .005$ (positive: $M = 10.62, SD = 4.04$; negative: $M = 10.17, SD = 4.18$; see Figure 2). The EC effect also did not interact with CS type, $F(3, 177) = 1.02$, $MSE = 4.79$, $p = .384$, $\eta^2_G = .004$. The main effect of CS type reflected the finding that briefly presented CSs (30ms/6 pairings: $M = 10.00, SD = 3.98$, 30ms/12 pairings: $M = 9.76, SD = 4.18$) were evaluated less positively than clearly visible items (100ms: $M = 11.32, SD = 3.88$; 900ms: $M = 11.12, SD = 4.07$).

Most importantly, there was again a significant EC effect for CSs presented for longer durations (100 or 900ms), $F(1, 59) = 4.09$, $MSE = 7.03$, $p = .048$, $\eta^2_G = .012$, but no EC effect for briefly presented CSs (30ms, 6 or 12 pairings), $F(1, 59) = 0.38$, $MSE = 4.67$, $p = .538$, $\eta^2_G = .001$.

Discussion

Experiment 6 realized an incidental-learning paradigm for which EC effects in the absence of awareness, when operationalized as memory for CS-US pairings, have been repeatedly reported. Using this paradigm, we investigated the notion that the processes underlying these EC effects may also produce EC under brief and masked CS presentation conditions. Replicating results of our previous experiments, we obtained (small) EC effects for longer CS presentation durations, but failed to obtain an EC effect for briefly presented and masked CSs.

Complementary insights from a joint analysis

Across six experiments, we did not find evidence for an EC effect for briefly presented and masked CSs. Perhaps, then, EC effects under those presentation conditions are smaller than expected. In this case, each individual study may have been underpowered, but a meta-analysis may be able to detect such smaller effects. To address this possibility, we report the results of a set of joint analyses of the data from all experiments. In a first section, we address the concern that the lack of evidence for EC may have been due to a lack of statistical power. Second, we conducted a random-effects meta-analysis of the EC effects obtained in each of the 27 experimental conditions realized in the present study. Third, we depict the EC- and CS-identification effects as a function of CS presentation duration, to illustrate the relative sensitivity of both effects to (increases in) presentation duration. We also illustrate how the EC effect varies as a function of CS identification, to illustrate how non-zero EC effect sizes depend on clearly above-chance CS identification effects. Finally, we focus on the subset of briefly presented and masked CSs and analyze EC effects as a function of CS identification performance on the person-by-item level.

Interpretability of the null finding of no EC for brief and masked CSs

In Experiments 2-5, we obtained significant interactions between CS presentation manipulation and US valence: Where EC was obtained under conditions that made it possible for participants to consciously process the CS-US pairings, it consistently vanished when approaching conscious perception boundaries (i.e., when using a masking procedure or by reducing CS presentation duration). Nevertheless, one might argue that the article's central finding is based on a null effect (i.e., the absence of EC under suboptimal presentation conditions), and that the studies may have lacked sufficient statistical power for obtaining an EC effect for subliminal CSs. We believe this is not the case, for empirical as well as statistical reasons. First, CS identification was slightly but significantly above chance in almost all the experiments reported here, demonstrating that the studies allowed for detecting relatively small effects. Perhaps more importantly, the lack of an EC effect cannot be attributed to a lack of statistical power, as each of the individual experiments reported here was highly powered. The power to detect an effect of the size suggested by the Hofmann et al. (2010) meta-analysis (i.e., $d = 0.46$) was high in every single study (i.e., $1 - \beta \geq .95$). The present designs further demonstrated its sufficient power by obtaining significant EC effects in the long/unmasked presentation conditions that were often smaller than .46 (see below for a meta-analysis). Thus, if an EC effect of comparable magnitude would have occurred under suboptimal presentation conditions, we would have detected it in the present studies. We can therefore conclude at this point that EC effects were at least reduced under the suboptimal viewing conditions realized here.

Another way of addressing this statistical issue is to compute a Bayes-factor analysis that allows quantifying the evidence for the null versus the alternative hypothesis. Bayes factors indicate how much more likely the data are under one hypothesis – the null hypothesis of zero subliminal EC effect – when compared to an alternative hypothesis – a medium-sized EC effect greater than zero.[9] A meta-analytic Bayes factor that combines the evidence of the brief/masked presentation conditions across all studies yielded considerable support for the null hypothesis of no subliminal EC: The Bayes factor $BF_{01} = 33.19$ states that the data are about 33 times more likely under the null hypothesis. This means that the results modify the prior odds in favor of the null hypothesis (i.e., no subliminal EC) over the alternative hypothesis (i.e., subliminal EC) by this factor; in other words, the present evidence should lead us to considerably strengthen our belief in the null hypothesis.

The second critical question was whether there would still be evidence for (smaller) EC even under suboptimal viewing conditions. If we reject the notion that EC effects are independent of awareness, EC effects may be weaker under suboptimal presentation conditions, and small effects could have gone undetected in the present study. Sensitivity power analyses showed that, based on the $N = 423$ participants across Experiments 1-6, and assuming $\alpha = \beta = .05$, the present study was able to detect effects as small as $d = 0.16$. Taken together, power analyses showed that the present studies could reliably detect not only the expected effects of medium size that were obtained by the meta-analysis (Hofmann et al., 2010), but also much smaller effects. Subliminal EC effects of an even smaller size (i.e., $d < .16$) may have gone undetected, and more powerful designs would be necessary to investigate such small effects. Foreshadowing the results of a meta-analysis of the present findings, which indicated a weighted mean EC effect of zero for briefly presented and masked

[9] We used a one-sided Cauchy distribution with scale parameter of .71, as recommended by Rouder, Speckman, Sun, Morey, & Iverson (2009). The Bayes factor depends on the exact choice of the null and alternative models (i.e., on the assumptions about the range of plausible effect sizes and their respective probability densities). We have chosen a standard alternative model for which effect sizes are most likely to vary between 0 and .71, with larger effect sizes increasingly unlikely to occur. Different assumptions do not change the substantive conclusions: Even with an alternative that strongly favors small effects (i.e., a scale parameter of .2), the Bayes factor still substantially favors the null hypothesis, $BF_{01} = 9.60$. With an alternative favoring larger effects (i.e., a scale parameter of 1), the Bayes factor is even more strongly in favor of the null hypothesis, $BF_{01} = 46.89$.

CSs, it is however unclear whether such high-power efforts would be worthwhile.

A meta-analysis of EC effects

We conducted a random-effects meta-analysis of the EC effects obtained in all experimental conditions across Experiments 1-6.[10] As a measure for the magnitude of the EC effect, we computed Cohen's d for each condition.[11] The mean EC effect size was $d = 0.16$ (with a 95% CI ranging from 0.08 to 0.24); however, there was substantial heterogeneity across effect sizes ($Q = 61.50, df = 26, p < .001$). We therefore included CS presentation condition as a moderator and computed separate estimates for subgroups of EC effects for briefly presented and masked CSs, comparing them to those for CSs presented for longer durations and/or non-masked.

The moderating role of CS presentation. For briefly presented and masked CSs, the mean EC effect size was practically zero, $d = 0.02$ (95% CI: -0.05, 0.09), and the effect sizes in this subgroup were homogeneous ($Q = 12.98, df = 12, p = 0.37$). For longer presentations, the EC effect size was of medium size, $d = 0.30$ (95% CI: 0.17, 0.42), more comparable to the effects reported in the Hofmann et al. (2010) meta-analysis (i.e., $.46 < d < .49$). Because the EC effect varied substantially across the long/unmasked conditions ($Q = 30.77, df = 13, p = 0.004$), we included orientation task as a moderator for this subset of effects.

A moderating role of orientation task. The heterogeneity in longer/unmasked presentation conditions points to potential moderating effects of orienting task. We compared EC effects from learning conditions in which attention was directed toward the CS-US pair

[10] We included all 27 conditions despite the fact that this introduced some dependency across conditions due to the within-subject nature of some of the experimental manipulations. We believe this dependency is unlikely to affect the substantial conclusions drawn from the meta-analytic results.

