Absence of evaluative conditioning with briefly presented conditioned stimuli even at above-chance identification levels

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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, based on a small body of evidence ($k = 8$ studies), suggested similar effect sizes for supra- and subliminal CSs (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 a number of parameters, namely the CS duration, the presence or absence of a masking procedure, the presence or absence of a visibility check, the CSs type, and the orienting instructions communicated to participants. Across studies, we were consistently unable to obtain evidence for briefly presented and masked EC effects. In most studies, this pattern was observed despite above-chance identification of the CSs. A meta-analysis conducted across the 27 experimental conditions supports the conclusion that EC effects for briefly presented and masked CSs are not very robust, or 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

Absence of evaluative conditioning with briefly presented conditioned stimuli even at above-chance identification levels

A ubiquitous feature of human cognition is that we constantly evaluate the objects of our thoughts. It is therefore important to ask about the determinants of these evaluations, and to investigate the processes by which evaluations are learned. Evaluative conditioning (EC) is one of the mechanisms by which preferences are thought to be acquired: The pairing of an initially neutral conditioned stimulus (CS) with an unconditioned stimulus (US) that is evaluated positively (negatively) affects subsequent evaluations of the CS to be more positive (negative) (De Houwer, 2007).

In dual-process theories of attitude learning, EC has been characterized as an automatic process, with one of its central features being the independence of awareness of the CS-US pairing during acquisition (Gawronski & Bodenhausen, 2006). The question whether EC can in fact occur in the absence of awareness is of great theoretical importance for theories of human learning, as EC is one of only few examples in which dissociations have been claimed between learning and conscious awareness (for reviews, see Lovibond & Shanks, 2002; Mitchell, De Houwer, & Lovibond, 2009).

In the absence of precise theoretical statements regarding the specific set of conditions under which automaticity conditions EC may occur (Moors & De Houwer, 2006), the role of awareness of the CS-US pairings in EC may be investigated by different means. Previous work has often focused on using measures of memory for the CS-US pairing, collected after the learning phase, as a proxy for awareness at learning. Although the latter memory-based and correlational approach showed important recent developments (Hütter, Sweldens, Stahl, Unkelbach, & Klauer, 2012), it is not without problems (for a discussion, see Gawronski & Walther, 2012; Sweldens, Corneille, & Yzerbyt, 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 (Dedonder, Corneille, Yzerbyt, & Kuppens, 2010; 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 manipulated awareness of CSs by varying stimulus strength via mode of presentation: Awareness of CSs was reduced or eliminated by presenting them very briefly and in between masks.

Dual-process theories

Cognitive models of attitude formation have classically proposed that people may come to like or dislike attitude objects through a diversity of mechanisms. The Elaboration Likelihood Model (ELM; Petty & Cacioppo, 1986) posited a diversity of processes lying along a continuum of cognitive elaboration, going from the simplest mechanisms (mere exposure and evaluative conditioning) to the most demanding ones (careful, effortful, time-consuming scrutiny of arguments strength). Chaiken (1987) departed from the latter ELM model by assuming that heuristic and systematic processing may occur in parallel. Both of these very influential and useful models assumed that a greater elaboration of information about the attitude object would generally require more time, motivation, and resources.

The idea of a dual and possibly parallel processing of information in attitude formation was further stimulated by the rise of indirect attitudes measures. If one adopts the view that direct (e.g., explicit evaluative ratings on a likert-type scale) and indirect (e.g., the affective priming task; Fazio, Sanbonmatsu, Powell, & Kardes, 1986; the IAT; Greenwald, McGhee, & Schwartz, 1998) evaluative measures tap into separate representations in memory, then the question arises where these representations come from. The dual-process view suggests that explicit attitudes are learned through a propositional process, which is non-automatic (i.e., conscious, resource-dependent, controllable, and motivated). Implicit attitudes, on the other hand, are learned through associative processes, which are assumed to be automatic (i.e., unconscious, resource-independent, uncontrollable, and non-motivated).

Perhaps because it takes the form of a simple association between an affective and a neutral stimulus, and also because this pairing may involve no lexical information (as in the classic picture-picture paradigm), EC was soon considered the best case for supporting an associative learning pathway (De Houwer, Baeyens, & Eelen, 1994) and actually still is today (Gawronski & Bodenhausen, 2006, 2014b). This explains why the EC paradigm is so critical for supporting theoretical claims of the dual-process models of attitude learning, and why it generated such intense debates regarding its automaticity in general, and its dependence upon awareness in particular (for a discussion, see Sweldens et al., 2014). It is noteworthy that the associative versus propositional debate on EC is not only central to attitude learning and social cognition, but also to implicit learning in general. As a matter of fact, EC may represent "one of the simplest learning preparation imaginable" and "that conditioning does not occur without awareness would seem to place a very major question mark over the possibility of learning without awareness" (Shanks, 2005).

Determinants of awareness

Consciousness resarch 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 it 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*. These stimuli do not enter consciousness. However, if they are attended, such subliminal stimuli have been shown to affect cognitive processes across a range of experimental paradigms (Van den Bussche, Van den Noortgate, & Reynvoet, 2009).

The technique of subliminal presentation has often been used to study the automaticity of psychological processes. If participants are unaware of the identity of a stimulus, or even

unaware of the fact that a stimulus has been presented, they are considered unable to consciously or strategically use stimulus information to inform their responses. Any effects of the subliminal stimuli can then be attributed to the operation of unconscious or automatic processes. This has also been the line of argument in research on EC. If EC can be shown to operate for subliminal stimuli, then we can conclude that the learning process must be implicit. In EC, however, the critical stimulus is the *CS-US pairing*. Awareness of a CS-US-pairing 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 unaware evaluative learning.

For instance, De Houwer, Hendrickx, & Baeyens (1997) investigated EC effects with subliminally presented USs. They found evidence for (albeit small) EC effects despite participants' inability to report the presence of the USs. These authors conclude that these EC effects are due to unconscious or implicit learning processes. An alternative account of these findings relies on the distinction between the US stimulus (of which participants were apparently unaware) and participants' response to this stimulus (which was not assessed and may have been consciously available). In other words, participants may have consciously experienced a (pleasant or unpleasant) feeling in response to the subliminal (positive or negative) USs. As a matter of fact, it has been reported that participants may process the valence of subliminal stimuli (Dijksterhuis & Aarts, 2003; Dijksterhuis, Corneille, Aarts, Vermeulen, & Luminet, 2004; Nasrallah, Carmel, & Lavie, 2009). If they associated this evaluative feeling with the presentation of the CSs in a conscious manner, then the resulting EC effect cannot be attributed to implicit learning. In principle, this alternative account applies to all studies of EC effects that used subliminal presentation of US stimuli. Furthermore, the account is difficult to test because it requires not only to establish the subliminality of the US stimuli but also to reliably detect subtle fluctuations in participants'

feelings that arise in response to these subliminal USs.

Here, we chose to focus on the more straightforward approach of studying EC effects with subliminally presented CS stimuli. For the latter type of stimuli, in order to establish subliminality, 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, we can be sure that participants were not aware of the entire CS-US pairing. The subliminal CS presentation approach therefore enables us to investigate whether EC may occur automatically, in the sense that it operates independently of awareness of CS identity, and therefore, of the CS-US pairing. If EC can be demonstrated for subliminal CSs, this would be strong evidence for unaware evaluative learning.

