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Does Affective Processing Require Awareness?

On the Use of the Perceptual Awareness Scale in Response Priming Research

Dirk Wentura[1*] Michaela Rohr[1*]

Markus Kiefer[2]

[1]Saarland University, Saarbrücken, Germany

[2] Ulm University, Germany

*Both authors contributed equally to this work

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Author Note

Dirk Wentura and Michaela Rohr, Department of Psychology, Saarland University, Saarbrücken, Germany. Markus Kiefer, Section for Cognitive Electrophysiology, Department of Psychiatry, Ulm University, Germany.

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Abstract

Masked priming paradigms are frequently used to shine light on the mechanisms and impacts of non-conscious cognition. Introducing a new method to this field, Lähteenmäki, Hyönä, Koivisto, and Nummenmaa (2015) claimed that affective priming requires awareness. In the present article, employing an improved methodological approach, we refute their claim. Specifically, Lähteenmäki et al. administered a subjective rating task (Ramsoy & Overgaard, 2004) after the priming task in each trial to directly assess awareness of the masked prime stimulus, and they used different prime durations (10 ms vs. 80 ms) to produce variance in subjective awareness ratings. Their main result was a lack of priming for subjectively unaware primes. However, their methods can be criticized on several grounds, most importantly their choice of parameters (esp. prime durations) and the lack of an essential analysis, namely a linear mixed model (LMM) analysis that pits prime duration and awareness ratings against each other. We report two experiments with different prime durations (i.e., 20, 40, 60 ms), analyzed with LMM, and additionally present a reanalysis of the Lähteenmäki et al. data with LMM. Our results show significant priming effects for subjectively unaware primes. Moreover, in contrast to Lähteenmäki et al. (2015), we draw primes and targets from different sets, such that priming effects can be unequivocally attributed to the processing of evaluative features of the masked primes. Based on our results, we conclude that the claim by Lähteenmäki et al. that affective processing requires awareness is not justified.

Keywords: visual awareness, affective priming, evaluative priming, masked presentation

Does Affective Processing Require Awareness?

On the Use of the Perceptual Awareness Scale in Response Priming Research

Masked priming paradigms are frequently used to elucidate the influence of non-conscious cognition and its underlying mechanisms (Van den Bussche, Van den Noortgate, & Reynvoet, 2009). In these paradigms, participants categorize visible target stimuli that are preceded by task-irrelevant primes that are presented very briefly and masked. The prime is typically found to influence the response to the target even under conditions promoting non-conscious prime processing. Numerous studies have demonstrated reliable masked priming effects in a variety of cognitive domains such as semantic (Kiefer, 2002; Marcel, 1983), evaluative (Klauer, Eder, Greenwald, & Abrams, 2007; Spruyt, De Houwer, Everaert, & Hermans, 2012; Wentura & Degner, 2010a), or visuo-motor processing (Vorberg, Mattler, Heinecke, Schmidt, & Schwarzbach, 2003; Zovko & Kiefer, 2013). This suggests that the concept of non-conscious cognition has a broad scope. However, the methods to assess unawareness of the prime stimuli have been a matter of fierce debate almost since the beginning of this line of research (Greenwald, Klinger, & Schuh, 1995; Klauer, Greenwald, & Draine, 1998; Merikle, Smilek, & Eastwood, 2001; Schmidt & Vorberg, 2006; Wiens, 2006).

Recently, Lähteenmäki, Hyönä, Koivisto, and Nummenmaa (2015) proposed new ways of tackling the contentious question of whether it is possible to reliably demonstrate that stimuli can be evaluatively or semantically processed without awareness. On a methodological level, they criticized the prototypical way of exploring this question, viz. use of post-task awareness measures, and suggested an alternative, namely a trial-by-trial awareness rating procedure (Ramsoy & Overgaard, 2004). On a theoretical level, they emphasized the difference between processing non-emotional semantic features (e.g., animacy) and affective/evaluative semantic features (i.e., pleasantness). Based on their experiments, they concluded “that both implicit and explicit affective and semantic categorization is dependent on visual awareness, and that affective recognition follows semantic categorization" (p. 339).

Lähteenmäki et al.'s (2015) article is inspiring and provided many fruitful insights. However, because the authors were very definitive about their conclusions ("Affective [and semantic] processing requires awareness"; see title and abstract), we felt inclined to challenge the validity of the empirical basis of this claim, at least for the part that is related to the *implicit* processing of affective and semantic information. We had two reasons for this: First, in order to show that *implicit* affective and semantic categorization is dependent on visual awareness, the authors employed the response priming technique in its most common instantiation, the evaluative priming paradigm;[1] however, they employed task parameters that are unusual in masked evaluative priming research, that is, either very short or relatively long prime durations, long SOAs, and primes and targets that came from the same set. Second, they failed to separate prime duration and prime awareness in their analyses. It follows that their experiments were probably not an adequate test of their hypothesis, and thus their far-reaching conclusions may not be warranted. In the following, we expand on these points, and present a re-analysis of Lähteenmäki et al.'s data applying a linear mixed-model approach that allows for a concurrent examination of the separate contributions of prime duration and prime awareness. We then present new empirical data corroborating our assumption that affective processing does not require awareness.

The Standard Technique and its Critique

The standard method of exploring the question of whether *implicit* affective categorization depends on awareness involves two tasks featuring stimuli that are presented

[1] Note, this paradigm is also known as the affective priming paradigm.

briefly and masked. The first task (administered in what is often referred to as the “test phase” of the paradigm) aims to provide evidence for the processing of semantic or evaluative features despite the brief and masked presentation and implicit processing (see, e.g., Kiefer, 2002; Kiefer & Martens, 2010; Rohr, Degner, & Wentura, 2012; Schmidt & Vorberg, 2006). Typically, this task uses a priming method; that is, the briefly-presented masked stimulus (the “prime”) is followed by a second stimulus, which is the target of the participant’s task, while the prime is task-irrelevant and can be ignored. Specific influences of the prime stimulus on target processing, evident in reaction time or accuracy differences, are regarded as evidence for prime processing. We will elaborate on this below. The second task, which typically follows the first task, aims to provide a test of prime awareness (it is thus often referred to as the “direct test”). The presentation sequence is almost identical to the first task; however, the task now relates to the prime, and participants are instructed to make some judgment regarding the prime in order to obtain evidence for awareness (or non-awareness) of the task-relevant prime feature.

Evidence from the two tasks can be combined in various ways. Typically, non-conscious processing is inferred if priming effects (i.e., reliable effects in the first task) can be found in the absence of awareness in the second task (e.g., d' = 0 for prime-feature discrimination; see Schmidt & Vorberg, 2006; Wiens, 2006); that is, an objective discrimination threshold is taken as the criterion of unawareness. However, this method has been criticized in several ways. For example, the approach does not account for the possibility that participants become aware of the prime on some trials but not others; the method also ignores inter-individual variability in awareness if a fixed prime duration is used and scores are calculated for the whole sample. Objective measures are often also criticized for ignoring the fundamentally subjective nature of consciousness (Wiens, 2006). Due to these criticisms, several alternative propositions have been put forward (Greenwald et al., 1995; Klauer et al., 1998; Merikle et al., 2001; Reingold & Merikle, 1988; Schmidt & Vorberg, 2006; Wiens, 2006), including the within-test-phase rating scale employed by Lähteenmäki et al. (2015) and similar variants (Dienes & Seth, 2010; Ramsøy & Overgaard, 2004; Sandberg, Timmermans, Overgaard, & Cleeremans, 2010; Szczepanowski & Pessoa, 2007; Szczepanowski, Traczyk, Wierzchoń, & Cleeremans, 2013; Timmermans, Sandberg, Cleeremans, & Overgaard, 2010; Zehetleitner & Rausch, 2013).

Indeed, one of the main points made by Lähteenmäki et al. (2015) was that the sequential two-task procedure with fixed stimulus presentation times is inadequate. The authors argued that this technique does not control for trial-wise awareness; as such, supposed evidence for non-conscious processing might be the consequence of averaging across conscious trials (that show the priming effect) and non-conscious trials (that do not show the effect). Furthermore, they argued that fixed prime and mask durations do not take into account inter-individual variability in awareness (if awareness is only calculated over the entire sample). Thus, the authors proposed using an individual trial-by-trial awareness measure. Specifically, they suggested administering the perceptual awareness scale (PAS; Ramsøy & Overgaard, 2004) at the end of each test-phase trial of the priming task; this scale asks for a graded subjective-awareness evaluation of the masked stimulus (from 1 = “I did not see the stimulus at all”; 2 = “I saw a glimpse of something, but don’t know what it was”; 3 = “I saw something, and I think I can determine what it was”, to 4 = “I saw the stimulus clearly”.

Lähteenmäki et al. (2015) used this technique to explore (a) whether evaluative and semantic features of masked stimuli are processed outside of awareness and (b) whether there might be prioritized processing of evaluative features over (other) semantic features. Their main conclusion was that (p. 361) “affective categorization requires both visual awareness and preceding semantic categorization.”