[11] We repeated the analyses reported below with different variants of computing the effect sizes measure (i.e., using the standard deviation of the raw scores versus the standard deviation of the difference scores). The pattern of results was not affected by the choice of measure.

(i.e., the valence and brightness focus conditions) to EC effects from incidental learning conditions in which participants' main orienting task was to identify the individual stimuli.

Under learning conditions that required attention to CS-US pairs, the mean EC effect size was medium-to-large, $d = 0.62$ (95% CI: 0.39, 0.86); there was no evidence that the effect sizes in this subgroup were heterogeneous ($Q = 4.63, df = 4, p = 0.33$). In contrast, EC was small when participants were not required to attend to the CS-US pair, $d = 0.20$ (95% CI: 0.10, 0.29); the effect sizes in this subgroup were also homogeneous ($Q = 12.11, df = 8, p = 0.15$).

Taken together, EC effects were zero for subliminal (i.e., brief and masked) CS presentations; they were small for supraliminal CSs under incidental learning conditions when participants attended only to individual CS/US stimuli; and they were medium-to-large for supraliminal CSs under incidental learning conditions that required attention to CS-US pairs. Figure 3 summarizes the EC effects for the three homogeneous subgroups identified in the meta-analysis.

EC and CS identification as a function of CS presentation duration

One way to analyze whether EC effects can obtain in the absence of awareness is to investigate whether they occur in the absence of CS identification. Figure 4 plots CS identification and EC effects (as well as their meta-analytic means and 95% CIs) as a function of CS presentation duration; it illustrates the point(s) on the duration axis at which EC and identification begin to exhibit non-zero effect sizes. If EC effects require little or no CS identification, then EC effect sizes should be greater than those for CS identification, especially at brief presentation durations. In particular, EC effect sizes should be greater than zero when CS identification is zero. In contrast, if EC requires CSs to be identifiable as a necessary precondition, then EC effect sizes should be smaller than those for CS identification. Results support the later possibility: CS identification was above chance already at 30ms, and clearly so at 100ms, showing large effects. In contrast, EC effects were zero at 30ms, and were only small at 100ms. Figure 4 shows that, across all levels of CS presentation duration, EC effect sizes were consistently smaller than those for CS identification, supporting the second interpretation.

EC effect sizes as a function of identification effect sizes

Another way to investigate whether EC effects are due to an awareness-independent process is to plot EC effects as a function of the awareness measure (i.e., CS identification) as in Figure 5 (see also Schmidt & Vorberg, 2006). If the process underlying EC effects is independent of awareness, a single-dissociation pattern might be expected: EC effects (of varying sizes) should be observed at zero levels of awareness; in other words, we should observe data points on the upper left side of the plot along the vertical axis. Figure 5 shows that this dual-process prediction was not confirmed: nonzero EC effects were obtained only in the presence of medium-to-large CS identification effects.

Another possible dissociation that could show up in this plot is the sensitivity dissociation (Reingold & Merikle, 1988; Schmidt & Vorberg, 2006). It assumes that the CS identification task is more sensitive to conscious processing than the EC effect. If this assumption holds, we can conclude that EC operates independently of CS identification if EC effects are of greater magnitude than the respective CS identification effects.[12] In Figure 5, such a dissociation would be reflected by data points that lie above the main diagonal. Whereas some data points from individual experimental conditions seem to suggest that there is such evidence, all meta-analytic mean effect sizes lie below the main diagonal, indicating the absence of a sensitivity dissociation.

To summarize, Figures 4 and 5 investigate the relation between CS identification and EC effects in qualitative manner. Dual-process theories of evaluative learning predict that

[12]Such a comparison only makes sense if both effects are expressed in the same metric; as a caveat, although we have computed comparable effect size metrics, they are not strictly identical, and Cohen's h can sometimes be difficult to interpret (e.g., perfect CS identification performance is equivalent to $h = 1.05$ given a chance level of 1/2, and to $h = 1.4$ given a chance level of 1/6).

EC effects are possible in the absence of awareness. Contrasting this prediction, both figures show that EC effects depend on above-chance identification of CS stimuli – in fact, above-chance CS identification is necessary yet insufficient for EC.

Correlation between CS identification and EC effects for the subset of briefly presented and masked CSs

Although the previous analyses did not yield evidence for subliminal EC, they did not address the possibility that this might be due to a mixture of processes operating on the set of briefly presented and masked CSs. For instance, if a subliminal EC effect obtained for the subset of objectively unidentified CSs, but a contrast (or negative) EC effect obtained for another subset, namely those CSs that were identified correctly above chance, then aggregating over both types of CSs – as we have done in our analyses of EC as a function of CS presentation – would mask the presence of a subliminal EC effect. To investigate this possibility, we investigated the subset of briefly presented and masked CSs more closely; specifically, we investigated evaluative ratings of individual CSs as a function of US valence and CS identification performance.

We analyzed the data for briefly presented and masked CSs using linear mixed models that are suitable for the present incomplete designs (i.e., for many participants, some of the possible CS-identification by US-valence combinations were not observed). CS evaluations were modelled as a function of US valence and CS identification performance as fixed factors. Random person and item factors were additionally included, with random intercepts as well as random slopes on both fixed factors. Additional factors in a given experiment (i.e., orienting task in Experiment 3, CS duration in Experiment 4, CS material in Experiment 5), as well as their interactions with US valence and CS identification, were also added as fixed factors. To evaluate the notion that EC was correlated with CS identification even for briefly presented and masked CSs, we tested whether a model that included the US-valence by CS-identification interaction term was better able to account for the data than a model without such a term.[13] With the exception of Experiment 3, the model without an interaction term was preferred (i.e., omitting the term did not harm model fit) in Exp.1, $\chi_{(df=2)} < .01, p > .99$; Exp.4 (30ms), $\chi_{(df=2)} = 0.89, p = 0.64$; Exp.4 (50ms), $\chi_{(df=2)} = 1.18, p = 0.55$; Exp.5, $\chi_{(df=3)} = 0.59, p = 0.9$; and Exp.6, $\chi_{(df=1)} = 0.59, p = 0.44$. In Experiment 3, a robust three-way interaction with orienting-task obtained, and separate analyses revealed that a correlation between CS identification and EC was present in the brightness-focus group, $\chi_{(df=1)} = 7.31, p = 0.01$, but not in the valence-focus group, $\chi_{(df=1)} = 0.13, p = 0.72$. In the former group, as depicted in Figure 6, an EC effect tends to emerge for brief and masked CSs when they were correctly identified on 3 or more trials. While it might be worthwhile to follow up on this finding (perhaps using a subjective-threshold approach) this significant interaction should be interpreted with caution because (a) the effect is based on relatively few observations that had 3 or more correct CS identifications, (b) linear mixed models are prone to inflated Type I error rates when they do not include the maximal random effects structure (Barr, Levy, Scheepers, & Tily, 2013), and (c) the effect is in contrast with the nonsignificant interaction obtained in Experiment 4 which also used a brightness-focus task.

These results indicate the general lack of a correlation between CS identification and EC in the set of briefly presented and masked CSs. This is also apparent from Figure 6, which illustrates evaluative ratings of brief and masked CSs as a function of CS identification performance and US valence. The depicted regression lines (with confidence bands) overlap, showing the absence of an EC effect, and similar slopes for positive and negative USs indicate the absence of a US-valence by CS-identification interaction. Although this analysis is merely correlational (i.e., because CS identification performance was measured, not manipulated), it helps rule out the possibility that an EC effect for incorrectly identified CSs was masked by a contrastive or negative EC effect for supraliminal CSs.

[13]We did not analyze Experiment 2 because of the incomplete CS-identification data. For the data from Experiments 1 & 3, model convergence was reached only when random item slopes were dropped.