Evaluative learning with subliminal CSs

There is so far little evidence relevant to the question of whether EC can occur with subliminal CSs (Hofmann et al., 2010). The present research aims a extending the empirical basis in order to provide a more conclusive answer to the question of whether EC effects are robustly obtained with subliminal CSs. In this section, we first discuss the methodological requirements for demonstrating EC with subliminal CSs. Next, we apply these considerations in a brief discussion of the available evidence. We conclude by motivating the present choice of methods for investigating subliminal EC.

Methodological considerations. The EC effect can be assessed in two different ways: It is typically assessed either by comparing, within-participants, the evaluation of CSs that were paired with positive USs to those paired with negative USs, or by comparing the evaluation of CSs before and after the pairing with a (positive or negative) US (Hofmann et al., 2010). The latter contrast requires a repeated measurement of the evaluation of the same CS within the same person. Unfortunately, the initial measurement phase might alert participants to the central role of stimulus valence in the study. Because it is often desirable that participants do *not* attend to stimulus valence (see discussion below), we refrained from

using this procedure and assessed EC effects as the difference between CSs paired with positive versus negative USs.

Subliminality of stimulus presentation is considerably more difficult to assess. 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 CS-US association (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.

Finally, Pratte & Rouder (2009) explain why assessment of visibility during learning is important: In research on the effects of subliminal stimulation (e.g., in subliminal priming studies), it is common to assess empirically whether the presentation conditions implemented in that study were in fact subliminal. This is typically done – independently from the main task of interest – in a separate visibility-check task in which the sequence of events is repeated but participants are no longer asked to perform the main task (e.g., ignore the subliminal prime and attend to the target); instead, they are asked to attend to and identify the subliminal stimuli (e.g., attend to the subliminal prime and ignore the target). In this approach, however, although stimulus presentation is held constant across the main task and the visibility-check task, other important aspects of the task may not be constant. For instance, in the subliminal-priming example, task difficulty is much greater in the visibility-check task than in the main task (i.e., it is much harder to identify subliminal primes than it is to identify supraliminal targets). 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 visibility-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 a separate visibility-check task.

Taken together, the relevant knowledge about CS identity can be detected in a sensitive way by a forced-choice visibility check 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 unconscious evaluative learning.

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 "subliminal EC" studies 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 - but for Study 2b, which is discussed below; 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).

In our view, the strongest available evidence for EC effects with subliminal CSs comes from studies by Custers and Aarts (2005, Exp. 2b) and Gawronski and LeBel (2008, Exp. 2). The former authors conditioned participants' motivation to engage in mundane behaviors (e.g., drawing) with conditioning trials composed of a sequence of a 500 ms fixation point, a

500 ms pre-masking row of Xs, a 23 ms CS word (the to-be-conditioned behavior), a 100 ms postmasking row of Xs, and a 150 ms US word (a positive, neutral or negative word). Evaluative ratings of the conditioned behavior were collected at the end of each trial on a likert-type of scale (on which behavior evaluations ranged from *1 = very negative* to *9 = very positive*). This explicit evaluative measure proved sensitive to US valence, with ratings of the behavior linearly more positive for CS-US pairings involving negative, neutral and positive USs words, respectively. No information is reported about a subliminality check in that particular study. One may however infer that a subliminality check session took place (similar to that used in Study 1 and Study 2a reported in that paper). Unfortunately, in these studies, the subliminality check apparently consisted of a debriefing procedure relying on introspective verbal recall, which, as we just discussed, lacks sensitivity (see also below).

Turning to Gawronski and LeBel (2008, Exp. 2), these authors used a lexical-decision task in which they conditioned the words "Europe" or "Asia" (i.e., the CSs) with positive or negative US adjectives. The conditioning trials started with a 500 ms pre-masking row of Xs, immediately followed by a CS for 17 ms, which was in turn immediately followed by a positive or negative US adjective. Both direct and indirect (i.e., Implicit Association Test, IAT; see Greenwald et al., 1998) evaluative measures were collected following the completion of the lexical-decision (i.e., conditioning) task. The IAT measure was sensitive to the pairing procedure, with more positive IAT scores for "Europe" (as compared to "Asia") when "Europe" had been paired with positive adjectives and "Asia" with negative adjectives (and vice versa). Likewise, explicit evaluations were consistent with the conditioning procedure, although this occurred when participants had been invited to focus on their feelings rather than on their knowledge about the CSs. Although this pattern of findings is consistent with the possibility of subliminal CS effects, no subliminality check was involved in that study.

A more comprehensive (and perhaps less biased) 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 (although, in our view, the Custers and Aarts's study 2b we discussed above would qualify as such). However, a set of $k = 8$ studies was identified that used indirect ("implicit") measures such as affective priming (Fazio et al., 1986) or the IAT (Greenwald et al., 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 the duration of CS presentation was below 50 ms. This criterion does not ensure, however, 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 the CSs on some trials. Such a partially conscious presentation could be sufficient to produce EC effects (see Pleyers et al., 2007). To conclude that the "subliminal" EC effect obtained in the meta-analysis does in fact reflect the results of unconscious learning, it is necessary to carefully consider the methods – and especially the results of subliminality checks – of each of the included studies.

Review of studies included in Hofmann et al. (2010)'s meta-analysis. 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. As mentioned above, the CSs were behavioral state words (e.g., study, write) that were presented for 23ms and pre- as well as post-masked by a row of Xs. They were paired with subsequently presented positive or negative adjectives. To check for awareness, the authors

report using an unspecified debriefing interview during which awareness of the subliminally presented words was assessed. Participants were reported to be unaware of the subliminal words during the debriefing interview. 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; in fact, the presentation condition 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 unaware CS presentations.

More generally, we also have concerns about the rationale behind the inclusion of the Custers & Aarts (2005) studies (but for study 2B) in a review of EC effects. This is because the authors used non-evaluative measures as DVs. As a matter of fact, these authors claim (in line with Berridge, Robinson, & Aldridge, 2009), that their studies tap onto a motivational system that has to be distinguished from a liking system. This, by the way, led them to expect and find motivational effects only in the case of positive, not negative, shaping of the behaviors. The idea behind the psychological and neurobiological dissociation between the liking and wanting systems is that the two systems are operating through separate brain circuits, so that one of the two psychological phenomena may be observed independently from the other (i.e., liking without wanting, or vice versa). More specifically, where the liking system is assumed to involve declarative goals and to be "largely mediated by cortical circuits", the wanting system is thought to require no cognitive elaboration and would be "mediated by more subcortically weighted neural systems" (Berridge et al., 2009, pp. 67–68).

The first three of the six studies reported by Dijksterhuis (2004) were also included in the meta-analysis. In these studies, EC effects on participants' self-esteem were investigated: the pronoun "I" was used as CS and was paired with positive versus neutral words. During a lexical-decision task, the CS was presented for 17ms, it was pre-masked by a row of Xs, and post-masked by either a positive trait word or by a neutral object word. On other trials, the

CS was replaced by a single X, and the target word was replaced by a nonword (i.e., a random letter string). Participants' task was to decide whether the final element of the sequence was a word or a non-word. After completion of the lexical-decision task, participants worked on an implicit measure of self-esteem (i.e., an initial-preference task, or a self-esteem IAT). Finally, 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 unconscious 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. As already discussed, the study adapted the paradigm used by Dijksterhuis (2004) to study stereotype change (i.e., the CSs "Europe" or "Asia" were paired with either positive or negative adjectives). This study 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 CS beverage brands 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. Furthermore, no similar effect was observed in conditioning brands when thirst was not controlled for. 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 provides relatively strong evidence for the possibility of EC effects with subliminal CS presentations. However, four comments are in order. 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 discussed above) 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 (see Custers & Aarts, 2010). Second, because both facial-expression USs were negative, it is also unclear whether this finding reflects evaluative conditioning: The observed effects were derived from 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 that implemented such an on-line identifiation task, we found evidence for awareness of CSs during the learning phase under very 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 intentional versus incidental nature of learning via 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 visibility and to investigate, under a range of different conditions, whether EC can still be detected when CS visibility 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 conscious perception of CSs. The stricter the subliminality check, the stronger the evidence for subliminal EC, if it can be found.