The empirical section of Lähteenmäki et al.'s (2015) article, which underpinned their conclusion, can be separated into two parts. Experiments 1 to 3 explored the relationship between the awareness rating of the masked primes and *explicit* forced-choice recognition or semantic categorization of the evaluative targets. Experiments 4 and 5 employed the priming technique, that is, an indirect ("implicit") assessment of the processing status of the masked stimuli (i.e., whether the prime influenced target processing) was put into relation to the awareness rating. It is the second empirical part in particular that motivated our research. This is because, from a masked-processing perspective (see below), the second part is especially relevant, as it links an objective indirect measure of stimulus processing (i.e., the priming effect) to the awareness rating. It is typically assumed that direct access (e.g., in explicit forced-choice recognition necessitates conscious awareness, whereas non-conscious processing can take place indirectly (e.g., Merikle & Reingold, 1991).

The Assessment of Masked Stimulus Processing

Experiments 4 and 5 of Lähteenmäki et al. (2015) used a standard evaluative priming paradigm, in which prime and target either share the same valence (congruent trials) or they do not (incongruent trials). Participants have to categorize the target according to valence. Typically, a congruency effect is found, that is, responses are faster and/or more accurate for congruent compared to incongruent trials. Since its introduction by Fazio, Sanbonmatsu, Powell, and Kardes (1986), hundreds of published studies have used this paradigm (see Herring et al., 2013, for a meta-analysis). Of particular relevance in the present context, the evaluative priming paradigm has often been used to explore whether very briefly presented and masked primes are still processed (i.e., elicit a priming effect; e.g., Draine & Greenwald, 1998; Kiefer , Liegel, Zovko, & Wentura, 2017; Kiefer, Sim, & Wentura, 2015; Klauer et al., 2007).

In this regard, the evaluative priming paradigm is a version of a more general paradigm, namely the response priming paradigm. In response priming experiments, targets are categorized in a binary choice task (e.g., positive vs. negative). Primes differ on the same dimension, and the critical variation is whether prime and target are associated with the same response category (congruent trials) or not (incongruent trials; see, e.g., Wentura & Degner, 2010b, for a discussion of response priming versus semantic priming). The response priming paradigm is one of the standard tasks to explore masked priming effects (e.g., see Kiefer, 2002; Kiefer & Martens, 2010, for pure, non-response-based masked semantic priming effects). For example, the paradigm has been used with simple geometric symbols as stimuli (e.g., left vs. right-pointing arrows, Vorberg et al., 2003; geometrical shapes, Martens, Ansorge, & Kiefer, 2011), but also with semantic categorization (e.g., “Is the target number greater or less than 5?, Dehaene et al., 1998; “Is the target animate or inanimate?”, Klinger, Burton, & Pitts, 2000; “Is the target name male or female?”, Draine & Greenwald, 1998). From this perspective, evaluative categorization is only one out of several semantic categorization tasks used within the response priming paradigm, although it is one that has found considerable interest (e.g., Draine & Greenwald, 1998; Hermans, Spruyt, De Houwer, & Eelen, 2003; Kiefer et al., 2015; Klauer et al., 2007; Klinger et al., 2000; Wentura & Degner, 2010a).[2]

As mentioned earlier, subliminal effects are typically tested with a priming task followed by a direct test that is an almost exact repetition of the priming-task sequence with the added instruction to categorize the prime. If a significant priming effect is obtained, there are several ways to infer subliminal prime processing based on the direct test (Draine & Greenwald, 1998; Schmidt & Vorberg, 2006). The most straightforward approach is to

[2] The diversity of semantic categorizations is also reflected in Lähteenmäki et al.’s (2015) comparison of evaluative categorization with non-evaluative semantic categorization (“animal or object?”); we will return to this issue in the *General Discussion*.

establish chance-level discrimination in the direct test (see, e.g., Ortells, Mari-Beffa, & Plaza-Ayllon, 2013; Schmidt, 2007; Schmidt & Vorberg, 2006, for discussions of this approach). Given the criticisms directed at this approach, Lähteenmäki et al. (2015) proposed an alternative procedure, and asked (some of) their participants to rate prime awareness with the PAS scale directly following target categorization. What did they find? In a nutshell, there was no evidence of priming in trials associated with PAS ratings of 1 ("I do not see the stimulus at all") or 2 ("I saw a glimpse of something, but don't know what it was"). In other words: priming effects were restricted to trials with PAS ratings 3 ("I saw something, and I think I can determine what it was") and 4 ("I saw the stimulus clearly")—that is, trials with partial or complete awareness. The notion that priming effects may emerge only on trials associated with some level of prime awareness presents a challenge for researchers who follow the research tradition of linking priming results with direct prime-categorization results, assuming that non-conscious processing exists.

It is important to note that had the null findings of Lähteenmäki et al. (2015) been restricted to PAS level 1 (i.e., had level 2 yielded a priming effect), most researchers using the more traditional technique would have presumably just shrugged their shoulders. At least implicitly, they allow for the possibility that participants subjectively perceive a "glimpse" of the prime stimulus—what counts is that they cannot consciously recognize any task-relevant content. This stance can be inferred from the fact that direct prime categorization tests rarely (if ever) ask for a binary categorization of stimulus "present" versus "absent". Rather, categorization is always based on some (albeit often superficial) stimulus attribute (e.g., shape or letter form).

This relates to critical evaluations of the perceptual awareness scale in the literature on consciousness and masking (Dienes & Seth, 2010; Schmidt, 2015; Zehetleitner & Rausch, 2013), as well as debates regarding the adequate assessment of phenomenal consciousness (e.g., Rausch & Zehetleitner, 2016). In this vein, some researchers have pointed out that the awareness rating scale does not exclusively target the relevant stimulus features, but instead any visual features (Rausch & Zehetleitner, 2016). Some authors have argued that use of the PAS disregards differences between potentially irrelevant conscious visual experience and relevant conscious visual content (Dienes & Seth, 2010). Zehetleitner and Rausch (2013, p. 1424) critically stated that the scale "might measure a larger set of experiences … because it requires participants to report experiences without content as well, which could also be nonvisual intuitions." Moreover, the PAS assesses sensory (e.g., a color impression) as well as non-sensory conscious content (e.g., a feeling of familiarity), and participants seem to also include task-irrelevant experiences in their subjective reports (Rausch, Muller, & Zehetleitner, 2015). Therefore, in our view, a rating like "I saw a glimpse of something, but don't know what it was" should not be considered evidence of phenomenal awareness of the masked prime, because it is unclear what this impression devoid of content actually is. Finally, Schmidt (2015) argued that a simple one-to-one correspondence of the scale and distinct states of (un)awareness cannot be assumed, as—similar to objective measures based on signal detection theory—both visual sensitivity and response biases need to be taken into account.

This critique is supported by an interesting detail in the results of Lähteenmäki et al. (2015) relating to the signal detection sensitivities reported for the shortest prime duration (i.e., 10 ms). To calculate these, Lähteenmäki et al. classified any PAS rating greater than 1 as a hit, and obtained false alarm rates using catch trials (i.e., trials without prime presentation), counting any PAS rating greater than 1 as a false alarm. For the priming experiments, false alarm rates ranged from 0.24 to 0.59.[3] Unless we assume that participants had hallucinations (which might subjectively justify ratings of 3 or 4), these false alarm rates suggest that participants had some difficulty neatly differentiating PAS levels 1 and 2 in particular. Most importantly, these comparatively large false alarm rates imply that a considerable proportion of PAS = 2 ratings were invalid; it therefore seems inadequate to take a PAS rating of 2 as a marker of conscious subjective impressions elicited by prime stimulus presentation. In this regard, we consider the step from PAS = 2 ("I saw a glimpse of something, but don't know what it was") and PAS = 3 ("I saw something, and I think I can determine what it was") as the qualitatively important demarcation line because the transition marks the difference between being unaware versus aware of the content of the stimulus.

We are very explicit about this detail at this early point of the article because parts of the article by Lähteenmäki et al. (2015) read as if the critical demarcation line for the question of whether stimulus processing is dependent on awareness is between PAS rating 1 („did not see the stimulus at all") and subsequent levels. However, with regard to the priming effects, the abstract (p. 339) summarizes: "When participants reported no awareness of the stimuli, … priming scores did not differ from zero. When participants were even partially aware of the stimuli, … both semantic and affective priming were observed." Definitely, the second sentence is only true (given the logic of the authors) if it refers to PAS levels 3 and 4 (and not level 2 because there was no priming). If the first sentence refers only to PAS level 1, this would, however, mean that one important condition (i.e., PAS level 2) is left out of the description. Certainly, the question of whether or not a non-identifiable glimpse should be considered a conscious experience of that stimulus is a general one. In this regard, we hold the

[3] The false alarm rates were not reported in the article; however, they can be calculated on the basis of the reported *d'* values and hit rates.

same opinion as some researchers in the field of consciousness (e.g., Zehetleitner & Rausch, 2013), who argue that claims of conscious experience should be reserved to cases where at least some of the stimulus content can be identified.