General Discussion

We begin by briefly summarizing the findings of the present study. Next, we turn to its limitations and identify open questions for future research.

Main findings

Across a series of experiments, we manipulated, within-subjects, the identifiability of CS stimuli by impairing stimulus strength. When CSs were presented for 30 ms and were masked, identification was low but remained clearly above chance levels. When CSs were either not masked, or presented for 100 ms or longer, they were clearly identifiable. These findings suggest that participants can partially identify CS stimuli even under brief and masked presentation conditions.

EC effects were found in Experiments 2-5 for CS that were clearly identifiable – either because they were presented for a longer duration (100ms in Experiment 4; 1000ms in Experiment 5), or because they were not masked (Experiments 2 & 3). Despite above-chance levels of CS identification, no EC effects were found under brief and masked presentation conditions in Experiments 1-6. This finding was replicated with different methods of reducing stimulus strength (i.e., masking in Exp. 2 & 3; presentation duration in Expts. 1, 4-6); it did not depend on the requirement of trial-wise identification responses (Exp. 2); it was replicated across different orientation tasks (valence judgments in Expts. 2 & 3; brightness judgments in Expts. 3 & 4; CS & US identification in Exp. 6; CS identification in Expts. 1 & 5); it was unaffected by the response requirement in the orienting task (Exp. 4); and it was replicated across different types of conditioned stimuli (nonwords, faces, products). These results suggest that EC does not occur with briefly presented and pattern-masked CSs.

Taken together, when we experimentally reduced CS stimulus strength to low but still above-chance levels of CS identification, EC effects were eliminated. In other words, we found that supraliminal presentation of the CS was necessary (but not sufficient) for EC in a logical sense, such that EC did not occur for subliminal CSs. This suggests that awareness is necessary (but not sufficient) for EC in a logical sense (Dawson & Furedy, 1976). The present findings do not, however, support the claim that awareness plays a causal role in EC: As we have only indirectly manipulated awareness via stimulus strength, the present study is not capable of addressing the causal status, or mediating role, of awareness. Nevertheless, the present findings are of course consistent both with strong as well as weak single-process learning models (Lovibond & Shanks, 2002): In the strong model, a single (propositional) learning process gives rise to awareness, which then plays a causal role in the production of the learning phenomenon (Mitchell et al., 2009). In the weak model, the learning process gives rise to both awareness and learning; in this model, although correlated with learning, awareness has no causal role and is merely epiphenomenal. The present finding is, however, inconsistent with dual-process models postulating awareness-independent evaluative learning (e.g., Gawronski & Bodenhausen, 2006; Jones et al., 2009).

Additional findings

The role of orienting task. Across studies, we manipulated the orienting task participants performed during the learning phase. On the one hand, EC was not robustly found – even for clearly supraliminal CSs presented for 100ms – when the orienting task induced a strong focus on CS identification and allowed participants to ignore the US (although EC was obtained with this orienting task in Exp. 5 with CSs presented for 1000ms). On the other hand, EC was obtained when participants were instructed to judge the valence of the CS-US pair, when they were instructed to judge the brightness of the CS-US pair, or when they were asked to attend to both the CS and the US. The orienting tasks differ with regard to several features that differ in their ability to explain this pattern.

In the EC literature, two types of associations are discussed as potentially underlying EC effects. First, referential learning accounts assume that CS-US (or S-S) associations are learned, such that retrieval of the CS activates the representation of the US, thereby allowing for its valence to affect responses. In contrast, holistic (S-R) accounts assume that during learning, the US elicits an evaluative response that is then associated with the CS; later CS retrieval then activates the associated evaluative response. Both the valence-focus orienting task as well as the surveillance task have been argued to favor S-R learning (although different processes have been postulated for each task; see Gast & Rothermund, 2011a; Jones et al., 2009); it is not known whether the brightness-focus task favors one of the two types of associations.[14] Because S-R learning is assumed to underlie EC effects in both the valence-focus task (which yielded large EC effects) and the surveillance task (which yielded small effects), the type of association – S-S versus S-R – is unlikely to account for the differences in EC effect size obtained across the present experiments.

The pattern of EC effects across orientation tasks may more readily be interpreted as suggesting that processing mode modulates EC, with an integrative or holistic mode presumably induced by a valence or brightness task leading to greater EC effects. In a similar vein, previous work has shown that EC was greater when participants processed similarities between CS and US than when they focused on differences (Corneille et al., 2009). The fact that, in other studies, EC has been found without special orienting instructions may be explained by the assumption that a holistic mode is the default. Yet, an explanation in terms of processing mode appears to be inconsistent with the finding of EC effects in a surveillance orienting task (Olson & Fazio, 2001) in which participants searched for a specific target stimulus, suggesting an analytic mode. Thus, it remains to be seen whether the moderating variable underlying the present pattern is indeed processing mode.

The above findings could also be explained by CS-US integration. They are consistent with recent work suggesting that a relational or integrative processing of CS and US is beneficial for EC (e.g., Jones et al., 2009). If EC depends on the formation of an association between CS and US in memory, then such an association should be more strongly formed

[14]In addition, S-R learning has been claimed to operate when CSs are paired with multiple different USs of the same valence and when there is temporal overlap between CS and US presentations (Sweldens, Van Osselaer, & Janiszewski, 2010); both conditions were realized in the present studies.

when CS and US are both attended to during learning and are therefore more likely to be stored in an integrated trace or episode (see also Kattner, 2012).

Another interpretation is in terms of attention to the information relevant for EC, that is, the CS-US pair and its valence. Across studies, we implemented four different levels of this variable: The highest level was present in the valence-focus task in which participants were instructed to attend to the CS-US pair and to judge its valence; EC effects were greatest under these conditions ($d = 0.90$, 95% CI: $0.51, 1.30$). Attention to the relevant information was reduced in the brightness-focus condition in which participants were also instructed to attend to the CS-US pair, but were asked to focus on brightness instead of valence. EC effects under these brightness-focus conditions tended to be markedly smaller than those obtained under valence-focus conditions ($d = 0.50$, 95% CI: $0.24, 0.75$). We further reduced attention to the relevant information in the last experiment in which participants were instructed to attend to the identity of the individual stimuli (i.e., both CS and US), but were not instructed to process these stimuli as CS-US *pairs*; only small EC effects were obtained under these conditions ($d = 0.22$, 95% CI: $0.01, 0.43$). Finally, EC effects were also small (even undetectable in some individual studies) when participants were instructed to attend to only the CS and encouraged to ignore the USs (which were introduced as background images; $d = 0.20$, 95% CI: $0.08, 0.32$). These findings suggest that the differences between orienting tasks may be interpreted as a function of the attention they direct toward the CS-US pair and its valence.

Main effects of masking and presentation duration on CS evaluation. Across studies, CSs were evaluated more positively when presented for a longer time or when they were not masked. We surmise that this finding may be interpreted as an evaluative conditioning effect. Specifically, longer or unmasked CS presentations may have elicited more positive feelings because of their enhanced ease of detection. Conversely, brief and masked CS may have elicited some frustration, resulting in relatively more negative evaluations. If this interpretation is correct, it would suggest that (1) participants' metacognitive experiences during their processing of a CS-US pairing may act as a US; (2) such experiences are influential enough to add to effects by the US stimuli presented together with the CS; and (3) this effect may occur spontaneously (as it was not moderated by the presence or absence of an identification task in Experiment 2). The latter hypothesis is consistent with other metacognition-driven evaluative learning effects reported in the literature. For instance, people devaluated stimuli that distracted them in a previous selective attention task (Fenske & Raymond, 2006). In addition, preferences increased for objects that were actively gazed at (S. Shimojo, Simion, Shimojo, & Scheier, 2003). And, perhaps conceptually closer to our current findings, it has been argued that mere exposure should be considered a special case of evaluative conditioning (Zajonc, 2001), in which the absence of noxious consequences of a repeatedly exposed stimulus (i.e., the CS) would act as an "internal" US that increases the CS liking. That both external and internal stimuli can result in evaluative conditioning effects is also consistent with instructed evaluative conditioning effects, in which participants are asked to categorize the CSs either positively or negatively (Gast, Gawronski, & De Houwer, 2012).