Sensitive assessment of visibility. To be able to provide strong evidence for subliminal EC, we implemented a strict visibility check: We tested CS visibility 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 performed a visibility check after each presentation trial during the learning phase. This avoids potential motivational task-difficulty confounds (Pratte & Rouder, 2009) in the visibility-check task because participants successfully identified the CS on at least half of the trials. In addition, because visibility is tested on an item-by-item basis immediately after each presentation, we avoid confounds due to learning

effects that increase visibility of subliminal CS after repeated presentations (Atas et al., 2013). [2]

Experimental conditions supportive of implicit 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 intentional as well as 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, it is possible that awareness-independent EC effects operate only under incidental learning conditions (e.g., Olson & Fazio, 2001), whereas awareness-dependent EC may be found also under intentional learning conditions (Gast & Rothermund, 2011; Kattner, 2012). To address these possibilities, we investigated EC with subliminal CSs using a variety of

[2] Assessing visibility of CS presentations on each trial allowed us to separate those CSs that were sometimes visible from those that were never seen (a parallel approach has recently been developed by Van den Bussche et al., 2013). Subliminality, as well as EC effects, are typically assessed on an 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. However, these analyses were not reported for the research herein because we did not obtain significant EC for brief and masked CSs to begin with, so that there was no aggregate effect to analyze further. (Note also that – in contrast to the experimental manipulation of visibility in the focus of the present study – the classification of CSs as visible versus not-visible is post-hoc and analyses involving this post-hoc classification are merely correlational).

orienting tasks. In the present studies, we realized an intentional learning condition as well as three different types of incidental learning conditions. Importantly, however, all learning conditions were conceptually very similar to the surveillance paradigm reported by 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 choose the identified stimulus from a list of options.

The (intentional versus incidental) nature of the learning task was varied by manipulating attention to the CS-US pair and to the valence feature (see Table 1 for an overview). In the context of EC, intentional learning implies processing the CS and US as a pair while attending to the valence dimension. We varied the intentionality of the orienting task in several steps. On the intentional end of the dimension, we used a *valence-focus* task (Experiments 2 & 3) in which participants were asked to attend to the valence of the CS-US stimulus pair (Gast & Rothermund, 2011). In a more incidental *brightness-focus* task, we also asked participants to attend to and judge the CS-US stimulus pair, but the required judgment was on the perception of brightness instead of valence (Experiments 3 & 4). In another even more incidental orienting task, participants worked only on a stimulus *identification* task (similar to the surveillance paradigm by Olson & Fazio, 2001) in which they had to attend to both the CS and US stimuli because they were asked at the end of each trial which CS or US had been presented during the trial (Experiment 6). Finally, at the incidental end of the dimension, participants were asked only to identify the CSs, while the USs were introduced as background pictures and participants were free to ignore them (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 implicit measures (i.e., $0.46 < d < 0.49$) with high statistical power (i.e.,

$1 - \beta \geq .95$).[3] The present studies are therefore adequately powered to detect the effects of interest. In addition, we report the results of a meta-analysis across all of our studies in order to estimate the magnitude of EC effects under the different presentation conditions. This allows 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 visibility measures are used.

Overview of Experiments. In six experiments, briefly presented 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 a visibility check (i.e., CS identification task), (4) the visibility 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 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 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 intentional versus incidental nature of orienting task by comparing an orienting task that involved a valence judgment of the CS-US pair with an orienting task requiring a brightness

[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, dz, is equal to the familiar Cohen's d.

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 incidental 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 the Olson & Fazio (2001)'s surveillance paradigm (Experiment 6).

Table 1 gives an overview of the experiments. To foreshadow, contrary to the current view that EC effects may emerge independent from CS-US contingency awareness, we did not find any evidence for EC with briefly presented and masked CSs.

Experiment 1

In Experiment 1, 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 pre-test, 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 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 because 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 motivational 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 (and motivational stimulus) 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 told that they would see words and meaningless letter strings (nonwords) very briefly, and that they should try to identify them. They were told that this is a difficult task that 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 used simultaneous presentation 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. In the supraliminal condition, CSs were presented for 100 ms; in the subliminal condition, they were presented for 30 ms. The CS was 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.

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 an 8-point rating scale, 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, we used the mean of the three evaluative ratings as the dependent variable.[4]

Visibility. Participants' mean proportions of correct CS identifications were analyzed in a repeated-measures ANOVA with duration (30 vs. 100ms) as the only factor. Mean visibility was affected by presentation duration, $F(1, 130) = 1182.18$, $p < .001$, $\eta^2_G = .90$: 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 presentation duration (30 vs. 100ms) and US valence (positive, negative).

We obtained a main effect of presentation duration, $F(1, 130) = 18.66$, $p < .001$, $\eta^2_G = .07$, but not of US valence, $F(1, 130) = 0.00$, $p = .987$, $\eta^2_G = .00$, nor did we obtain an interaction of the two, $F(1, 130) = 2.28$, $p = .133$, $\eta^2_G = .00$. The significant presentation

[4]We used R (3.1.3, R Core Team, 2015) and the R-packages *BayesFactor* (0.9.11.1, Morey & Rouder, 2015), *coda* (0.17.1, Plummer, Best, Cowles, & Vines, 2006), *data.table* (1.9.4, Dowle, Short, Lianoglou, R Saporta, & Antonyan, 2014), *ez* (4.2.2, Lawrence, 2013), *Formula* (1.2.1, Zeileis & Croissant, 2010), *ggplot2* (1.0.1, Wickham, 2009), *HistData* (0.7.6, Friendly, 2015), *Hmisc* (3.17.0, Jr, Charles Dupont, & others., 2015), *lattice* (0.20.30, Sarkar, 2008), *lsr* (0.5, Navarro, 2015), *MASS* (7.3.39, Venables & Ripley, 2002), *Matrix* (1.1.5, Bates & Maechler, 2015), *meta* (4.3.0, Schwarzer, 2015), *metafor* (1.9.7, Viechtbauer, 2010), *papaja* (0.1.0.9054, Aust & Barth, 2015), *plotrix* (3.5.12, J, 2006), *plyr* (1.8.3, 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.

duration factor reflected a mere-exposure effect: 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], $t(130) = -0.93$, $p = .354$.

Discussion

First, results showed that participants could identify nonwords above chance level even if they were presented for only 30 ms and masked. Although identification performance was low, it was clearly above chance, indicating that, at least, some CSs could be 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, an automatic process should be able to operate on the CSs. We conclude that a necessary condition for EC to occur was therefore clearly met.

Experiment 1 failed to yield an EC effect: The 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 and less automatic than is typically 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 never failed to obtain EC (e.g., Stahl et al., 2009). Interpreting the present findings in light of these earlier results, CS identification appears to be necessary but not sufficient for EC.

This comparison also poses questions as to which aspects of the procedure were detrimental to EC. We suspected that the instructions' focus on the visibility check (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 with CS-US integration processes. In Experiment 2, we therefore changed three features of the procedure in order to render EC effects more likely: First, participants were instructed to focus on valence; second, USs were no longer described

as background pictures but were to be attended. Finally, we manipulated the presence versus absence of the identification task to test whether it interfered with EC.