Thus, to summarize, Lähteenmäki et al. (2015) finding that priming effects are restricted to trials with PAS rating levels 3 and 4—that is, trials with partial awareness—presented a challenge to the field. We address this challenge in the present paper, focusing primarily on Lähteenmäki et al. (2015) juxtaposition of PAS levels 1 and 2 on the one hand and levels 3 and 4 on the other hand, for two reasons: First, catching a (valid) “glimpse” of a stimulus is not equivalent to conscious processing of a task-relevant feature. Second, the false alarm rates in Lähteenmäki et al. call into question the validity of the level 1 versus 2 distinction. Given this backdrop, we will argue that the priming experiments by Lähteenmäki et al. (2015)are characterized by some questionable parameters, and that a crucial analysis is missing from their investigation. Based on additional experimentation, we will then assert that it was premature to propose that masked priming effects can only be found for partially or fully aware primes (i.e., PAS levels 3 and 4).

A Critique of the Priming Experiments by Lähteenmäki et al. (2015)

There are a number of procedural factors in the priming experiments by Lähteenmäki et al. (2015) that provide grounds for criticism.

The Choice of Timing Parameters

The two priming experiments of Lähteenmäki et al. (2015) varied prime duration (10 ms vs. 80 ms) and stimulus onset asynchrony (SOA; 150 ms vs. 300 ms) orthogonally. Given the literature on masked evaluative or semantic priming, both choices seem suboptimal if the aim was to elicit reliable priming effects.

Stimulus onset asynchrony. Lähteenmäki et al. (2015) wrote on p. 351: “Because we wanted to test for the primacy of affective processing, prime-target SOAs were chosen to maximize the sensitivity for affective priming.” Had Lähteenmäki et al.used an *unmasked* evaluative priming paradigm, an SOA of 150 ms would have indeed been a reasonable choice; however, an SOA of 300 ms—although often used in the literature—would have arguably been a poor choice even for an unmasked paradigm (see Hermans, De Houwer, & Eelen, 2001), let alone a *masked* priming study. For example, Greenwald, Draine, and Abrams (1996) found a rapid decline in masked priming effects for SOAs exceeding 100 ms (see also Kiefer & Brendel, 2006; Kiefer & Spitzer, 2000). Thus, for a conceptual replication, a shorter SOA should be used.

Prime duration. Even more important is the choice of prime durations in Lähteenmäki et al. (2015). The main result was that there was no significant priming at 10 ms, but significant priming at 80 ms. According to the PAS ratings, there was no awareness in the 10 ms condition but a considerable amount of awareness in the 80 ms condition. These results are fully in line with expectations given the literature on visual masking (Bachmann & Francis, 2014; Breitmeyer & Ögmen, 2006). However, the chosen prime durations of 10 ms or 80 ms are rarely used in masked priming studies to investigate subliminal processing: A meta-analysis by Van den Bussche et al. (2009; Appendix A) reported a mean duration of 42 ms (*SD* = 11.2 ms; range 10 to 72 ms) across 88 studies. A prime duration of 80 ms is typically associated with full awareness, whereas a duration of 10 ms might be too short to reliably trigger non-conscious processes at all because of very limited stimulus energy (see, e.g., Vorberg et al., 2003). Thus, studies aiming to find subliminal effects would usually use prime durations that lie in-between the values chosen by Lähteenmäki et al. To prevent misunderstandings, we hasten to add that it is of course always the combination of prime duration, chosen masking procedure, and stimulus characteristics that determines the grade of (non-)awareness. However, we wanted to highlight that the choice made by Lähteenmäki et al. strongly biased the 10 ms condition toward a null effect and the 80 ms condition toward prime awareness. We therefore suggest that intermediate prime durations would be desirable for a conceptual replication.

PAS Ratings, Prime Duration, and Priming: A Strong Assumption Made but not Tested

Setting prime durations at 10 ms versus 80 ms had one additional implication: Lähteenmäki et al. (2015) insinuated that these extreme prime duration values produced the within-condition awareness variance needed to apply their analysis of choice, that is, to analyze priming effects as a function of PAS ratings. The 10 ms prime duration was predominantly associated with low awareness ratings (i.e., rating levels 1 and 2, although a minority of trials was associated with high ratings), whereas the 80 ms duration was predominantly associated with high awareness ratings (i.e., rating levels 3 or 4, although a minority of trials was associated with low ratings). The authors further suggested that varying prime duration is simply a means to generate variation in awareness, and that awareness ratings of 1, 2, 3, or 4 always denote the same awareness and processing status of the prime, irrespective of prime duration. This implicates that after obtaining the PAS rating, prime duration should be a dispensable predictor of priming. If so, however, prime duration should be redundant in a multiple regression with PAS as a competing predictor, and it should not moderate the relationship between priming and PAS.

This is a strong hypothesis given that, first and foremost, prime duration (10 ms vs. 80 ms) in Lähteenmäki et al. (2015) was a significant predictor of priming scores (see p. 352 for Exp. 4 and p. 354f for Exp. 5). Thus, it is conceivable that the correlation of prime awareness and priming scores was simply a spurious correlation, caused by variation in prime duration. Moreover, it is conceivable that prime duration moderated the relationship between prime awareness and priming scores. PAS ratings might not denote the same degree of awareness or prime processing for different prime durations. In our view, a more appropriate analysis would have been a linear mixed-model analysis (LMM) with “congruency status”, “level of awareness”, “prime duration”, and their interactions as predictors and response times (RT) as the criterion (see, e.g., Sandberg et al., 2010).

A re-analysis of Lähteenmäki et al. (2015) Experiments 4 and 5 is presented in the Supplement. In a nutshell, LMM analyses of both experiments showed a three-way interaction of awareness, duration, and priming. (Note: Following the logic outlined above, a rating of 3 or 4 was dummy-coded as aware, a rating of 1 or 2 as unaware.) With a 10 ms prime duration, there was neither an overall priming effect, nor a moderation of priming by PAS rating. This is not very surprising, given that a 10 ms presentation is at the lower end of the interval typically used in masked priming experiments. Accordingly, with 10 ms primes, there was not even a hint of priming in (the few) trials with PAS ratings of 3 or 4. Thus, priming in both experiments emerged only in the 80 ms condition, which—as expected—was associated with a majority of “aware” trials (that is, trials with PAS ratings of 3 or 4): 59.6 % and 83.4 % of all valid trials in Exp. 4 and Exp. 5, respectively. Indeed, in the 80 ms condition, priming was dependent on subjective awareness: There was a large priming effect for trials associated with PAS ratings ≥ 3, whereas there was a small, numerically positive priming effect for trials associated with PAS ratings ≤ 2. Due to the low number of trials with PAS ratings 1 or 2 (especially in Exp. 5), the status of the masked priming effect under conditions of subjective unawareness was statistically ambiguous (see Supplement for further explanation).

An Additional Critical Detail for Inferences Regarding Underlying Mechanisms

On p. 352, Lähteenmäki et al. (2015) revealed an important detail about their priming experiments. They wrote: “Each picture was presented 16 times in each block (four times as a non-conscious prime, four times as conscious prime, and eight times as probe).” This means that the same stimuli appeared as the target (called “probe” by the authors) in some trials and as the prime in other trials (i.e., what Herring et al., 2013 called “intermixed stimulus presentation”). This is not untypical for (masked) evaluative priming studies and is not a violation of tacit rules. However, whether primes and targets are drawn from the same set or discrete sets is of eminent importance for the interpretation of masked effects.

The argument goes as follows: If a stimulus is used as a target, a stimulus-response episode is created and stored (see, e.g., Abrams & Greenwald, 2000; Damian, 2001; see also Kunde, Kiesel, & Hoffmann, 2003). If, on a different trial, the same stimulus is then presented as a prime, it can serve as a retrieval cue for the episode that includes the response. Response-retrieval can either facilitate responding (in congruent trials) or hamper it (in incongruent trials). Several articles have investigated this phenomenon. For example, with regard to evaluative priming, Abrams and Greenwald (2000) found that even parts of words that had appeared as targets in previous trials were capable of eliciting priming effects. If words like *humor* and *tulip* (two positive words) had been evaluated, even a hybrid non-word prime combining both words (*hulip*) was processed as if it were a positive word. Even a word-hybrid prime that itself is clearly negative (*tumor*) was processed like a *positive* word. Furthermore, evaluative masked priming with often-repeated pictorial primes that also served as targets has been shown to depend exclusively on S-R activation, bypassing semantics (Kiefer et al., 2017). To the extent that intermixed presentation bypasses semantic prime processing, it cannot be used to test whether evaluative or semantic features are extracted from masked stimuli. Thus, one may critically conclude that Lähteenmäki et al. (2015) did not adequately test the question of whether affective processing requires awareness. We and other researchers (e.g., Abrams &

Greenwald, 2000; Kiefer et al., 2015; Klauer et al., 2007; Wentura & Degner, 2010a) strongly recommend that primes and targets are taken from discrete stimulus sets to test such questions.

In conclusion, there are several reasons for not taking the response priming results of Lähteenmäki et al. (2015) as evidence for the claim that “affective processing requires awareness.” In the remainder of this article, we will present two new response priming experiments that employed a trial-by-trial PAS rating. However, we used task parameters that we deemed more adequate to test non-conscious affective processing, based on the outlined theoretical and empirical literature.