Limitations and open questions

The present studies have implemented a commonly used procedure of investigating subliminal processes – pattern masking – to study EC effects for subliminal CSs. Under these specific conditions, we did not find such subliminal EC effects. It remains possible that subliminal EC effects will be found under different presentation conditions. In this section we briefly discuss conditions we deem relevant for investigation in future research.

EC effects for briefly presented stimuli may have failed to obtain because implicit misattribution operated on the mask stimuli (instead of the CSs). Because the joint presentation of CS and US was much briefer (i.e., 30 ms) than the joint presentation of the US and the forward and backward masks (i.e., 500 ms and 1470 ms), an association may have formed only for the mask stimuli but not for the CS. For instance, in terms of affect misattribution, an affective response elicited by the US may have been misattributed not to the CS but to the masks: Perhaps the proposed misattribution process is independent of awareness of the CS but does require simultaneous presentation duration to exceed a certain minimum. This may explain the lack of EC for masked CSs in Experiments 2 and 3. To explain why, in Experiment 4, the misattribution process operated on masked CSs when presented for 100 ms but not when presented for 30 ms, it may be assumed that the minimum presentation duration required for EC via misattribution lies between 30 and 100 ms. In effect, such a modified account would predict the absence of EC with subliminal presentation – unless presentation duration can be extended to longer durations without simultaneously increasing awareness. Masking CSs using continuous flash suppression allows for longer presentation durations and may be well suited to investigate this notion (for an overview of masking techniques see Breitmeyer, 2015). More generally speaking, however, whereas it is plausible that EC may *extend* to the mask stimuli, it is not easy to see why EC should be *restricted* to the masks and no longer operate on the CSs; EC effects would be of little practical relevance if they were so easily disrupted by the presence of other stimuli besides CS and US. And if they were, dual-process models of attitude learning would still need to explain how one particular CS among several ones surrounding the US is singled out as the target for conditioning effects.

EC for objectively unidentified CSs may be found with other masking techniques. We reliably and repeatedly failed to find EC effects for a specific presentation condition, namely pattern-masked CSs presented for 30ms. Yet, EC for subliminal CSs may be obtained under other presentation conditions than those realized here: Perhaps EC effects require a greater amount of basic perceptual processing of the CS. Here we used a pattern mask to reliably disrupt processing of the CSs; this type of mask strongly interferes with basic processes in early areas of the visual processing stream and is therefore well-suited for the present purposes of disrupting conscious processing (Breitmeyer, 2015). Other masking techniques (e.g., metacontrast masking, object-substitution masking) interfere only at later processing stages; such techniques may be more likely to yield EC effects because they allow early visual processing to continue for a longer period while ensuring that a conscious representation of the masked stimulus cannot be formed (Breitmeyer, 2015).

EC may obtain for subjectively subliminal CSs. The present study used an objective threshold measure: Correct identification of a masked CS was taken to reflect evidence for perception of this CS above an objective threshold (Merikle, 1982). However, it is possible that the identification task did not exclusively assess conscious processes, and that the above-chance identification performance obtained herein may be (in part or in whole) based on unconscious processes (Henley, 1982). Put differently, the measure does not tell us whether participants in fact consciously perceived the correctly identified CSs – that is, whether processing was not only above an objective but also above a subjective threshold (Cheesman & Merikle, 1986). A correct response in the CS identification task may come about in different ways: Participants may have been fully aware of the CS; they may have perceived only one distinctive feature of the CS that allowed them to discriminate it from the other stimuli; or they may only have processed the stimulus unconsciously (i.e., without being aware of its identity) and may have selected the correct CS because it felt more familiar than the other options (e.g., Craik, Rose, & Gopie, 2015). In particular, some CSs may have exceeded an objective threshold (which enabled participants to correctly identify them based on a feeling of familiarity), but may have remained below the (higher) subjective threshold (i.e., participants did not consciously experience viewing this CS). EC effects may obtain with such subjectively subliminal CS presentations (e.g., this may have been the case in the brightness-focus condition of the present Experiment 3; see Figure 6). The present data do not allow us to address this issue of subjective awareness; future studies should investigate this possibility.

EC may obtain for CSs presented below the objective identification threshold. The objective threshold/strategic (OTS) model of unconscious processes posits that unconscious effects can only be detected in the absence of conscious processes because conscious effects would override unconscious influences (Snodgrass, Bernat, & Shevrin, 2004). More specifically, the OTS model postulates that unconscious perception is maximal at (or just below) the objective *detection* threshold, that is, when participants do not even notice the presence of the stimulus in question (this threshold is assumed to lie below the objective *identification* threshold investigated in the present research). In this case, conscious strategies to override unconscious processing are predicted to be absent. Applied to the present research question, such a model would predict that unconscious EC might be able to operate in cases in which the presence of a CS is not even detected. The model predicts that, when stimulus strength increases above the detection threshold, conscious overriding would initially reduce the effects of unconscious processes on EC (i.e., until the objective identification threshold is reached), before further increases in stimulus strength beyond the objective identification threshold would begin supporting EC effects due to conscious processes. The latter predictions are in line with our findings. Yet, because the present study investigated the objective *identification* threshold we have no data regarding the objective detection threshold and therefore cannot test this model's critical prediction. Thus, in principle, the lack of an EC effect for briefly presented CSs may be explained by conscious override. Such an explanation can account for the present findings if unconscious EC effects can indeed be found at even lower levels of stimulus strength than those we realized here. Despite the admittedly limited practical relevance of such unconscious effects that operate only in the complete absence of conscious processes (Reingold, 2004), it would certainly be worthwhile from a theoretical perspective to investigate the possiblity of EC effects for CSs at the objective detection threshold.

EC may obtain with indirect evaluative measures or different CS-US pairing settings. The six experiments reported here relied on direct evaluative measures (i.e., evaluative ratings on Likert-type scales). We chose this measure for power-related reasons, as EC effects have proven much more sensitive to and robust on direct than indirect evaluative measures (see Hofmann et al., 2010). One may argue, however, that indirect measures (e.g., based on the speed of responses) may be more sensitive for weak EC effects that may not be detected by direct evaluative ratings because they fall below a (conservative) criterion. In addition, it may be argued that indirect evaluative measures such as the IAT or the affective priming task are more likely to reveal awareness-independent learning processes (e.g., because indirect measures tap associative representations, whereas explicit measures tap propositional ones). Consistent with the latter view is a study by Rydell et al. (2006) who found – in the context of a subliminal US (not CS) study – subliminal associative versus propositional learning procedures to selectively impact on indirect and direct evaluative measures, respectively. One should note, however, that both theoretical and empirical arguments speak against the latter suggestion. First, it is theoretically unclear why direct and indirect measures should dissociate as a function of propositional versus associative learning. For instance, outcomes of propositional learning may be automatized, leading to effects on indirect attitude measures, and attitudes claimed to be acquired via associative processes can be consciously accessed, allowing for conscious reports on direct evaluative measures (e.g., Gawronski & Bodenhausen, 2011, 2014; Hahn, Judd, Hirsh, & Blair, 2014).[15] Second, dual-process proponents have repeatedly reported supportive evidence for awareness-independent learning that was obtained using direct evaluative measures (e.g., Hütter et al., 2012; Olson & Fazio, 2001). And the opposite is also true, with awareness-depdendent learning effects obtained on indirect evaluative measures (e.g., De Houwer, 2006; Pleyers et al., 2007; Stahl et al., 2009). Yet, as none of the current studies involved indirect evaluative measures, it remains an open empirical question for the future research whether subliminal CSs allow for successful EC when using indirect measures.