Experiment 2

In Experiment 2, we explored whether EC for 30ms masked stimuli can be found when a valence focus is induced: EC effects may depend on an attentional focus on valence during learning (Gast & Rothermund, 2011). Experiment 2 also used a different visibility 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.

Importantly, to investigate whether the absence of EC in Experiment 1 was due to the visibility check, we manipulated the presence versus absence of the visibility check. Thus, the visibility check 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 check during the learning phase.

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 (visibility check: present vs. absent), with repeated measures on the first two factors. Half of participants were randomly assigned to the visibility-check condition, the other half did not perform the visibility check.

Materials. In Experiment 2, the same 24 nonwords as in Experiment 1 were used as CS (the words were no longer used 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 visible condition, and the other half was presented

in the invisible 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 unknown 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 told to attend to pictures and words and 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 visibility check 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, visibility was never tested, and the next trial commenced after the valence rating.

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 visibility-check group, it was followed by the visibility 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.

[5]Due to a programming error, identification performance in the first five blocks was overwritten by the computer program. 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).

Results

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. Thus, we used the mean of the three evaluative ratings as the dependent variable.

Visibility. Using the data from the visibility-check group, 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 visibility was affected by the mask, $F(1, 30) = 3014.99$, $p < .001$, $\eta^2_G = .99$: 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 visibility-check group as between-participants factor and masking and US valence as repeated-measures factors. The between-participants visibility-check factor was not significant, $F(1, 60) = 0.01$, $p = .927$, $\eta^2_G = .00$, and did not enter any significant interactions. We obtained main effects of masking, $F(1, 60) = 23.37$, $p < .001$, $\eta^2_G = .04$, and of US valence, $F(1, 60) = 29.99$, $p < .001$, $\eta^2_G = .09$, as well as an interaction between them, $F(1, 60) = 23.82$, $p < .001$, $\eta^2_G = .06$.

The main effect of masking reflects a mere-exposure effect (i.e., 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, $F(1, 60) = 31.98$, $p < .001$, $\eta^2_G = .20$, but not for masked CSs, $F(1, 60) = 1.10$, $p = .299$, $\eta^2_G = .00$ (see Figure 1).

Discussion

In this experiment, visibility was high for unmasked CSs and it approached significance for masked CSs (note here that the power of the visibility test was clearly lower than of the test of the EC effect, as only half of the participants entered this analysis). A robust EC effect was obtained for unmasked and clearly visible CSs. In contrast, there was no evidence for EC in the masked condition. Hence, EC seems to vanish when reaching the boundaries of conscious perception. As a further insight, this study reveals that the visibility check/identification task did not affect EC: the two groups had comparable levels of EC for unmasked CSs. Finally, these findings support the view that inducing a valence focus task promotes EC effect for visible CSs.

Experiment 3

In Experiment 3, we again tried to obtain an EC effect for briefly presented and masked CSs – that are still identifiable at above-chance levels. We also used this experiment to further examine the role of processing goals in EC by directly manipulating this factor. That an EC effect was obtained under valence instructions in Experiment 2 but was not obtained under identification instructions in Experiment 1 is consistent with the view that EC depends on processing goals (Corneille, Yzerbyt, Pleyers, & Mussweiler, 2009; Gast & Rothermund, 2011). In sum, Experiment 3 addresses two novel questions: Does EC obtain for masked CSs of short duration that reach the conscious perception threshold? And, if it does so, does this effect depend on processing goals activated prior to the learning phase? Experiment 3 tests these hypotheses by comparing 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.

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 told to attend to the picture-word pair and 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 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.

Results

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. Thus, we again used the mean of the three evaluative ratings as the dependent variable.

Visibility. 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 visibility was affected by the mask, $F(1, 55) = 2768.75$, $p < .001$, $\eta^2_G = .95$, but neither by orienting task, $F(1, 55) = 2.08$, $p = .155$, $\eta^2_G = .02$, nor the interaction, $F(1, 55) = 0.04$, $p = .847$, $\eta^2_G = .00$. 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 (i.e., 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 CS presentation 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, $F(1, 55) = 16.10$, $p < .001$, $\eta^2_G = .05$, and of US valence, $F(1, 55) = 13.48$, $p = .001$, $\eta^2_G = .04$, as well as an interaction between them, $F(1, 55) = 12.33$, $p = .001$, $\eta^2_G = .03$.

The main effect of masking reflects a mere-exposure effect (i.e., the finding that masked CSs, $M = 4.03, SD = 1.68$, were rated less positively than unmasked CSs, $M = 4.53, SD = 1.97$). 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, $F(1, 55) = 15.56$, $p < .001$, $\eta^2_G = .10$, but not for masked CSs, $F(1, 55) = 0.28$, $p = .597$, $\eta^2_G = .00$. The lack of an EC effect for masked CSs also holds when analyzed separately for the orientation task groups (valence: $F(1, 27) = 0.12$, $p = .732$, $\eta^2_G = .00$; brightness: $F(1, 28) = 1.36$, $p = .253$, $\eta^2_G = .05$).

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 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 intentional (valence) and incidental (brightness) orientation conditions.

In the next study, we attempted to further generalize the findings to the visibility

manipulation used in Experiment 1 (i.e., by manipulating presentation duration instead of masking). We also attempted to reduce the potential for interference with EC introduced by the orienting task: 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.

Experiment 4

In Experiment 4, we tested whether the above findings can be replicated when visibility is not manipulated via masking but via presentation duration: Here, the CSs were presented for either 30, 50, or 100 ms, and all CSs were masked. In addition, we investigated whether EC is disrupted by a response in the orienting task (or whether it depends on such a response): In this study, 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.

Results

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. Thus, we again used the mean of the three evaluative ratings as the dependent variable.

Visibility. 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 visibility was affected by duration, $F(2, 100) = 487.43$, $p < .001$, $\eta^2_G = .81$, but not by orienting response, $F(1, 50) = 0.17$, $p = .683$, $\eta^2_G = .00$, nor their interaction, $F(2, 100) = 0.02$, $p = .977$, $\eta^2_G = .00$. 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 CS presentation as well as US valence as repeated-measures factors. We obtained a main effect of CS presentation, $F(2, 100) = 25.04$, $p < .001$, $\eta^2_G = .14$, a non-significant effect of US valence, $F(1, 50) = 3.70$, $p = .060$, $\eta^2_G = .01$, as well as their significant interaction, $F(2, 100) = 4.41$, $p = .015$, $\eta^2_G = .01$. The presence vs. absence of an orienting response did not affect ratings, $F(1, 50) = 2.05$, $p = .158$, $\eta^2_G = .02$, nor did it interact with US valence, $F(1, 50) = 2.80$, $p = .100$, $\eta^2_G = .01$, or CS duration, $F(2, 100) = 0.78$, $p = .459$, $\eta^2_G = .01$; the three-way

interaction was also not significant, $F(2, 100) = 0.05$, $p = .953$, $\eta^2_G = .00$.

The main effect of CS presentation reflects a mere-exposure effect: 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 presentation: As can be seen in Figure 2, an EC effect was obtained only for CSs presented for 100 ms, $F(1, 50) = 8.76$, $p = .005$, $\eta^2_G = .05$, but not for CSs presented for 30 ms, $F(1, 50) = 0.00$, $p = .982$, $\eta^2_G = .00$, or 50 ms, $F(1, 50) = 0.12$, $p = .733$, $\eta^2_G = .00$.