Overview of Experiments

We report two response priming experiments using emotional facial expressions as primes and targets; emotion categories were anger and sadness. In recent work (Wentura & Rohr, 2018), we found robust emotion-specific masked priming effects for pairs of negative emotions (i.e., anger vs. sadness, anger vs. fear, sadness vs. fear). Using these categories avoids the salient visual cue of a smiling face and goes beyond the usual positive vs. negative distinction, which is often considered an easier judgment than judgments requiring emotion-specific processing (e.g., Murphy & Zajonc, 1993).

Mask, prime duration, and SOA. In our recent work (Wentura & Rohr, 2018), face-like images with unidentifiable characteristics were used as forward and backward masks (these were created by converting a neutral face into a spatially-quantized face mask; see Bachmann, Luiga, & Pöder, 2005). These masks were not suited for the present research, because subjective impressions (e.g., “I saw eyes”) that are the basis for the PAS rating needed to be clearly attributable to the prime stimulus (and not the mask). Therefore, following other precedent work (Rohr, Degner, & Wentura, 2015; Rohr & Wentura, 2014), we used a black and white fractal image (see Figure 1).

We decided to use three prime durations: 20 ms, 40 ms, and 60 ms. Given our earlier research using this kind of masking,[4] we expected a duration of 40 ms to yield priming, with direct-test performance at chance level. With a prime duration of 20 ms, we were skeptical about finding a priming effect; with a prime duration of 60 ms, we expected priming but also above-chance performance in the direct test. SOA was set to 70 ms; that is, mask duration was 50 ms (30 ms, 10 ms) with a prime duration of 20 ms (40 ms, 60 ms), respectively.

Prime set ≠ target set. In contrast to Lähteenmäki et al. (2015), we used different sets of prime and target stimuli. Prime stimuli were never openly shown to participants and they were never categorized by participants as angry or sad before the direct test. Moreover, to avoid perceptual priming, primes were cropped frontal-view images and target stimuli were profile views.

Assessment of (un)awareness. Our focus was on testing the “subjective awareness” hypothesis put forward by Lähteenmäki et al. (2015) by choosing more adequate experimental parameters. Thus, our starting point was the assumption that introducing the PAS rating into the priming paradigm *might* be a valid procedure that *might* yield clearly interpretable results. However, this is not self-evident. It is conceivable that adding the PAS rating to the procedure alters the processing characteristics of the priming task—a well-known problem in semantic priming research, where it has been shown that a prime-awareness task can interfere with masked priming effects (see, e.g., Dagenbach, Carr, & Wilhelmsen, 1989; Kahan, 2000). Lähteenmäki et al. (2015) conceded that there were differences between those participants

[4] For the sake of transparency, we note that in the methodologically similar experiments in Wentura and Rohr (2018), we used prime durations of 24 ms (Exp. 1) and 20 ms (Exp. 2) and found priming effects. However, as mentioned earlier, in that study we used a different mask. Using the fractal mask used here, we have found objective prime unawareness with a prime duration of 50 ms (albeit for a special type of prime, i.e., frequency-filtered facial expressions; Rohr & Wentura, 2014).

performing the standard task and those performing the priming-plus-rating task. Therefore, subsamples in both the present experiments received the priming task without the PAS ratings; differences between PAS and no-PAS samples can shed light on the validity issue. As usual in the standard technique (see above), all participants were given an objective prime-categorization task at the end of the experiment.

Method

Experiment 1 used the task parameters described above. Experiment 2 was an almost exact replication (see below for details on minor changes). Both experiments yielded essentially the same results regarding the priming effects for subjectively unaware primes. For reasons of succinctness and to use the best statistical base especially for the LMM analyses, we report the results for the combined data sets (with Experiment added as a between-participants factor). The planned experiment-wise analyses are reported in the Supplement.

Participants

Experiment 1. The effect size of the anger versus sadness comparison in Wentura and Rohr (2018) was $d_Z = 0.58$. This effect, however, was based on a total of 160 trials, whereas each of the present priming effects (i.e., for 20, 40, 60 ms prime duration) was based on only 120 trials; we therefore downscaled our expectation a bit. A power analysis (using G*Power; Faul, Erdfelder, Lang, & Buchner, 2007) indicated that to detect an effect of $d_Z = 0.40$ (i.e., effects between small and medium according to Cohen, 1988) with power 1-β = .80 (α = .05) requires a sample of 52 participants. One-hundred five non-psychology undergraduate students from Saarland University ($n_{\text{PAS}} = 53$ for the PAS sample, $n_{\text{no-PAS}} = 52$ for the no-PAS sample; 78 females, 27 males, age *Md* = 24.0 years, range: 19-35 years) participated for a remuneration of €8. All participants had normal or corrected-to-normal vision. The data of two further participants were discarded, one due to excessive error rates (i.e., 38 %, which was an extreme outlier in the distribution of mean error rates) and one due to exceedingly long mean RTs (i.e., mean RT > 1,500 ms, which was an extreme outlier in the distribution of mean RTs).

Experiment 2. Based on Wentura and Rohr (2018) and Experiment 1, we conducted a power analysis assuming an effect size of $d_Z = 0.36$ for the masked emotion-priming effect under conditions of subjective unawareness (see preregistration). To detect such an effect with $1-\beta = .80$ and $\alpha = 0.05$, a sample size of 63 participants is needed. We recruited $N = 126$ non-psychology undergraduate students from Saarland University, who participated for a remuneration of €10 ($n_{\text{PAS}} = 63$; $n_{\text{o-PAS}} = 63$; 78 females, 48 males; age $Md = 21.0$ years, range: 18-33). All participants had normal or corrected-to-normal vision. In accordance with our preregistration, we replaced three participants with an error rate greater than 40 % and six participants giving a PAS rating of 3 or 4 on more than 25 % of catch trials. One additional participant was replaced (again a priori; i.e., without inspection of data) because they showed conspicuous behavior indicative of a potential mental-health issue.

Design

For both the PAS and no-PAS samples, we used a 2 (prime emotion: anger vs. sadness) × 2 (target emotion: anger vs. sadness) × 3 (prime duration: 20 ms vs. 40 ms vs. 60 ms) within-participants design. To reduce complexity, the two factors prime emotion and target emotion were combined into a ‘priming’ factor with levels congruent (i.e., anger/anger; sadness/sadness) vs. incongruent (i.e., anger/sadness; sadness/anger).

Materials

We used the same stimuli as in our previous research (Rohr et al., 2012; Wentura, Rohr, & Degner, 2017; Wentura & Rohr, 2018). Face stimuli were taken from the Karolinska Directed Emotional Faces database (KDEF; Lundqvist, Flykt, & Öhman, 1998). The primes showed frontal views of the faces, whereas targets depicted profile views, to avoid perceptual priming. All images were set to a size of 250 × 250 pixel (approx. 77 × 77 mm). Four instances of each expression (anger, sadness) served as primes. Prime faces were framed by a gray oval that occluded distracting features (e.g., hair), leaving only facial features visible (see Figure 1 for an example). Targets were additional instances taken from different individuals (i.e., five men and five women in left and right profile view for each expression).[5] In the direct awareness task, targets were replaced by neutral-expression profile views of the same individuals, such that prime discrimination responses could not be influenced by target emotion. A fractal image was used to mask the primes.

In Experiment 2, primes and targets were presented in black-and-white instead of colored versions, because pre-testing the revised PAS instructions (see below) revealed that participants made inferences based on perceived colors (e.g., "I must have seen an eye: There was something white, surrounded by skin color.").

Procedure

Participants were seated at individual computers in separate cubicles. The experiment was implemented in PsychoPy2 (PsychoPy Version 1.85; Peirce, 2007) on standard PCs with 17'' CRT monitors with a refresh rate of 100 Hz. All instructions were given on the computer screen. Participants were informed that the experiment was concerned with emotion recognition and that their task was to categorize the presented emotional expressions as accurately and quickly as possible. The response categories (i.e., anger, sadness) were assigned to the 'D' and the 'L' keys of a standard German QWERTZ keyboard. Reminders of the response categories were presented at the bottom of the screen during the blank screen following the target (see

[5] Due to a programming error, additional images of three further individuals (one woman, two men) were included in the „anger" target picture pool, resulting in a slightly lower presentation frequency of images in the anger target category compared to the sadness target category. The general balance of prime-target conditions was, however, maintained throughout the experiment. The error was rectified for Experiment 2.

Figure 1). Assignment of response keys to emotions was counterbalanced across participants.

The beginning of a trial was signaled by a black fixation cross that remained in the middle of the gray screen for 300 ms. It was followed by the sequence of a forward mask presented for 100 ms, the prime presented for either 20, 40, or 60 ms, and the backward mask presented for 50, 30, or 10 ms, respectively (i.e., SOA was constant at 70 ms). The target remained on screen for a maximum of 500 ms, but disappeared as soon as a response was given. We instructed participants to respond within a deadline of 1,500 ms after target onset. In case of late responses, participants received written feedback on the screen that their response was slow. Participants initiated the next trial by simultaneously pressing the D and L keys; the next trial started 1,300 ms thereafter. This was done to ensure that participants' fingers were already on the response keys, to avoid any influence of finger movements on response times (given that participants used different keys for their PAS rating).