A similar comment applies to the pairing procedures used here. One proposed mechanism for awareness-independent evaluative learning about a novel CS is that

[15] E.g., "people usually have experiential access to their affective gut reactions resulting from associative processes, and that they often rely upon these reactions in making propositional evaluative judgments" Gawronski & Bodenhausen (2011), p.74.

participants implicitly misattribute the evaluative experience caused by the US to the (neutral) CS (Jones et al., 2009). For this mechanism to be operative, both CS and US stimuli should appear on screen simultaneously or in close temporal succession, so as to create a potential for confusion about the origin of the feeling; and participants should attend to the CS, so that the experience is likely to be misattributed to the CS (Jones et al., 2009). Attention to the US is assumed to be unnecessary; the valence associated with the US is presumably extracted and activated automatically (i.e., in the absence of attention to the US; Jones et al., 2009).

Sweldens et al. (2010) proposed a similar mechanism for awareness-independent evaluative learning, namely the forming of direct (S-R) associations between CS and evaluative response. When such a direct link is formed, the CS can directly elicit the evaluative response without the intermediating role of the US representation. In addition to the simultaneous presentation of CS and US, the forming of direct S-R links is said to depend on another procedural factor: the pairing, of each CS, with multiple different USs of the same valence. This procedure has been implemented in most studies reporting evidence for awareness-independent evaluative learning.

In the current six studies, and in particular in Exp. 6, we met several requirements for allowing implicit misattribution or S-R learning. In all studies, CS-US presentations were temporally overlapping, and CSs were paired with USs of the same valence but different identities. In addition, in all studies except Expts. 2 and 3, attention during learning was focused on CS identification – as in the surveillance paradigm – or on non-evaluative features of the US (i.e., the brightness focus).

It is however possible that even stronger requirements would need to be met for implicit misattribution to operate on subliminal CSs. Specifically, subliminal CSs may lead to successful EC when the following conditions are *jointly* met: (1) USs of weak or moderate intensity (as more intense USs prevent implicit misattribution), (2) USs of low perceptual salience (as more salient USs prevent implicit misattribution), (3) identical CS-US onsets (as presenting the USs before the CSs may increase US salience and so prevent implicit misattribution), (4) incidental learning (as explicit learning prevents implicit misattribution), (5) use of indirect evaluative measures (see discussion above).

Three comments are in order here. First, even though our studies may not be optimal for eliciting implicit misattribution effects, they are arguably more adequate than the modalities used in previous studies that reported significant EC effects with "subliminal" CSs – yet involved, for instance, sequential and non-overlapping CS-US pairings. Second, meeting collectively the above conditions may, indeed, be conducive of EC with subliminal CSs. Yet, these conditions would then appear to be so constraining that they would inevitably lead to question the theoretical and practical relevance of dual-process approaches to attitude learning. Third, and perhaps even more important, it is not even clear whether the implicit misattribution account would predict any EC effect under subliminal conditions. As a matter of fact, Jones et al. (2009) argued and showed that EC based on implicit misattribution requires that participants shift their gaze between the CS and the US so as to visually connecting the two – this was supported using both an eye-tracking procedure and a flashing procedure that drew participants' attention to the CS-US co-occurrence. It is unclear how the gaze-shifting requirement may be implemented in a subliminal setting.

Given these open questions, our findings do not conclusively rule out the possibility of awareness-independent evaluative learning. Yet, they point out clear limitations to the scope of such effects that will guide theorizing and help build more precise models of evaluative learning.

Practical and theoretical implications

General claims that EC is independent of awareness may be specified in different ways. Using a framework of conscious perception by Dehaene et al. (2006), we separated attention and stimulus strength as important contributions to awareness in EC. We manipulated stimulus strength and showed that EC does not occur for brief and masked presentation of

CSs. Thus, in terms of this framework, EC does not seem to be possible under subliminal (but attended) conditions. This finding suggests that EC should also be absent under subliminal and unattended conditions because stimulus-related activation, which may have been increased somewhat via top-down attention in the present study, should be even weaker without such top-down attention. The present findings suggest that EC is dependent on awareness in the sense that it cannot operate on perceptually unidentifiable CS stimuli.

The present findings do not deny the possibility that EC may occur under supraliminal but unattended conditions, a condition termed “preconscious” in the above framework. Preconscious processing is characterized by strong stimulus-related activation (that may, e.g., trigger priming effects), but this activation is limited to sensorimotor areas and does not extend to frontal areas involved in attention and conscious perception; as a consequence, the stimulus is not consciously represented or reportable (Dehaene et al., 2006). However, because preconscious stimuli are thought to become conscious once attention is directed towards them, it is difficult to assess the level of consciousness, using on-line self-report measures of awareness as in the present approach, without interfering with the preconscious state of the stimuli.

The possibility of preconscious EC is compatible with recent findings claiming to show EC effects in the absence of awareness. Most prominently, the work in the surveillance paradigm has repeatedly demonstrated EC effects under incidental learning conditions (e.g., Olson & Fazio, 2001; Stahl & Heycke, 2016). In that work, both CSs and USs are clearly identifiable, and participants were required to attend to the individual stimuli (in order to detect the presence of a specific target picture) but not to CS-US pairs or co-occurrence relations. However, participants later reported being unaware of the co-occurrence relations or contingencies between the stimuli (i.e., the fact that one CS was paired only with positive USs, and another CS paired only with negative USs). EC effects in this paradigm may reflect preconscious learning because, while participants processed clearly identifiable CS and US stimuli, they apparently remained unaware of the critical CS-US co-occurrence information.

For practical purposes, the meta-analysis of the present findings suggests that – although small – EC effects are reliable in incidental learning situations, even when people are neither instructed to attend to CS-US co-occurrences nor to attend to the valence of the US. In such situations, people are often busy performing a different and unrelated task, and their intention is not one of learning about the unfamiliar CS stimuli; nevertheless, incidental evaluative learning processes may still operate. If, under such incidental and largely unattended conditions, participants' evaluations of these novel CSs are susceptible to modulation by the valence of the paired USs, these US valence effects can be characterized as unintentional (i.e., occurring in the absence of an explicit intention to learn).[16] Such incidental and unintentional effects of US valence are thought to be at the basis of some advertising effects (e.g., Biegler & Vargas, 2016). While advertising may exert positive influences for producers, consumers benefit if they are aware of its potential influence on their evaluations and decisions. For instance, advertising can have serious negative side-effects when advertisements for prescription pharmaceuticals lead to an unwarranted increase in the use of those drugs (Biegler & Vargas, 2013). It would be of great practical relevance to investigate, for instance, whether warnings about the possible unintended and incidental influences of advertisements are effective in limiting such undesired side-effects. The present study suggests that practitioners and applied researchers should focus their concerns on EC for supraliminal stimuli under incidental learning conditions.

[16]The present data do not speak to the question whether such effects are also uncontrollable in the sense that participants are unable to resist such influences if they were made aware of their possible existence.

Appendix

US images used in Experiments 1-5 (IAPS No.)

Positive USs: 1440, 1460, 1710, 1750, 1920, 2040, 2050, 2057, 2058, 2070, 2071, 2080, 2091, 2150, 2209, 2260, 2340, 2550, 2660, 5830, 5831, 5910, 7330, 7502, 8420

Negative USs: 2053, 2141, 2710, 2750, 2900, 3160, 3220, 3550, 6243, 6311, 6831, 6838, 7380, 9000, 9006, 9007, 9265, 9340, 9400, 9430, 9432, 9500, 9520, 9830, 9920

US images used in Experiment 6 (IAPS No.)