Discussion

We again obtained EC effects only for clearly visible CSs but failed to find EC for briefly presented CSs. This extends our previous findings, which relied on manipulations of the presence versus absence of a mask, to a manipulation of CS duration when all stimuli were masked. Contrasting the notion that a response in the orienting task interferes with EC, we found no effect of the presence versus absence of the response requirement in the orientation task on visibility or EC. Given this finding, it is likely that orienting responses did not interfere with visibility 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 for either 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. We ensured that none of the participants took part in a previous study of this experimental line within the last year. We implemented a 3 (CS presentation duration: 30, 100, 1000 ms) by 3 (material: nonwords, faces, product images) by 2 (US valence: positive vs. negative) repeated-measures design.

Materials. We used a different 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 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.

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 (USs). All participants performed the identification task on all trials in which they were to select the CS from a list of 12 options (taken from the same set as the CS on that trial). In contrast to previous experiments, CS stimuli were only post-masked in this study (with post-mask duration computed 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 that was internally coded as ranging from -100 to +100.

Results

Visibility. Mean proportion of correctly identified CSs was analyzed in a CS duration (30, 100, 1000 ms) by material (nonword, face, product) repeated-measures

ANOVA. Mean visibility was affected by duration, $F(2, 120) = 2061.73$, $p < .001$, $\eta^2_G = .94$, and by material, $F(2, 120) = 18.79$, $p < .001$, $\eta^2_G = .05$, as well as their interaction, $F(4, 240) = 15.59$, $p < .001$, $\eta^2_G = .08$.

The main effect of 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, material, and US valence as repeated-measures factors. We obtained main effects of US valence (i.e., an EC effect), $F(1, 60) = 8.25$, $p = .006$, $\eta^2_G = .01$, and of material, $F(2, 120) = 11.63$, $p < .001$, $\eta^2_G = .05$, but not of CS duration, $F(2, 120) = 0.11$, $p = .897$, $\eta^2_G = .00$. As illustrated in Figure 2, the EC effect was qualified by an interaction with CS duration, $F(2, 120) = 6.40$, $p = .002$, $\eta^2_G = .01$: An EC effect was obtained only for CSs presented for 1000 ms, $F(1, 60) = 17.57$, $p < .001$, $\eta^2_G = .07$, but not for CSs presented for 30 ms, $F(1, 60) = 0.02$, $p = .895$, $\eta^2_G = .00$, or 100 ms, $F(1, 60) = 1.04$, $p = .313$, $\eta^2_G = .00$. The EC effect was not modified by the material factor.

The main effect of 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 material and CS duration, $F(4, 240) = 2.61$, $p = .036$, $\eta^2_G = .01$, 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 subliminal CSs was replicated for a different set of nonwords, as well as extended to faces and product images. CS visibility 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 and 100ms conditions.

Under similar conditions as in Experiment 1, Experiment 5 obtained an EC effect for CSs presented for longer durations (i.e., 1000ms) that are typical of EC studies. The study therefore replicates the finding from Experiment 1 that, when participants are instructed to focus on CS identification, the valence of irrelevant background pictures is not automatically registered and associated with CSs, even if those CSs are clearly visible in many cases (i.e., in the 100ms condition). This finding suggests that CS identifiation is not sufficient for EC to occur under incidental learning conditions, but that some minimal amount of attentional processing to the USs is necessary.

Experiment 6

In a final study, we attempted to realize an incidental learning situation similar to the surveillance paradigm by 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 on valence or on pairings, but 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 the authors interpret these findings as the result of an implicit-misattribution process by which US valence is misattributed to the CS (Jones, Olson, & Fazio, 2010). According to this account, an unaware EC-effect should occur when source confusability for the evaluative response on the US is maximized, and procedural features thought to maximise source confusability (and, by implication, an unaware 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. In Experiment 6, 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 attend to the CS, which was presented centrally and cued by the forward mask. CS and US stimuli were approximately of the same size (which should also increase CS saliency 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 4 (CS-duration: 30ms/6 pairings, 30ms/12 pairings, 100ms, 900ms) repeated-measures design, with a third factor (repetition: 6 vs. 12 CS presentations) 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 (i.e., between 4 and 5 on a scale ranging from 2 to 7); this was comparable for positive ($M = 3.92$) and negative USs ($M = 3.92$), 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. According to Jones et al. (2010), five pairings of a CS with different USs may not be sufficient to produce an unaware 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 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 consisted of the presentation of CS and US with a common onset, and 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 continuous scale ranging from "very unpleasant" (-100) to "very pleasant" (+100).

Results

Visibility. 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 visibility was affected by CS duration, $F(3, 177) = 2258.07$, $p < .001$, $\eta^2_G = .97$. 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 almost 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, $F(3, 177) = 9.94$, $p < .001$, $\eta^2_G = .08$, but only a non-significant tendency toward an effect for US valence, $F(1, 59) = 3.15$, $p = .081$, $\eta^2_G = .01$ (see Figure 2). The EC effect also did not interact with CS type, $F(3, 177) = 1.02$, $p = .384$, $\eta^2_G = .01$. The main effect of CS type can be interpreted as a mere-exposure effect: 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: $M = 11.12, SD = 4.07$). CS evaluations tended to vary as a function of the valence of the US (positive: $M = 10.62, SD = 4.04$; negative: $M = 10.17, SD = 4.18$).

Replicating previous findings, there was again a significant EC effect for CSs presented for longer durations (100 or 900ms), $F(1, 59) = 4.09$, $p = .048$, $\eta^2_G = .02$, but no EC effect for briefly presented CSs (30ms, 6 or 12 pairings), $F(1, 59) = 0.38$, $p = .538$, $\eta^2_G = .00$.

Discussion

Experiment 6 realized an incidental-learning paradigm for which EC effects in the absence of awareness (assessed as memory for CS-US pairings) have been repeatedly reported. Using this paradigm, we investigated the notion that the processes underlying such unaware EC effects are also responsible for EC under subliminal presentation conditions. Replicating previous findings in the present study, we obtained small EC effects for longer CS presentation durations. However, we again 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 any 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 EC may have been based on 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 show the EC effect as a function of the CS identification effect, to illustrate how non-zero EC effect sizes depend on clearly above-chance CS identification effects.

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

It could be argued that the article's central finding is based on a null effect (i.e., the absence of EC under suboptimal presentation conditions). Actually, the central finding relates to the presence of significant interactions: 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 by using a masking procedure or by reducing CS presentation duration. In addition, CS identification was significantly above chance in almost all the experiments reported here, such that presentation conditions allowed for a detectable amount of cognitive processing of the CSs. Finally, 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 present study in fact had adequate statistical power to test the hypothesis that EC is independent of conscious awareness, which implies that the size of the effect should not be modulated by awareness of CSs. 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 also 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 we realized.

Another way of addressing this statistical issue is to compute a Bayes-factor analysis that allows quantifying the evidence for the null hypothesis. Bayes factors are ratios relating the evidence of two hypotheses: They 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.[6] A meta-analytic Bayes

[6]We used a one-sided Cauchy distribution with scale parameter of .71, as recommended by Rouder, Speckman, Sun, Morey, & Iverson (2009). The Bayes factor depend on the exact choice of the alternative model (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

factor which combines the evidence of the brief presentation conditions of 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. Of course, 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 CSs, it is however unclear whether such high-power efforts would be worthwhile.

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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, $BF_{01} = 46.89$.

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.[7] As a measure for the magnitude of the EC effect, we computed Cohen's d for each condition.[8] 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 intentional learning conditions – in which attention was directed toward the CS-US pair (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 CSs.

[7] 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.

[8] 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.