Initially, participants worked through two practice blocks with a total of 48 trials; these were identical to the experimental trials except that neutral face expressions were presented instead of the emotional primes, and that participants received feedback in case of an error. The main part of the experiment comprised five blocks of 84 trials each (72 experimental trials and 12 catch trials without a prime. Participants were informed that a "spiral mask" and potentially an additional image would very briefly appear before the target, and that they had the additional task of indicating to what extent they had perceived the additional image, using a scale from 1 to 4. They were instructed to respond with '1' if they had not seen anything; with '2' if they caught a glimpse of the image but could not say what it was (i.e., no specification of content possible); '3' if they were able to form a partial impression of the image and specify some aspects of it; and '4' if they were able to form a clear impression of the image and name its content (see Lähteenmäki et al., 2015, for similar instructions). The scale levels '1' and '2' were assigned to the ‘Q’ and ‘W’ keys on the left side of the keyboard, levels ‘3’ and ‘4’ to the ‘P’ and ‘Ü’ keys on the right (‘Ü’ corresponds to ‘[‘ on a QWERTY keyboard).

In Experiment 1, some participants had high rates of PAS ratings 3 and 4 in catch trials; therefore, PAS instructions were revised for Experiment 2. All participants were informed that the experiment was concerned with emotion recognition and comprised two tasks: a target categorization task and a task that would require them to report the degree of awareness of masked stimuli. To illustrate, they were presented with a graphic depiction of a trial including the masks and prime stimulus. They were then given two practice blocks of 48 trials each (i.e., 96 trials in total) with no PAS rating to practice the main task (i.e., target categorization), before performing a third practice block including the PAS rating. At the beginning of the PAS practice block, they were provided with the PAS rating instructions and had to orally repeat the instructions to the experimenter, to ensure they understood how to use the scale. The PAS practice block had 24 trials: To facilitate anchoring of the scale ratings, it started with three trials with a 60 ms prime that was followed by a 430 ms blank screen (instead of a mask) and a 10 ms mask (under such conditions, the prime should be visible and the rating should thus be ‘4’). The subsequent trials used experimental parameters (i.e., varying prime/mask durations and a 70 ms SOA); first, there were three trials with a 20 ms prime, then three trials with a 40 ms prime, and finally three trials with a 60 ms prime (there was a self-paced break after each set of three trials). The next three trials used ascending prime durations (i.e., 20, 40, 60 ms). The last nine trials used randomly selected durations. Participants used the number keys on the main keyboard (i.e., not the number pad) to give their PAS responses.

During the practice block, participants were instructed to discuss each rating response with the experimenter before giving it, to make sure the scale was used as intended, that is, to minimize inferential influences (Sandberg & Overgaard, 2015). Specifically, while piloting the experiment, we noticed a tendency of participants to infer or complete the visual input with theoretical knowledge about the task (i.e., in the current task context, as soon as one perceives something other than the mask—e.g., a short flickering—one *infers* that it was a face, and given the demand characteristics of the rating, one might feel inclined to give a ‘3’ despite not having *seen* anything; in other words, both pattern completion and demand characteristics are an issue here; we return to this issue in the Discussion).

After the priming experiment, all participants were given an objective measure of awareness; they were instructed to categorize the emotional expression of the prime faces using the same response keys as in the previous task. The direct test comprised 360 trials (i.e., the number of experimental trials in the main experiment; no catch trials). All presentation parameters were the same as in the priming task, with the exception that target stimuli were neutral-expression faces and that participants were under no time pressure to respond.

The no-PAS condition differed only with regard to the PAS rating, which was completely omitted. In Experiment 2, the no-PAS group also received a graphic depiction of a trial in the instruction phase; however, this depicted only the mask, not the prime, as participants were of course not informed of the existence of the primes (in line with standard priming experiments). Furthermore, participants in the no-PAS group received a funneled debriefing procedure after the priming task and before the direct test, as in standard priming experiments. This debriefing informed participants of the presence of primes in the experiment and served as a measure of (global, instead of trial-wise) subjective awareness (e.g., “Did you notice a flickering preceding the clearly visible portraits?”; “Did you notice something within the flickering?”; “If so, what did you notice?”).

We note for the sake of completeness that there were two further minor changes in Experiment 2: First, no reminders of response assignment were presented throughout the experiment; second, the random presentation of targets was restricted to no more than 5 successive repetitions of the same emotion and no direct repetition of the same target identity.

Results

We report all measures, manipulations and exclusions for our studies. All data are openly accessible at https://osf.io/9t4pk/?view_only=caea37bb451346439357e56e1cf31177 . Unless otherwise noted, the criterion of significance was set to $\alpha = .05$ (two-tailed). We used Bonferroni-Holm adjustment for follow-up tests to significant effects involving the duration factor (e.g., the strongest effect out of three will only be termed significant if $p < .05/3$).

Priming effects in PAS and no-PAS samples

Error rates were 8.8% (*SD* = 5.5%) in Experiment 1 and 11.7 % (*SD* = 6.8%) in Experiment 2. Trials with RTs below 200 ms or RTs greater than 1.5 interquartile ranges above the third quartile with respect to the individual distribution of RTs were discarded (Tukey, 1977; see also Rohr et al., 2012); this led to exclusion of 4.1% of trials in Experiment 1 and 4.3% of trials in Experiment 2.

Mean RTs of correct trials and error rates are reported in Table A1 in *Appendix A*. For the sake of simplicity, we directly analyzed priming scores (i.e., $RT_{incongruent} – RT_{congruent}$); see Table 1.[6] A 3 (prime duration: 20 ms vs. 40 ms vs. 60 ms) × 2 (PAS group: PAS present vs. PAS absent) × 2 (Experiment: 1 vs. 2) MANOVA for repeated measures with PAS group and experiment as between-participants factors and priming scores as the dependent variable yielded two significant effects: There was a significant constant effect (i.e., the mean priming difference across all conditions was significantly greater than zero), $F(1,227) = 7.34$, $p = .007$, $\eta_p^2 = .031$, which was significantly moderated by prime duration × PAS group, $F(2,226) =$

[6] In Appendix A, we report analyses on response times directly as a function of duration and PAS group.

5.59, $p = .004$, $\eta_p^2 = .047$ (all other *F*s < 1.37 except $F[2,226] = 2.60$, $p = .077$, $\eta_p^2 = .022$ for the main effect of prime duration and $F[1,227] = 2.13$, $p = .146$, $\eta_p^2 = .009$ for the main effect of experiment). To decompose the prime duration × PAS group interaction, we analyzed the priming scores of the three durations as a function of PAS group. (Note: Analyses including experiment as an additional factor did not show any significant effects involving this factor.)

As expected, with a 20 ms prime duration, there was no priming effect, $F < 1$ for both the constant effect and the moderation by PAS group. According to Bayes factor analyses, there was "strong" evidence for the null hypothesis (with H_1: positive priming) at 20 ms duration, $BF_{0+} = 19.16$ (Jeffreys, 1961; Wagenmakers, Wetzels, Borsboom, & van der Maas, 2011).

For the 40 ms condition, the analysis yielded a significant constant effect (i.e., a positive priming effect), $F(1,229) = 5.18$, $p = .024$, $\eta_p^2 = .022$, which, however, was significantly moderated by PAS group, $F(1,229) = 9.14$, $p = .003$, $\eta_p^2 = .038$. Most important for the subsequent PAS-related analyses, the priming effect in the PAS sample was significant, $t(115) = 3.16$, $p = .002$, $d_Z = 0.29$. There was "strong" evidence for the hypothesis of a positive priming effect at 40 ms duration in this group, $BF_{+0} = 22.02$. Somewhat surprisingly, the corresponding priming effect in the no-PAS sample was absent, $|t| < 1$.

For the 60 ms condition, the analysis yielded a significant constant effect (i.e., a positive priming effect), $F(1,229) = 5.71$, $p = .018$, $\eta_p^2 = .024$, which was not significantly moderated by PAS group, $F(1,229) = 1.32$, $p = .251$, $\eta_p^2 = .006$. According to Bayes factor analyses, there was only "anecdotal" evidence for a positive priming effect, $BF_{+0} = 2.33$. Although the non-significant interaction term did not suggest follow-up analyses, it should be noted that the priming effect for the PAS sample was not significant, $|t| < 1$ ($BF_{0+} = 5.00$, i.e. "substantial" evidence for the null), whereas the corresponding effect for the no-PAS sample was quite robust, $t(114) = 3.64$, $p < .001$, $d_Z = 0.34$ ($BF_{+0} = 94.75$, i.e. "very strong" evidence for H_1).

A final note on the RT analyses: It is evident from Table 1 that in contrast to the PAS samples (which show very homogeneous results across experiments), the no-PAS samples are more heterogeneous. According to the overall analysis, these heterogeneities can be considered random fluctuations; moreover, they only tangentially relate to the aim of the present article. However, for the sake of full transparency and in view of possible meta-analyses (which possibly will be constrained to standard priming procedures—that is, no-PAS samples), we report further analyses of the no-PAS samples in Appendix B.