Positive USs: 1440, 1460, 1463, 1710, 1721, 1750, 1920, 1999, 2040, 2050, 2057, 2058, 2070, 2071, 2080, 2091, 2150, 2209, 2216, 2260, 2311, 2340, 2341, 2345, 2395, 2550, 2660, 4626, 5830, 5831, 5910, 7330, 7502, 8420, 8461, 8540

Neutral USs: 1121, 1313, 1616, 1935, 2220, 2372, 2487, 2575, 2580, 2620, 2635, 3550, 4631, 5395, 5455, 5510, 5535, 5731, 5740, 5920, 7000, 7004, 7080, 7090, 7187, 7211, 7233, 7490, 7503, 7504, 7550, 7620, 7640, 7820, 7830, 8211

Negative USs: 2053, 2141, 2205, 2276, 2455, 2750, 2900.1, 3160, 3181, 3220, 3300, 6311, 6831, 7359, 9000, 9007, 9041, 9180, 9220, 9265, 9280, 9290, 9320, 9330, 9331, 9340, 9342, 9415, 9430, 9432, 9435, 9520, 9530, 9561, 9611, 9830

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Table 1

Attentional requirements of the orienting conditions implemented in the present study

Orienting Task	Attend CS	Attend US	Attend CS-US Pair	Attend Valence
Valence judgment (Exp. 2, 3)	Required	Required	Required	Required
Brightness judgment (Exp. 3, 4)	Required	Required	Required	–
CS/US Identification (Exp. 6)	Required	Required	–	–
CS Identification (Exp. 1, 5)	Required	–	–	–

Note. CS: conditioned stimulus; US: unconditioned stimulus; Exp.: Experiment.

Table 2

Overview of experiments

Exp	CS Presentation	Orienting Task	CS Material
1	30ms vs. 100ms (masked)	CS Identification	Nonwords
2	masked vs. nonmasked (30ms)	Valence judgment	Nonwords
3	masked vs. nonmasked (30ms)	Valence/brightness judgment	Nonwords
4	30ms, 50ms, 100ms (masked)	Brightness judgment	Nonwords
5	30ms, 100ms, 1000ms (masked)	CS Identification	Nonwords, Faces, Products
6	30ms, 100ms, 900ms (masked)	CS/US Identification	Products

Note. CS: conditioned stimulus; US: unconditioned stimulus; Exp.: Experiment.

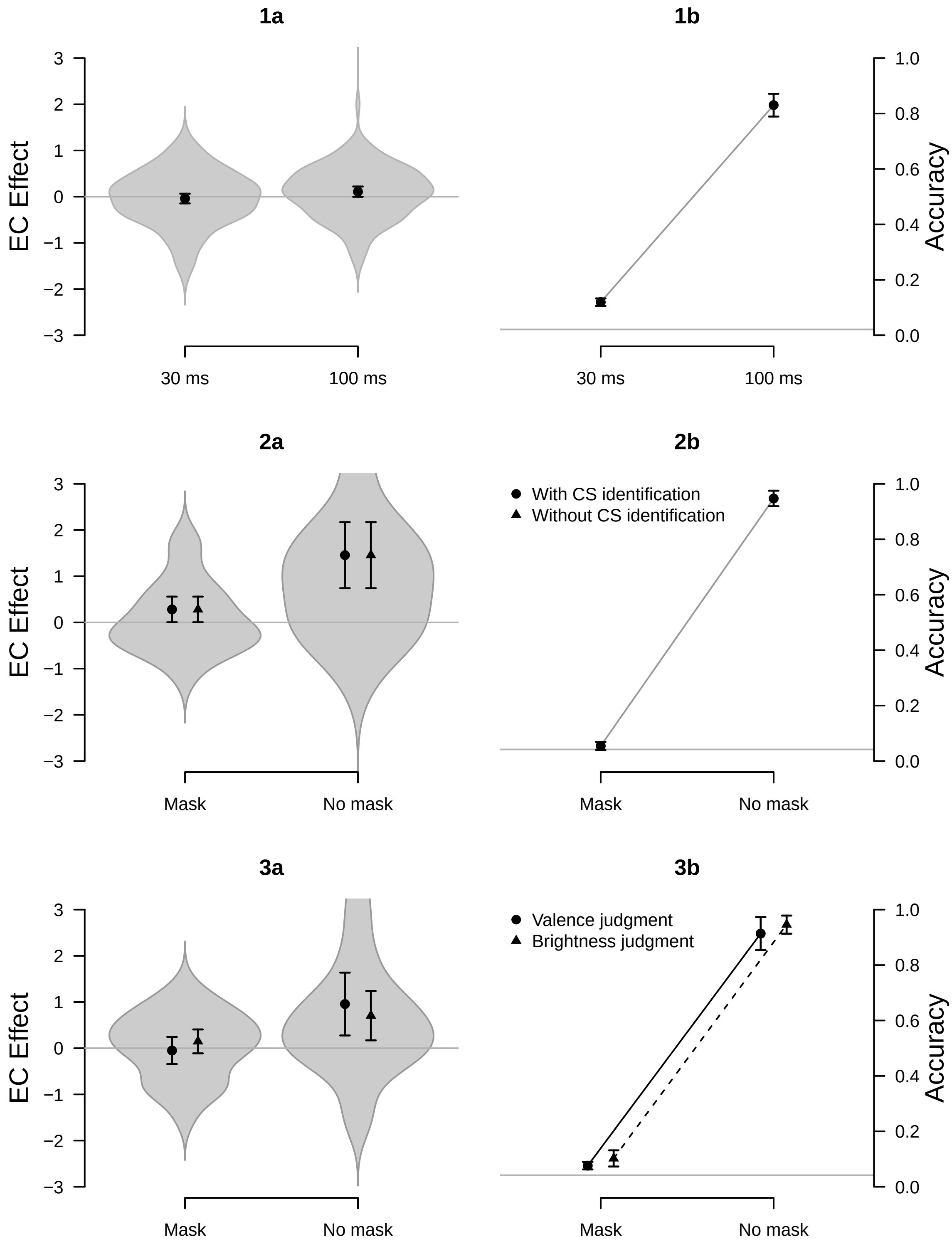

Figure 1. EC effects (left panels, a) and CS identification (right panels, b) for Experiments 1 (top), 2 (middle), and 3 (bottom). Violin plots (background) show distribution of EC effects (i.e., the width of the violin plot reflects the density of the distribution of EC effects across participants). Group means (and 95% CIs) are superimposed in black.