Under intentional learning conditions (i.e., for attended 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, the mean incidental EC effect size (obtained under CS-identification orientation conditions) was small, $d = 0.20$ (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 (i.e., when participants attended only to individual CS/US stimuli); and they were medium-to-large for supraliminal CSs under more intentional learning conditions (i.e., when participants attended 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 visibility. 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 conscious CS visibility, 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 visibility is zero. In contrast, if EC requires CSs to be consciously visible 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 y-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 (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 conscious CS identification if EC effects are of greater magnitude than the respective CS identification effects.[9] In the figure, 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, we have investigated the relation between conscious CS identification and EC effects in qualitative manner. Dual-process theories of evaluative learning predict that EC effects are possible in the absence of awareness. Contrasting this prediction, both figures show that EC effects depend on above-chance visibility of CS stimuli – in fact,

[9]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).

above-chance visibility is necessary yet insufficient for EC.

General Discussion

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

Main findings

Across a series of experiments, we manipulated, within-subjects, the conscious visibility 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 visible (i.e., showed high identification rates). These findings suggest that participants can partially identify CS stimuli even under brief and masked presentation conditions.

EC effects were found in Experiments 2, 3, 4, and 5 for CS that were clearly visible – either because they were presented for a sufficient duration (100ms in Experiment 4; 1000ms in Experiment 5), or because they were not masked (Experiments 2 & 3). Despite above-chance levels of identification, no EC effects were found under brief and masked presentation conditions in Experiments 1-6. This finding was replicated with different techniques to reduce visibility (i.e., masking in Exp. 2 & 3; presentation duration in Exp. 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 task 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 visibility to low but still above-chance levels, EC effects were eliminated. This suggests that visibility is a

precondition for EC, such that EC does not occur in the absence conscious visibility of CSs.

Additional findings

Across studies, we manipulated the orienting task that participants performed during the learning phase. On the one hand, EC was not 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 pattern of EC effects across orientation tasks may be taken to suggest 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, Fazio, & Olson, 2009). If EC depends on the formation of some association between CS and US in memory, then such an association should be more strongly formed when CS and US are both attended to during learning and are therefore

more likely to be stored in an integrated trace or episode.

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 attention: The highest level was present in the valence-focus task where 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.

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 conditions, we did not find such subliminal EC effects. Of course it remains possible that EC effects will be found under different presentation conditions. In this section we briefly discuss conditions we deem relevant for investigation in future research.

Interindividual variablity in subliminal EC effects. It might be argued that participants differ greatly in their ability to detect and identify briefly presented stimuli, and this between-participant variability may have increased the variability of EC effects selectively in the subliminal condition, thereby masking potential small subliminal EC effects. Perhaps, with even greater statistical power, it is possible to detect EC with subliminal CSs. We believe this does not severely limit the interpretation of the present findings because the increased interindividual variability would also have affected CS identification performance, but the level of variability in the 30ms-masked condition was not sufficient to mask significant above-chance identification performance. In addition, empirically, variability was comparable in the shorter and longer duration conditions, as well as in the masked and unmasked conditions.

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, the valence experienced due to 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 CS 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 conscious visibility. Masking techniques such as metacontrast masking or continuous flash suppression 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 CS; 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 to be automatically singled out by our attentional system 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. We 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, 1984). However, it is possible that the identification task did not exlusively assess conscious processes, and that the above-chance identification performance obtained herein may be (in part or in whole) based on unconscious processes (Reingold & Merikle, 1988). 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

(Merikle, 1984). A correct response in the 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 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). Perhaps some CSs 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. The present data do not allow us to address this issue of subjective awareness; future studies should investigate this possibility.

EC may obtain for objectively subliminal CSs. A different model of unconscious processes – the objective threshold/strategic (OTS) model – 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 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 lies well 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, with increasing visibility 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 visibility 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 visibility 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 to do so 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). Some may argue, however, that indirect evaluative measures such as the IAT or the affective priming task are more likely to reveal implicit learning processes. 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 and propositional learning procedures to 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 implicit attitudes can be consciously accessed, allowing for conscious reports on direct evaluative measures (e.g., Gawronski & Bodenhausen, 2011, 2014a; Hahn, Judd, Hirsh, & Blair, 2014).[10] Second, dual-process proponents have repeatedly reported supportive evidence for implicit learning that was obtained using direct evaluative measures

[10]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.

(e.g., Hütter et al., 2012; Olson & Fazio, 2001). And the opposite is also true, with propositional 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 (or not) for successful EC when using indirect measures.

A similar comment applies to the pairing procedures used here. One proposed mechanism for implicit evaluative learning about a novel CS is that participants implicitly misattribute the evaluative experience caused by the US to the (neutral) CS (Jones et al., 2009). For this mechanism to operate, both CS and US stimuli should appear on screen simultaneously or in a 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, Van Osselaer, & Janiszewski (2010) proposed a similar mechanism for implicit 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 implicit 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 pairings were overlapping, and CSs were paired with USs of 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 than 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 interests of dual-process approaches to attitude learning. Third, and perhaps even more important, it is not even clear whether proponents of 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 latter gaze-shifting requirement may be implemented in a subliminal setting.

Given these open questions, our findings do not rule out the possibility of automatic 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 incidental as well as intentional 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 by Fazio and colleagues in the surveillance paradigm has repeatedly demonstrated EC effects under incidental learning conditions (e.g., Olson & Fazio, 2001). In their work, both CSs and USs were clearly visible, and participants were required to attend to the stimuli (in order to detect the presence of a specific target picture). However, participants reported being unaware of the

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). These EC effects may reflect preconscious learning because, while participants processed clearly visible CS and US stimuli, they apparently remained unaware of the CS-US pairing.

For practical purposes, the meta-analysis of the present findings suggests that EC effects are small but reliable under incidental learning conditions – that is, in situations in which participants are not processing the CS and US stimuli as a pair nor are attending to the valence of the US. In such situations, participants 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 conditions, participants' evaluations of these novel CSs are susceptible to modulation by the valence of the paired USs, these US valence effects could be characterized as automatic in the sense that they occur unintentionally, that is, in the absence of an explicit intention to learn.[11] Such incidental and unintentional effects of US valence are at the basis of advertising effects. 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 lead to 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.