Additionally, we analyzed priming scores for error rates (i.e., $Err_{incongruent} - Err_{congruent}$; see Table A1) to check whether any of the results were at odds with the RT data. A 3 (prime duration: 20 ms vs. 40 ms vs. 60 ms) × 2 (PAS group: PAS present vs. PAS absent) × 2 (Experiment: 1 vs. 2) MANOVA for repeated measures with PAS group and experiment as between-participants factors and priming scores as the dependent variable yielded no significant effects, all *F*s < 1 (except $F(1,227) = 3.03$, $p = .083$, $\eta_p^2 = .013$ for the constant (which was numerically positive) and $F(2,226) = 2.09$, $p = .125$, $\eta_p^2 = .018$ for duration).

Priming and subjective prime awareness (PAS rating)

PAS ratings. Table 2 shows the distribution of PAS ratings across prime durations (incl. catch trials). As already indicated in the *Procedure* section, in Experiment 1 there was a consistent percentage of level-4 PAS ratings across conditions, including the catch trials. However, closer inspection of the data set showed that *n* = 19 participants used PAS ratings of 3 or 4 in more than 40% of catch trials (*M* = 84%, range 42-100%), whereas the remaining participants (*n* = 34) used PAS ratings > 2 in only 10% or less of all catch trials (*M* = 1%, range 0% – 10%). Therefore, Table 2 shows the distribution of PAS ratings for this latter subsample as well. As can be easily seen, for Experiment 2 (with its improved instructions), the overall distribution of ratings was largely comparable to this subsample. Again, we can see that the number of ratings > 2 in catch trials was negligible; however, the differentiation between levels 1 and 2 did not "work" properly for catch trials, despite thorough instructions and extensive practice with the scale at the beginning of the experiment. We analyzed PAS ratings as a function of duration (incl. catch trials; Helmert contrast-coded) by using LMM analysis (using the lmerTest package, Kuznetsova, Brockhoff, & Christensen, 2016, based on lme4, Bates, Maechler, Bolker, & Walker, 2015, of the R environment for statistical computing; R-Core-Team, 2016). We allowed random intercepts and slopes for participants.

All contrasts were significant, $\beta = -0.113$ ($SE = 0.010$), $t(115.00) = 10.79$, $p < .001$, for catch trials versus non-catch trials (Helmert 1)[7], $\beta = -0.065$ ($SE = 0.009$), $t(115.00) = 7.30$, $p < .001$ for 20 ms vs. 40/60 ms (Helmert 2), and $\beta = -0.086$ ($SE = 0.011$), $t(115.00) = 7.66$, $p < .001$ for 40 ms vs. 60 ms (Helmert 3). Arguably due to the above-mentioned issue with the use of the PAS scale in a sub-sample of Experiment 1, the duration × experiment interaction terms were significant as well, all $t(113.00) > 5.10$, $p < .001$. However, all duration contrasts were significant for Experiment 1, with $\beta = -0.050$ ($SE = 0.012$), $t(52.00) = 4.08$, $p < .001$, for catch trials versus non-catch trials; $\beta = -0.020$ ($SE = 0.008$), $t(52.00) = 2.37$, $p = .021$, for 20 ms vs. 40/60 ms; and $\beta = -0.028$ ($SE = 0.009$), $t(52.00) = 2.96$, $p < .005$, for 40 ms vs. 60 ms. The same was true for Experiment 2: $\beta = -0.166$ ($SE = 0.013$), $t(62.00) = 12.74$, $p < .001$, for catch trials versus non-catch trials; $\beta = -0.102$ ($SE = 0.013$), $t(62.00) = 7.88$, $p = .001$, for 20 ms vs. 40/60 ms; and $\beta = -0.135$ ($SE = 0.017$), $t(62.00) = 8.02$, $p < .001$, for 40 ms vs. 60 ms. Thus, as expected, PAS ratings increased systematically with prime duration.

PAS-dependent priming. The conventional analysis to relate RTs to PAS ratings is to aggregate RTs across the duration × PAS × priming conditions. For the reasons outlined in the

[7] Coding values for the contrasts were: 3 / -1 /-1 / -1 (Helmert 1), 0 / 2 / -1 / -1 (Helmert 2), and 0 / 0 / 1 / -1 (Helmert 3) for catch / 20 ms / 40 ms / 60 ms conditions, respectively.

Introduction, we collapsed PAS levels 1 and 2, as well as 3 and 4, respectively (PAS dichotomized, PASd). Furthermore, to get reasonably robust aggregate variables, we set the minimum number of valid trials to five per cell of the 3 (duration: 20 vs. 40 vs. 60 ms) × 2 (PAS: 1,2 vs. 3,4) × 2 (priming: congruent vs. incongruent) design. Table 1 shows priming effects (i.e., differences between incongruent and congruent conditions) for the 3 (duration: 20 vs. 40 vs. 60 ms) × 2 (PASd: 1,2 vs. 3,4) matrix. As can be seen, sample sizes are slightly reduced for the lower PAS ratings and dramatically reduced for the higher PAS ratings (hence, for a 3 [duration] × 2 [PASd] MANOVA, the effective sample size would be too small for a meaningful analysis). Nevertheless, the following points are obvious: First, the overall pattern of priming effects (40 ms > 60 ms > 20 ms) is mimicked by the pattern for PAS levels 1 and 2. Second and most importantly, the priming effect (M = 16 ms; SE = 6 ms) for the lower PAS ratings in the 40 ms prime-duration condition was significant, $t(103) = 2.77$, $p = .007$, $d_Z = 0.27$ ($|t| < 1$ for the moderation by experiment). This priming effect can be considered “substantial” evidence for the hypothesis of a positive priming effect at 40 ms duration with subjective unawareness, $BF_{+0} = 7.91$.

Finally, the mean priming scores for PAS levels 3 and 4 are numerically at the same level as typical priming scores. However, error variance was substantial (thus, all $|t| < 1.24$). For the sake of completeness, the mean priming effect based on all trials with PAS levels 3 and 4, irrespective of prime duration, was M = 20 ms (SE = 17 ms), $t(86) = 1.17$, $p = .247$, d_Z = 0.12.

Linear mixed-model analyses. To examine the independent contributions of prime duration and PAS rating, we ran LMM analyses. The fixed variables of our model were prime duration (20 ms vs. 40 ms vs. 60 ms; coding see below), PASd (1,2 vs. 3,4), and priming (congruent vs. incongruent), as well as their interactions. Prime duration was contrast-coded with 20 vs. 40/60 ms (D1) and 40 ms vs. 60 ms (D2; see note of Table 3 for the exact coding values). The subsequently reported analyses were first run allowing for random intercepts and random slopes for participants. In case of non-convergence (which was the case in the three omnibus analyses), results for random-intercepts analyses are reported. All analyses were repeated including experiment as a further predictor (incl. all interaction terms); there were no significant moderations of any terms involving the priming predictor by experiment.

First, we ran two analyses that (in addition to the essential priming predictor) included only prime duration or PASd, respectively, to demonstrate correspondence with the conventional analyses (see Table 3). As can be seen in the first analysis, the priming predictor was non-significant (p = .096); however, priming was significantly moderated by duration (20 ms vs. 40/60; p = .039). As already known from the conventional analysis, there was no priming in the 20 ms condition, but substantial priming in the 40 ms condition, hence the interaction. The second analysis, disregarding prime duration, showed a significant priming effect overall (p = .023). Although it was not significantly moderated by PASd (p = .103), the observed pattern fits with the hypothesis of Lähteenmäki et al. (2015): For PAS 3 and 4, the (absolute) weight of the priming predictor increases to (4.0 + 2.9 =) 6.9 ms (which corresponds to a priming effect of (6.9 × 2 =) 14 ms); for PAS 1 and 2, the weight decreases to 1.1 ms (i.e., a priming effect of 2 ms).

The overall analysis including prime duration as well as PASd (and all interaction terms) yielded only one significant effect involving the priming factor, namely a main effect of priming (p = .047). In addition, the three-way interaction of priming × prime duration (D2) × PASd was associated with p = .084. Given the results of the analysis without the PASd factor (see above), we considered this sufficient evidence to analyze duration conditions separately; these follow-up results are provided in the lower panel of Table 3.

As expected, with a 20 ms prime duration, there was no priming effect and no moderation by PAS level. For the 40 ms condition, the overall priming effect was significant and no significant priming × PASd interaction emerged. (Note, as in the conventional analysis, the priming effect was significant in an LMM analysis restricted to the lower PAS levels, $t(1059.0) = 2.45$, $p = .014$.) The analysis of the 60 ms condition yielded a significant priming × PASd interaction. No significant priming effect emerged when restricting the analysis to lower PAS ratings, $t(89.5) = 0.34$, $p = .735$, whereas there was significant priming when analysis was restricted to higher PAS ratings, $t(47.35) = 2.34$, $p = .023$. Thus, with a prime duration of 60 ms, the emerging pattern corresponded with the hypothesis of Lähteenmäki et al. (2015). However, for the 40 ms condition, a pattern was found that suggests affective priming does not require conscious awareness.

In anticipation of the discussion, we briefly address a secondary finding: In all duration conditions, PAS levels 3 and 4 (in comparison to PAS 1 and 2) were associated with slower responses. This slowing effect was (see weights for PASd; 22.1 × 2 =) 44 ms in the 20 ms prime-duration condition, (26.5 × 2 =) 53 ms in the 40 ms condition, and at (47.2 × 2 =) 94 ms even significantly larger (see overall analysis, PASd × D2) in the 60 ms condition.