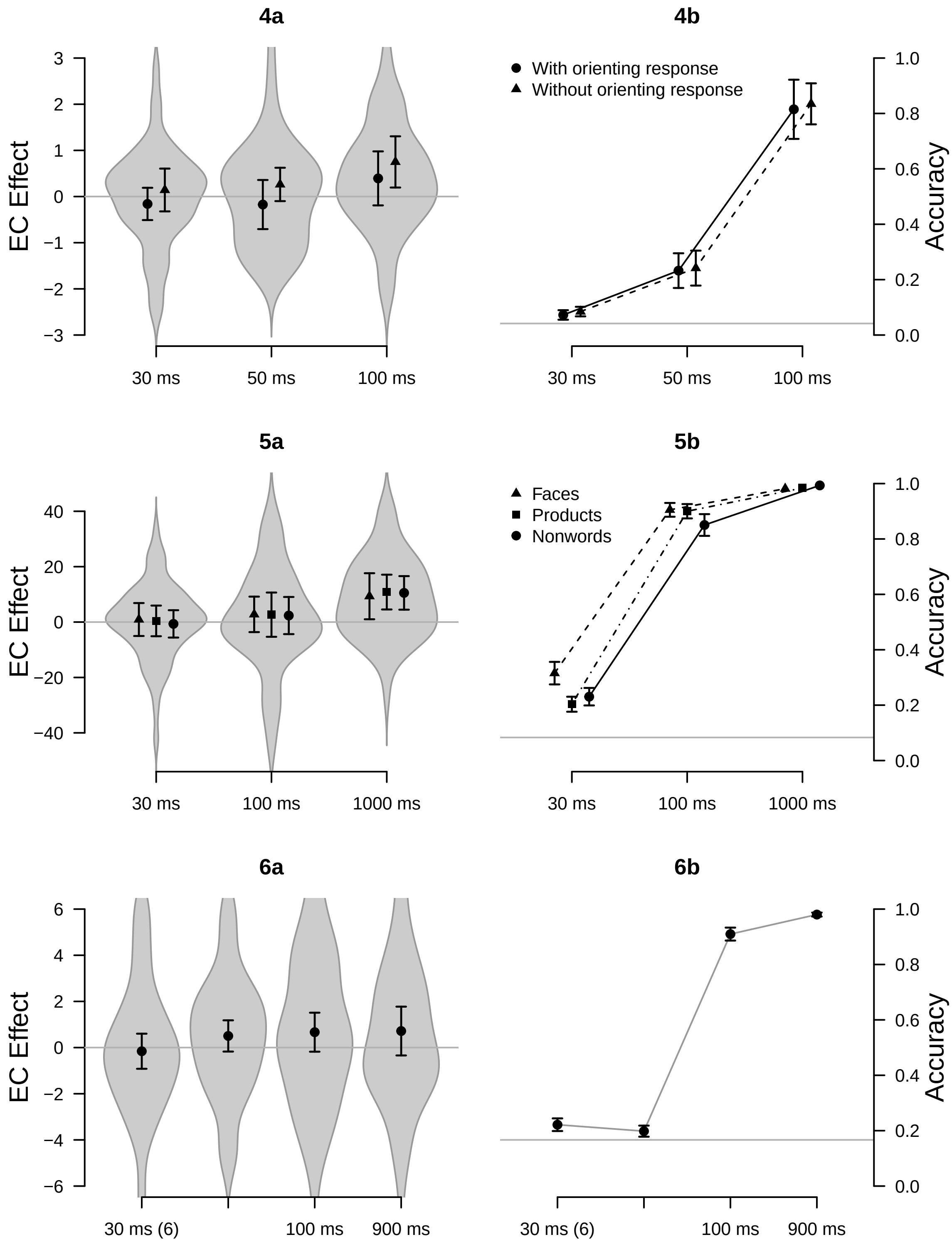

Figure 2. EC effects (left panels, a) and CS identification (right panels, b) for Experiments 4 (top), 5 (middle), and 6 (bottom). Violin plots (background) show distribution of EC effects (i.e., the width of the violin plot reflects the density of the distribution of EC effects across participants). Group means (and 95% CIs) are superimposed in black.

Study	EC (d)	95% CI
Brief/masked CSs		
Exp.1, 30ms, CS identification	−0.08	[−0.20; 0.05]
Exp.2, 30ms, valence focus	0.28	[0.01; 0.55]
Exp.3, 30ms, brightness focus	0.15	[−0.10; 0.40]
Exp.3, 30ms, valence focus	−0.05	[−0.34; 0.24]
Exp.4, 30ms, brightness/no response	0.13	[−0.27; 0.52]
Exp.4, 50ms, brightness/no response	0.29	[−0.10; 0.69]
Exp.4, 30ms, brightness/response	−0.13	[−0.39; 0.14]
Exp.4, 50ms, brightness/response	−0.13	[−0.51; 0.25]
Exp.5, 30ms, CS identification, nonwords	−0.03	[−0.23; 0.18]
Exp.5, 30ms, CS identification, faces	0.04	[−0.20; 0.28]
Exp.5, 30ms, CS identification, products	0.02	[−0.21; 0.24]
Exp.6, 30ms, CS/US identification (12)	0.18	[−0.06; 0.42]
Exp.6, 30ms, CS/US identification (6)	−0.06	[−0.36; 0.23]
Random effects model	**0.02**	**[−0.05; 0.09]**
Long/unmasked CS; Attend individual CS (US)		
Exp.1, 100ms, CS identification	0.06	[−0.07; 0.20]
Exp.5, 1000ms, CS identification, nonwords	0.39	[0.16; 0.62]
Exp.5, 1000ms, CS identification, faces	0.33	[0.04; 0.63]
Exp.5, 1000ms, CS identification, products	0.42	[0.17; 0.67]
Exp.5, 100ms, CS identification, nonwords	0.08	[−0.15; 0.32]
Exp.5, 100ms, CS identification, faces	0.10	[−0.13; 0.33]
Exp.5, 100ms, CS identification, products	0.09	[−0.17; 0.35]
Exp.6, 100ms, CS/US identification	0.23	[−0.06; 0.52]
Exp.6, 900ms, CS/US identification	0.20	[−0.09; 0.50]
Random effects model	**0.20**	**[0.10; 0.29]**
Long/unmasked CS; Attend CS−US pair		
Exp.2, 30ms, nomask, valence focus	1.08	[0.50; 1.66]
Exp.3, 30ms, nomask, brightness focus	0.56	[0.13; 0.99]
Exp.3, 30ms, nomask, valence focus	0.74	[0.20; 1.29]
Exp.4, 100ms, brightness/no response	0.63	[0.15; 1.11]
Exp.4, 100ms, brightness/response	0.31	[−0.14; 0.75]
Random effects model	**0.62**	**[0.39; 0.86]**

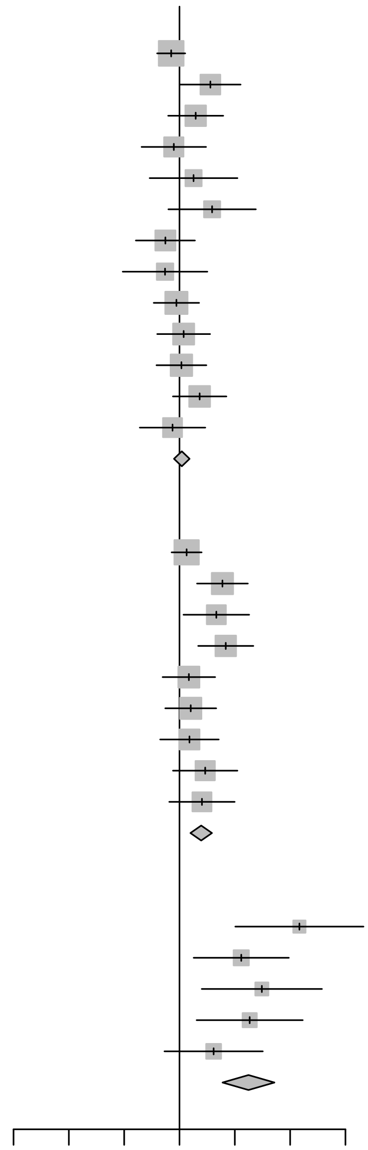

Figure 3. Results of random-effects meta-analysis of EC effects obtained in the present study, grouped by homogeneous subsets of experimental conditions. Effect size estimates and 95% confidence intervals are given for individual conditions as well as group means. The forest plot illustrates effect size estimates (squares represent individual conditions, with size indicative of precision and lines representing 95% CI; diamonds represent group means, with horizontal extent reflecting 95% CIs).