[11]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

Orienting conditions implemented in the present study

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

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

Table 2

Overview of experiments

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

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

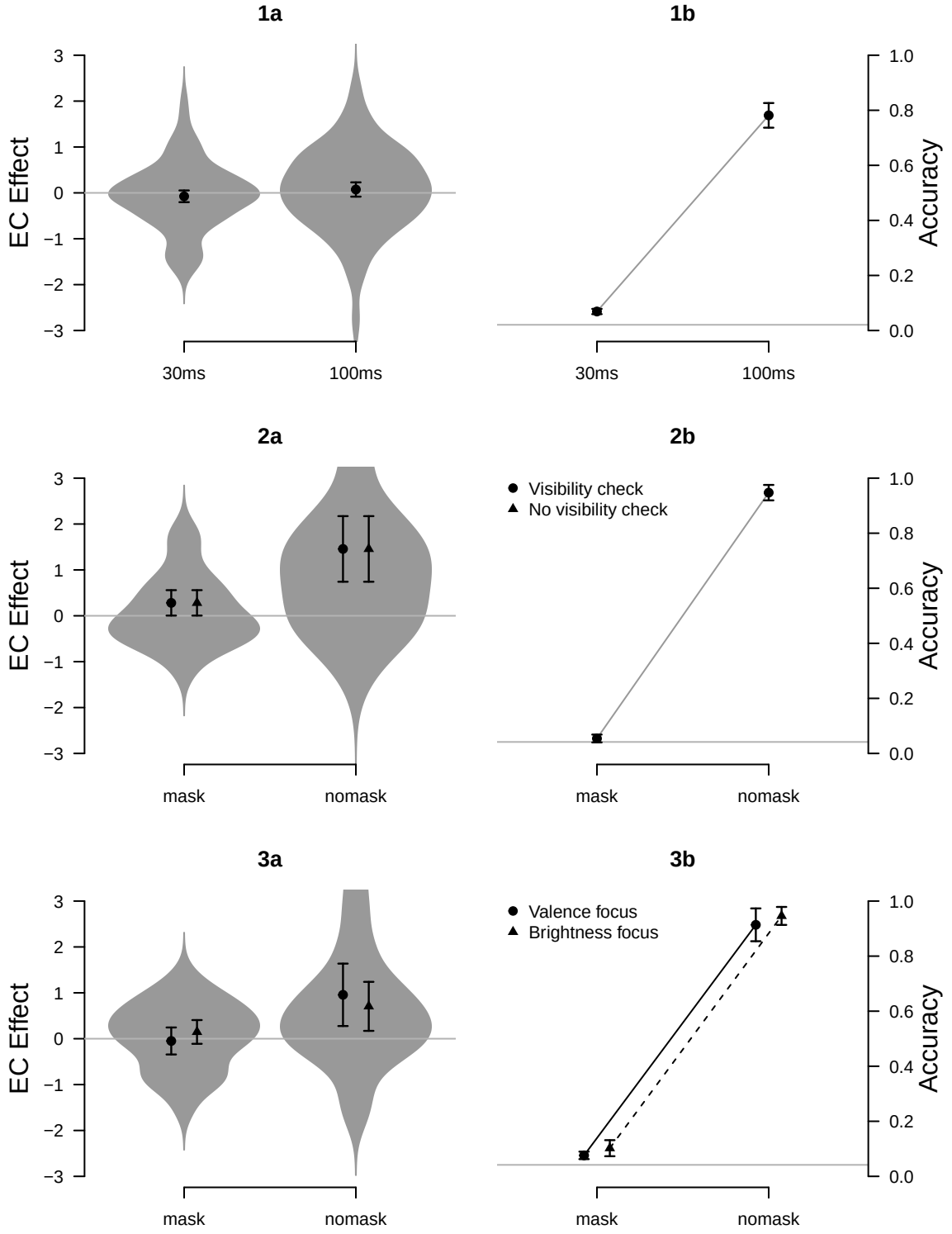

Figure 1. EC effects (left panels, a) and visibility (right panels, b) for Experiments 1-3. Violin plots (background) show EC effects for individual participants (i.e., the width of the violin plot reflects the density of the distribution of EC effects). Group means (and 95% CIs) are superimposed in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

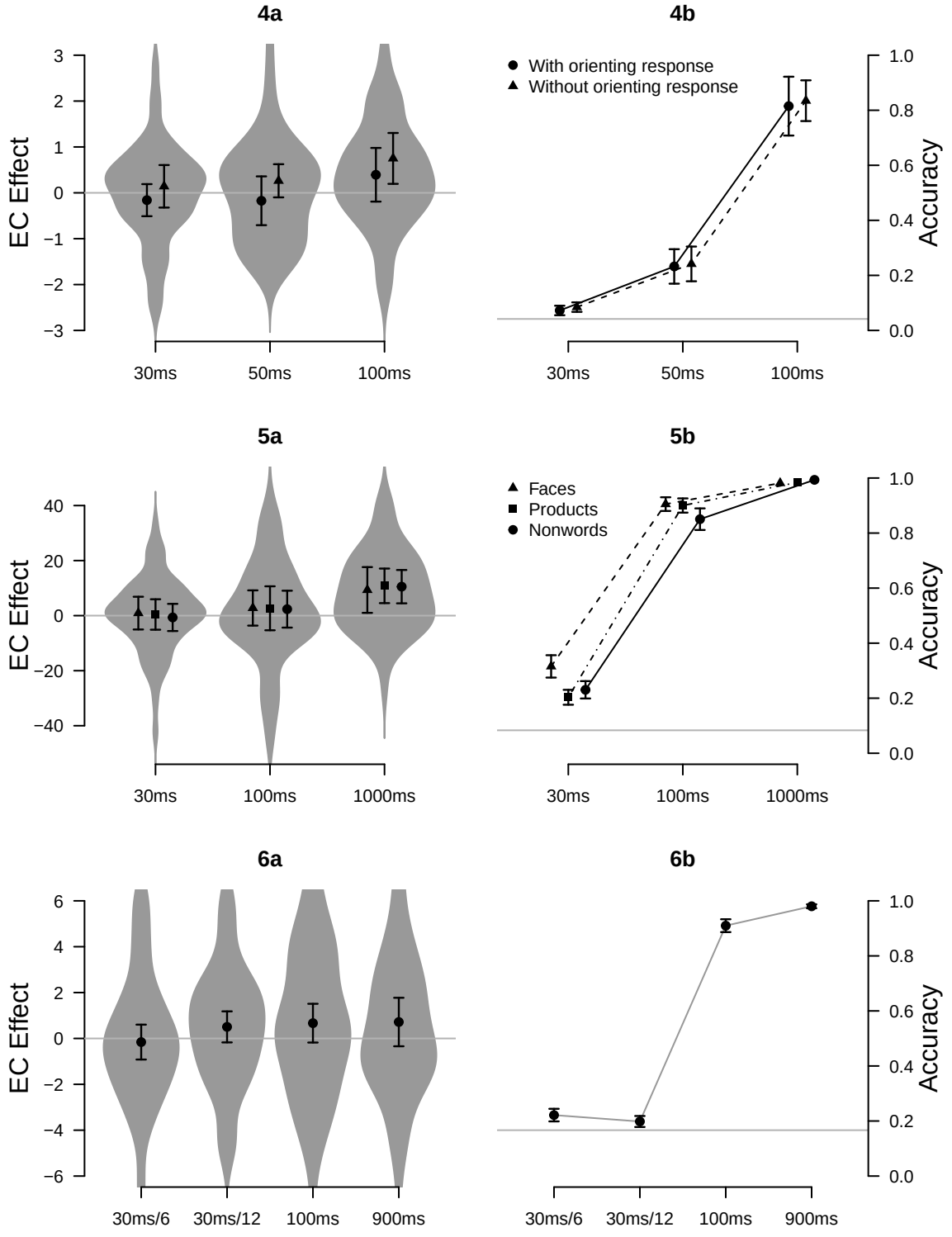

Figure 2. EC effects (left panels, a) and visibility (right panels, b) for Experiments 4-6. Violin plots (background) show EC effects for individual participants (i.e., the width of the violin plot reflects the density of the distribution of EC effects). Group means (and 95% CIs) are superimposed in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