Given the debate on how to define and measure consciousness adequately (i.e., whether each and every phenomenal experience and thus also experiences associated with a PAS rating of 2 should be considered conscious awareness), we additionally report an LMM analyses using the dichotomy of PAS = 1 versus PAS > 1 (rather than 1,2 versus 3,4) in *Appendix C*. The main result is comparable to the one presented in the main text: There was a significant interaction of priming × prime duration (reflecting the fact that there was no priming at 20 ms, but significant priming at 40/60 ms) but no hint to an interaction by PAS. This holds as well for separate analyses of the different duration conditions.

Priming and objective prime awareness (direct test)

Finally, we report analyses on the direct prime categorization data—and how they relate to the priming data—that are analogous to the analyses using subjective awareness. We calculated *d'* for the three prime durations. To account for ceiling hit or false alarm rates, we followed the log-linear approach (see Hautus, 1995; Stanislaw & Todorov, 1999), which involves adding 0.5 to both the number of hits and the number of false alarms and adding 1 to both the number of signal trials and the number of noise trials, before calculating the relative hit and false alarm rates. Mean *d'* values are shown in Table 4. We report the data separately for no-PAS and PAS samples because priming effects differed somewhat across samples.

PAS samples. A 3 (prime duration: 20 ms vs. 40 ms vs. 60 ms) × 2 (Experiment: 1 vs. 2) MANOVA for repeated measures yielded only a significant prime-duration main effect, $F(2,113) = 16.46$, $p < .001$, $\eta_p^2 = .226$ (all other *F*s < 1.44).

The mean *d'* for the 20 ms prime-duration condition was not significantly greater than zero, $t(115) = -0.71$, $p = .482$. Since there was no priming in the 20 ms condition, we did not further explore the relation of priming and *d'*.

The mean *d'* for the 40 ms condition was positive but also non-significant, $t(115) = 1.64$, $p = .104$, $d_Z = 0.15$. The priming effect in the 40 ms condition did not correlate with the corresponding *d'*, $r = -.08$, $p = .375$. In an additional analysis, we restricted the test for the priming effect in the 40 ms condition to those participants who showed random responding in the direct test. That is, we calculated individual χ^2 statistics for the 2 (prime: anger vs. sad) × 2 (response: anger vs. sad) table of direct test trials; 12 (out of 116) participants had a χ^2 value associated with $p < .05$ (note, $n = 6$ is the expected rate of Type I errors). Excluding these participants still yielded a priming effect of $M = 18$ ms ($SE = 5$ ms), $t(103) = 3.70$, $p < .001$, d_Z

= 0.36, BF_{+0} = 114.69 ("extreme" evidence; Jeffreys, 1961; Wagenmakers et al., 2011). (For the sake of completeness: M = -12 ms (SE = 17 ms), $|t| < 1$, for those with $\chi^2 < .05$.)

As expected, the mean *d'* for the 60 ms prime-duration condition was significantly greater than zero, $t(115) = 5.76$, $p < .001$, $d_Z = 0.53$, although the performance level was still rather low. The correlation of the priming effect in the 60 ms condition with the corresponding *d'* just missed the criterion of significance, *rSpearman-Rho* = .18, $p = .056$ (rank correlation to take adequate account of an extreme *d'* value). With regard to individual contingencies, 38 (out of 116) participants had a χ^2 value associated with $p < .05$. Excluding these participants, there was no priming, M = 0 ms (SE = 8 ms), $|t| < 1$. For those with $\chi^2 < .05$, priming was M = 13 ms, SE = 8 ms, $t(37) = 1.61$, $p = .116$, $d_Z = 0.26$.

No-PAS samples. Direct categorization was roughly the same for the no-PAS samples (see Table 4). (An overall analysis including PAS group as a factor yielded no effects involving PAS group, all *F*s < 1.25.) A 3 (prime duration: 20 ms vs. 40 ms vs. 60 ms) × 2 (Experiment: 1 vs. 2) MANOVA for repeated measures yielded a significant prime-duration main effect, $F(2,112) = 8.12$, $p < .001$, $\eta_p^2 = .127$, which was moderated by experiment, $F(2,112) = 4.10$, $p = .019$, $\eta_p^2 = .068$.

The mean *d'* for the 20 ms prime-duration condition was non-significant, $t(114) = 1.91$, $p = .059$, $d_Z = 0.18$ ($|t| < 1$ for the moderation by experiment). As in the PAS samples, we refrained from further analyses because there was no significant priming at 20 ms.

The mean *d'* for the 40 ms condition was significantly greater than zero, $t(114) = 2.62$, $p = .010$, $d_Z = 0.24$ ($|t| < 1$ for the moderation by experiment). The priming effect in the 40 ms condition did not correlate with the corresponding *d'*, $r = .03$, $p = .719$. Those participants who showed random responding in the direct test (103 out of 115) showed no priming, M = -3 ms (SE = 3 ms), $|t| < 1.02$. (M = 7 ms, SE = 12 ms, $|t| < 1$ for those [n = 12] with $\chi^2 < .05$.)

Finally, the mean *d'* for the 60 ms condition was significantly greater than zero, $t(114) = 4.58$, $p < .001$, $d_Z = 0.43$. This effect was moderated by experiment, $t(64.53) = 2.27$, $p = .027$. However, although *d'* was numerically larger in Experiment 1, in terms of effect sizes it was almost the same outcome, $t(51) = 3.63$, $p < .001$, $d_Z = 0.50$, for Experiment 1, and $t(62) = 3.34$, $p = .001$, $d_Z = 0.42$, for Experiment 2. The correlation of the priming effect in the 60 ms condition with the corresponding *d'* was significant, *rSpearman-Rho* = .20, $p = .035$ (rank correlation to take adequate account of a few extreme *d'* values). With regard to individual contingencies, 30 (out of 115) participants had a χ^2 value associated with $p < .05$. Excluding these participants yielded a significant priming effect of $M = 9$ ms ($SE = 4$ ms), $t(84) = 2.12$, $p = .037$, $d_Z = 0.23$. The subsample of participants with a significant χ^2 value, however, showed a larger priming effect; it was $M = 21$ ms ($SE = 4$ ms), $t(29) = 5.05$, $p < .001$, $d_Z = 0.92$.

Discussion

We started this study as a response to Lähteenmäki et al. (2015) claim that “affective processing requires awareness.” We have outlined why we think that their study might not provide conclusive evidence to address this issue and presented new empirical data that indeed provided evidence for affective processing *without* subjective awareness. Specifically, using different task parameters (i.e., prime durations of 20 ms, 40 ms, and 60 ms instead of 10 and 80 ms; an SOA of 70 ms instead of 150 or 300 ms; and taking primes and targets from distinct sets) as well as linear mixed-model analyses, we were able to show that priming effects can emerge for conditions of perceptual unawareness (PAS ratings 1-2).

Certainly, the empirical results need to be discussed in detail: As expected, there was no priming at the shortest prime duration (i.e., 20 ms). There were only few trials in this condition with PAS ratings > 2, and the PAS rating did not moderate priming. Central to our claim, however, was the finding of a priming effect at the intermediate prime duration (i.e., 40 ms), which was independent of the subjectively reported awareness level. This finding was significant even if analyzing only those trials with subjective (content) non-awareness, that is, trials with PAS ratings 1 or 2. Consequently, Lähteenmäki et al. (2015) conclusion cannot be upheld on the basis of subjective awareness ratings. Most importantly, this priming effect was found with primes that were never shown to participants without being masked, and that were never categorized in the target task. Therefore, the results strongly suggest that the affect-related features were processed without awareness.

This conclusion would even be justified if the sequential standard technique were applied using objective signal-detection awareness measures: Only a small minority of participants showed above-chance responding in the 40 ms prime-duration condition, and the priming effect prevailed if these participants' data were discarded. Moreover, while direct prime categorization performance linearly increased with prime duration, priming effects did not. Thus, there was a dissociation between priming and direct prime categorization, which is usually taken as an indication of the involvement of non-conscious processes (e.g., Vorberg et al., 2003).

However, if we assume for a moment that the 40 ms condition had been omitted from our design, one might have taken the LMM analysis of the 60 ms prime-duration condition as support for Lähteenmäki et al. (2015) claim, as priming in this condition was moderated by PAS rating. For PAS ratings > 2, there was a priming effect (in the LMM analysis) that was completely missing for PAS levels < 2. Interestingly, this result was mirrored by the direct categorization data, in that only participants with above-chance categorization showed a priming effect. Thus, we find our a-priori criticism of Lähteenmäki et al. (2015) choice of prime durations corroborated by our results: the shortest prime duration (i.e., 10 ms in Lähteenmäki et al.; 20 ms in the present study) was too short to find any priming effects; the longest duration (i.e., 80 ms in Lähteenmäki et al.; 60 ms in our study) potentially produced a different type of priming effect due to the marginal visibility of primes (see also below). Inclusion of an adequate intermediate prime duration thus turns out to be critical.