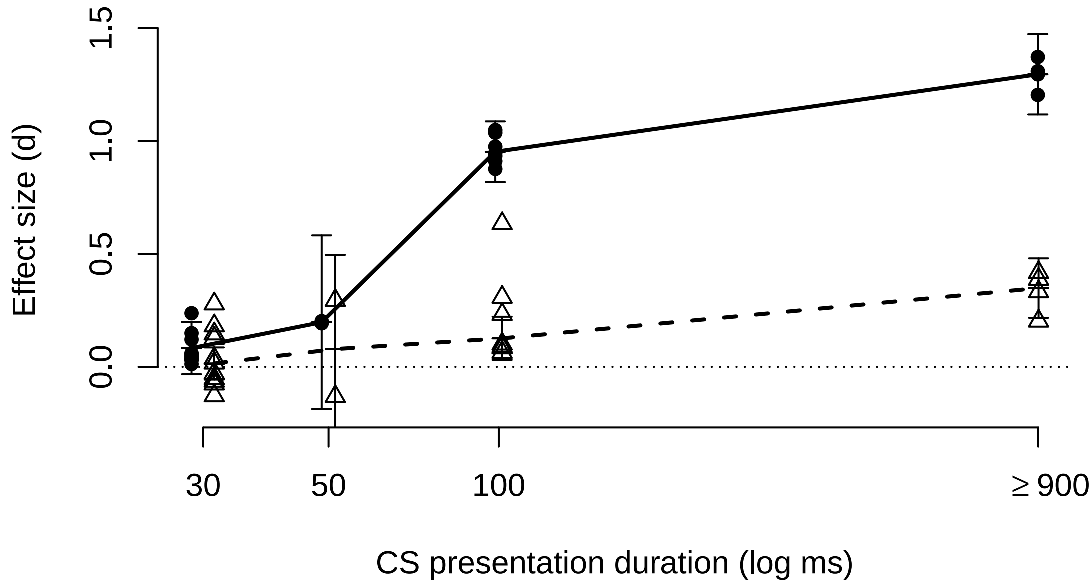

Figure 4. CS identification effects (circles; solid line) and EC effects (triangles; dashed line) by CS duration. Symbols represent effects of individual studies; lines represent meta-analytic means (with error bars showing 95% CIs). Brief non-masked presentation conditions ($k = 3$) are not depicted.

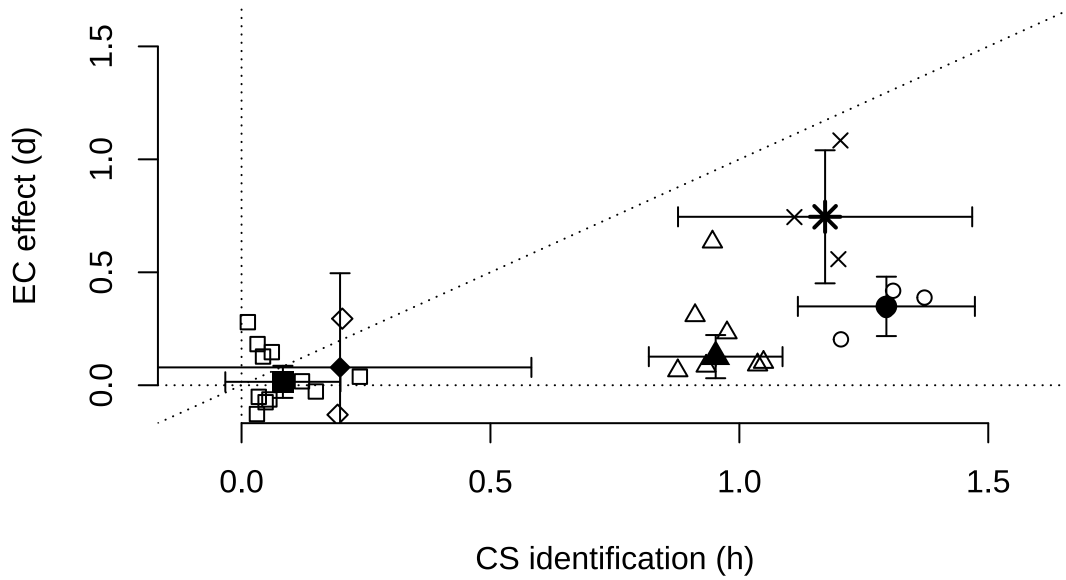

Figure 5. EC effect sizes (Cohen's d) as a function of CS identification effect sizes (Cohen's h) for individual conditions (open symbols) and meta-analytic means (filled symbols; error bars show 95% CIs). Symbol indicates CS presentation condition (squares: 30ms; diamonds: 50ms; triangles: 100ms; circles: 900/1000ms; crosses: 30ms non-masked).

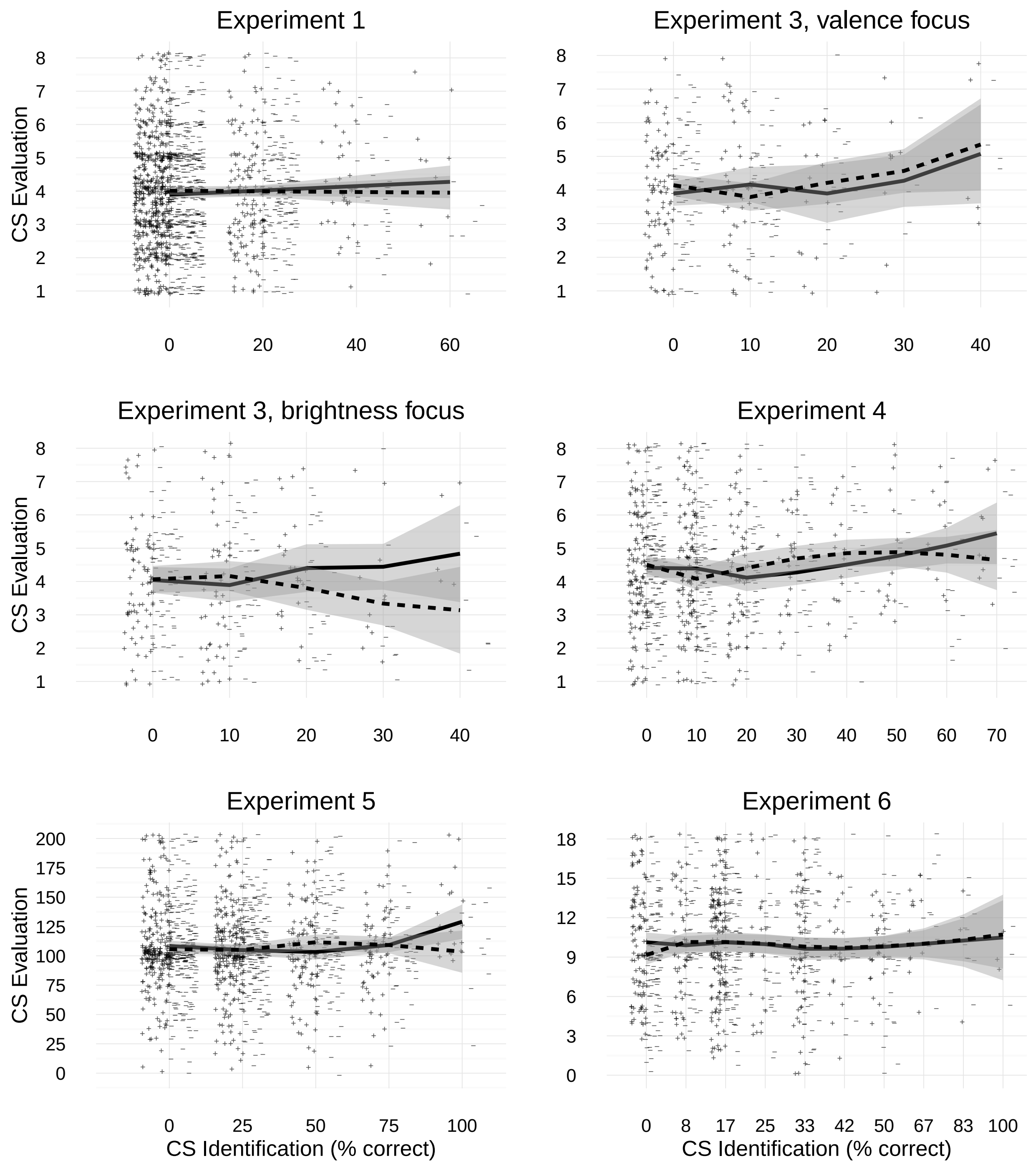

Figure 6. Evaluative ratings of only the briefly presented (30ms) and masked CSs, plotted as a function of CS identification (horizontal axis) and US valence. Symbol type represents US valence (+: positive USs; -: negative USs; each symbol represents an individual CS rated by one participant; symbols are jittered to avoid overplotting). Lines (solid: positive USs; dashed: negative USs) depict locally smoothed regression models (with 95% confidence bands).