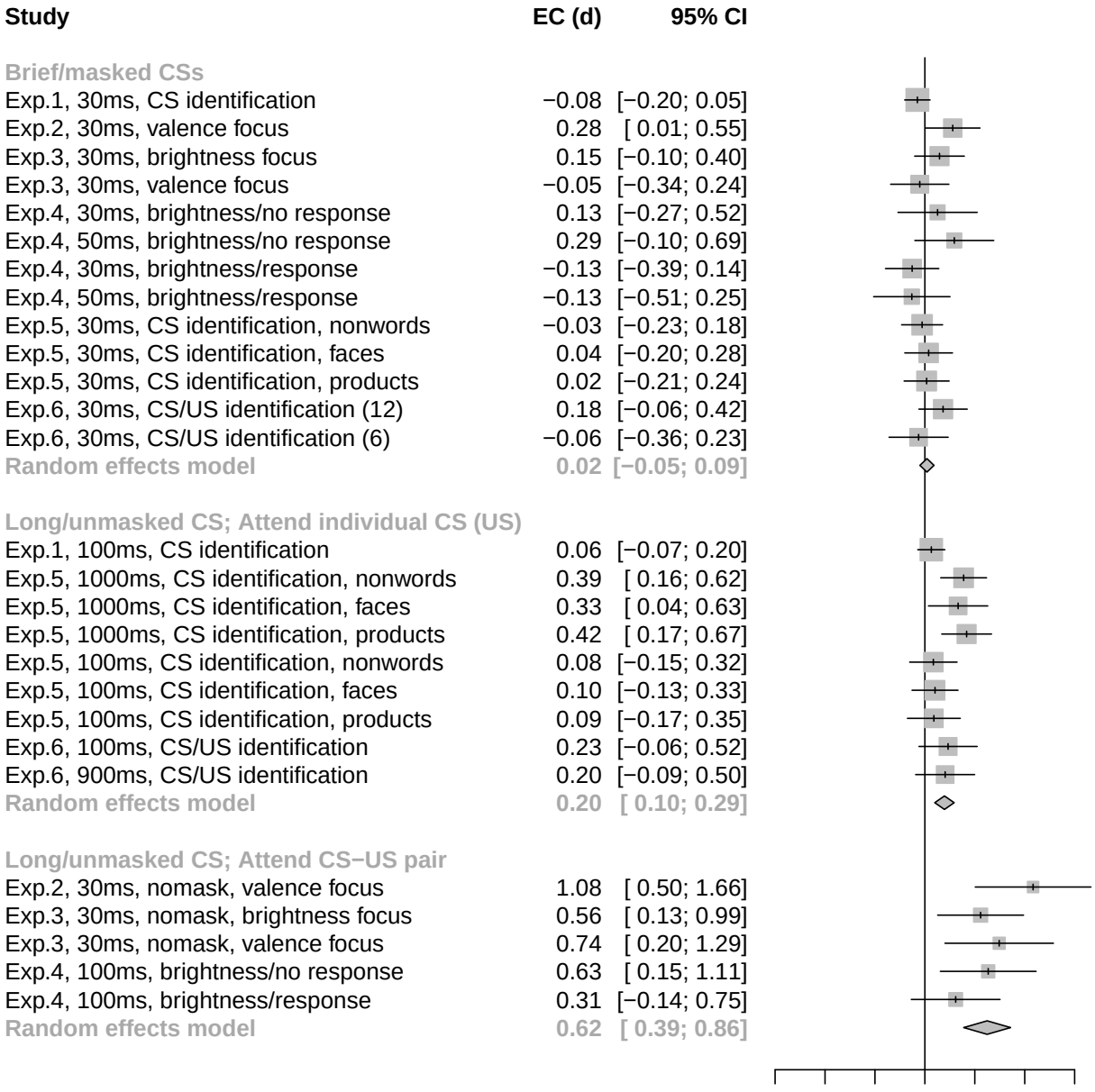

Figure 3. Meta-analysis of EC effects in the present study (homogeneous subsets)

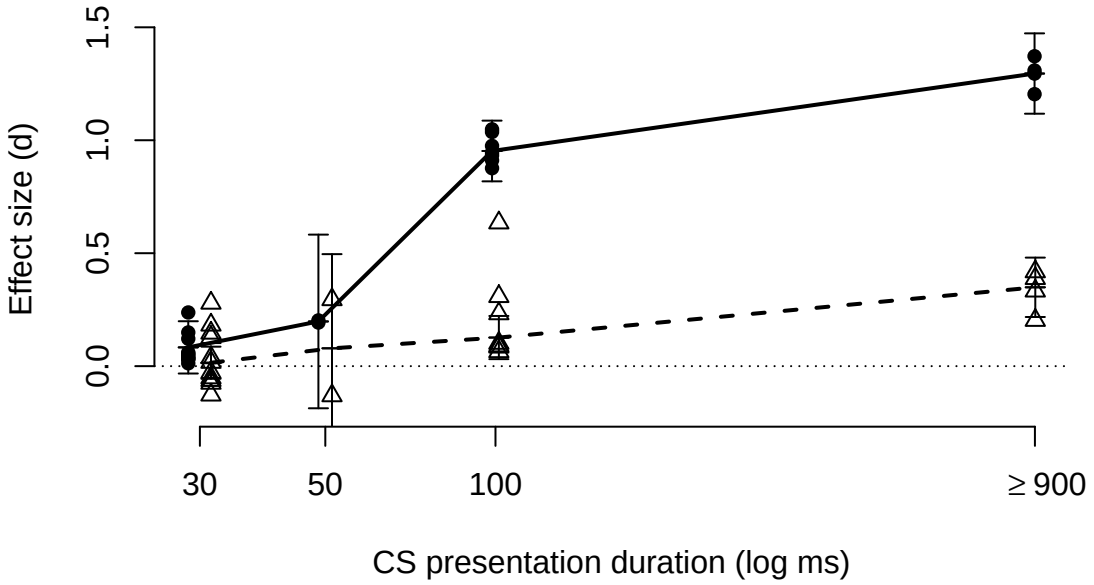

Figure 4. CS identification (circles; solid line) and EC effects (triangles; dashed line) by CS duration. Symbols represent effects of individual studies; lines represent meta-analytic means (and 95% CIs). Brief non-masked presentation conditions ($k = 3$) are not depicted.

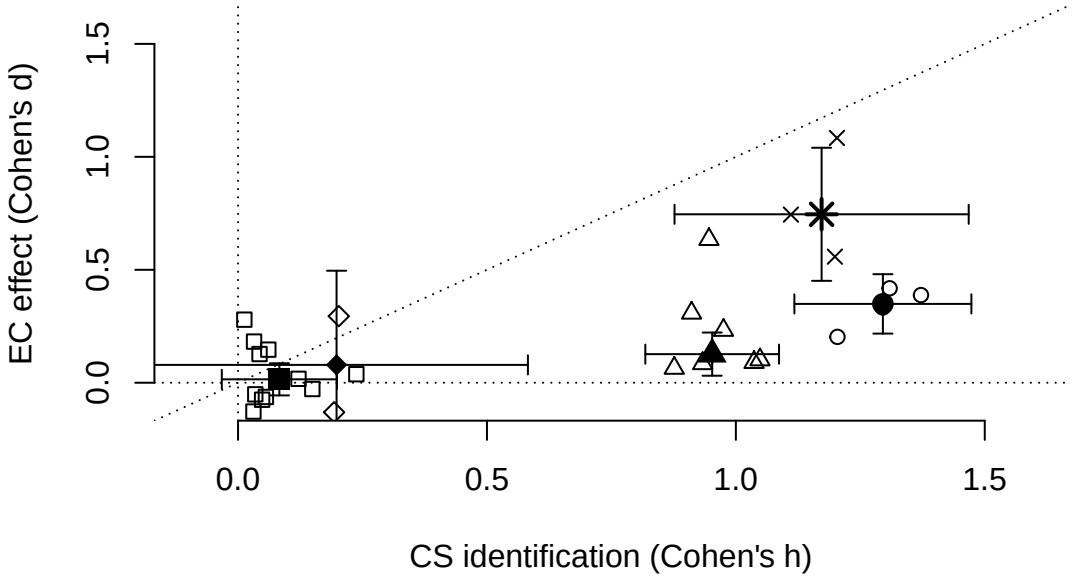

Figure 5. EC effects as a function of CS visibility (with meta-analytic means and 95% CIs; squares: 30ms; diamonds: 50ms; triangles: 100ms; circles: 900/1000ms; crosses: 30ms non-masked).