One might wonder whether a simple increase of prime duration by 50 % (i.e., from 40 ms to 60 ms) can plausibly make a qualitative change. However, we should keep in mind the fact that variation of prime duration (with SOA kept constant) inevitably introduces a confound: One can either fix the duration of the mask (thereby accepting a variable blank inter-stimulus interval with unknown consequences) or the duration of the mask varies inversely with prime duration. We opted for the latter, such that the intended effect of the variation of prime duration (or prime “energy”) is buttressed by the variation of mask duration. The purpose of this conjoint variation of prime and mask duration was to create conditions of differential masking efficacy and thus prime visibility (i.e., high masking efficacy / low visibility with short prime duration, medium efficacy /visibility with medium duration and low masking efficacy / high visibility with long prime duration). Thus, the transition from 40 ms to 60 ms is not just a 50% increase in prime duration, but also associated with a decrease in masking efficacy, and these two aspects together might account for a transition from effectively masked primes to primes that are likely visible to some extent.

Our general approach in this research was to tentatively proceed from the assumption that Lähteenmäki et al.’s (2015) dual-task priming procedure might in principle be valid and should be put to a further test by using more appropriate parameters (i.e., prime durations, SOAs, prime set ≠ target set). And indeed, (a) we found substantial priming (i.e., the procedure does not prevent priming effects from occurring) while (b) the PAS rating itself seems to have some validity (i.e., it reflected prime presence vs. absence and it reflected the variation of prime duration in a meaningful way; see Table 2).

Nevertheless, there are two aspects that might indicate that introducing the PAS rating alters the processing characteristics of a response-priming experiment. One aspect is the non-significant priming effect in the 60 ms prime-duration condition (despite a clear priming effect in the no-PAS samples) and its dependence on PAS level (in contrast to the 40 ms priming effect that was independent of PAS level). The second aspect concerns differences in the results between the PAS and no-PAS samples.

The potential effects of introducing the PAS rating

The introduction of the dual-task procedure has obvious effects on general performance levels: Participants were considerably slower in the target task (more than 180 ms in Exp. 1 and more than 320 ms in Exp. 2; see Table A1), compared to the no-PAS sample, if they had to subsequently rate their prime awareness. Moreover, as already noted, this increase in RT was a function of prime duration and PAS rating, with the largest increase in the 60 ms prime-duration condition, which had a high likelihood of PAS ratings > 2. This finding hints at a change in the temporal dynamics of attentional focus in the dual-task situation, which may even be exacerbated in conditions where there is some (initial, partial) perceptual capture of the prime, where the participant's aim may become to form a complete perceptual impression of the prime and encode it into working memory.

The overall non-significant priming effect in the 60 ms prime-duration condition (despite clear priming in the no-PAS samples) and its dependence on PAS level (in contrast to the 40 ms condition) also suggests that the introduction of the PAS rating may have side effects that modulate priming, a notion that is further supported by the PAS rating's correlation with basic response speed in the priming task: Target responses were slowed in particular (by almost 100 ms) in 60 ms prime-duration trials with PAS ratings > 2 (see the LMM analyses). In light of this, one might ask whether translating a subjective impression into a PAS rating produces a specific kind of priming effect as a by-product that is not only an indication of priming per se. Specifically, in trials with a PAS rating > 2, participants' subjective impression of the prime goes beyond "a short, fleeting impression […with] no specification of content". Thus, they might try to store this impression in order to give a valid rating at the end of the trial, which will be a time-consuming process. At the same time, they will try to categorize the target expression, and this categorization process might be delayed if target and prime (i.e. the content being categorized and the content being stored for the subsequent awareness rating) are incongruent; in other words, a priming effect may result because encoding a prime into working memory, or holding it in working memory, will interfere with target processing especially in cases of incongruence. Alternatively, the prime storage process might be hindered if target and prime are incongruent, which could also cause slower responses. Either way, the response priming process may be different when a PAS rating is involved compared to a priming process triggered by an entirely task-irrelevant prime. This assumption is also corroborated by the absence of significant priming effects in the 60 ms prime-duration condition for PAS ratings < 3—that is, when participants did not report awareness of at least some stimulus features. The absence of priming at this longer prime duration is striking, given that the shorter 40 ms prime-duration condition produced significant priming effects for lower PAS ratings. One possible interpretation is that the shift in attentional focus related to the effort of recognizing a marginally-visible prime (i.e., a prime in the 60 ms condition) interfered with affective prime processing only if the prime had a certain strength (i.e., the intentional prime representation outperforms the implicitly activated prime representation and therefore its influence on target processing), thereby abolishing the priming effect. In line with this notion, instructions to recognize marginally-visible masked primes have been shown to alter the pattern of semantic priming effects, presumably through an attentional center-surround mechanism (Carr & Dagenbach, 1990). Future research might put these speculations to the test.

Besides the marked increase in mean RT, the most obvious consequence of the between-participants PAS group factor (PAS present vs. absent) was an ostensible shift in the priming effect with regard to prime duration: It seems as if the standard procedure (i.e., the no-PAS condition) produces a priming effect only with longer (e.g., 60 ms) prime durations, whereas the PAS condition already produces a priming effect with a 40 ms duration. Which result should we trust in?

As outlined above, attentional processes might differ between the two groups, such that—depending on instructions and dual-task-related task-execution factors (e.g., attentional focus, speed-accuracy trade-off, window for evidence accumulation, etc.)—priming effects might indeed emerge at different prime durations. However, in the present case, we have good reasons to believe that the anomaly lies with the no-PAS sample: Although the analyses including PAS group and experiment as factors yielded a clear null effect for the 40 ms condition in the no-PAS sample (and no moderations by experiment), suggesting that the prime did not trigger any processing in this condition, an exploratory analysis constrained to the no-PAS samples (see *Appendix B*) suggested that this conclusion might not be justified. While there was a small positive, albeit non-significant, priming effect in Experiment 1, we found a significant *reversed* effect in Experiment 2. Reversed effects in response priming designs have occasionally been found, and they can be elegantly explained (Klauer, Teige-Mocigemba, & Spruyt, 2009, Klauer & Dittrich, 2010). In a nutshell, if one assumes that responses in binary decision tasks might be given on the basis of the *relative increase* of evidence in favor of a response within an evaluation (time) window, it is critical whether the prime event is routinely included in the evaluation window or not. If it is included, a target-congruent prime will cause a steeper accumulation of (target-congruent) evidence within the window (compared to neutral or incongruent conditions); hence a positive priming effect will result. If the prime event is excluded, a target-congruent prime will have already increased the start value of the evidence accumulator. Thus, the *relative* increase (caused by the target alone) within the window will be lower compared to neutral or incongruent conditions; hence a reversed effect will be found.

If we adopt this theory for the present data, we have to assume that participants in the PAS sample tried to lock the evaluation window to the first mask (or its offset) such that the prime event is included in the evaluation window. This is plausible since the prime event is already task-relevant for participants of the PAS sample. Participants in the no-PAS sample (or a subsample of it), however, may have tried to lock the evaluation window to the offset of the mask—especially in Experiment 2 where we gave more detailed instructions (see below). If we assume, for the sake of the argument, that on average the evaluation window starts a moment (e.g., 30 ms) before mask offset (such that target onset is guaranteed to fall within the window), we can even explain the difference between the reversed effect in the 40 ms condition and the small positive priming effect in the 60 ms condition: A 40 ms prime falls outside of the evaluation window, producing a reversed effect, whereas a 60 ms prime partly falls inside the window, hence producing a small positive effect.

Why did we only observe this reversed effect in Experiment 2? A likely reason is the non-trivial change to instructions: unlike typical priming experiments, instructions in the no-PAS condition of Experiment 2 featured a schematic depiction of the trial sequence that clearly showed the mask-target sequence. This might have given participants a clear indication as when to start the evaluation window (i.e., approximately with mask offset). However, note that the difference between Experiments 1 and 2 might be quantitative rather than qualitative: Although the priming effect in the 40 ms condition of Experiment 1 was positive, it was smaller than the one observed in the corresponding PAS sample (albeit not significantly). Thus, it might be the case that the proportion of participants that produced a reversed effect increased from small (Exp. 1) to large (Exp.2). Be that as it may, the main conclusions of the present article—which are based on the results from the PAS samples—do not rest on these interpretations.

A question that we cannot answer with the present study is whether its results will generalize to other types of affective stimuli, or whether affect-related processes might be involved in the processing of such stimuli. In order to prevent easy discrimination of salient features (i.e., smiles), we used two negative emotions instead of the typically employed comparison of positive and negative stimuli. From an emotion perspective, this aspect makes the present work particularly interesting because it goes beyond the processing of stimulus valence. However, whether our results generalize to other emotion categories or stimulus materials is a question for future research. Likewise, given that we used a response-priming paradigm in the present study, it might be interesting to expand research including the PAS to paradigms that rely on different mechanisms (e.g., semantic priming) or contexts that assess emotion-related processing more directly (e.g., additional assessment of facial electromyography, see Rohr, Folyi, & Wentura, 2018).

Conclusions

With this article, we wanted to connect the research of Lähteenmäki et al. (2015) with the long-standing research tradition on masked response priming. Our arguments, experiments, and analyses yielded outcomes that are clearly at odds with Lähteenmäki et al.'s claims. Indeed, our experiments provided evidence for affective processing *without* awareness.

To summarize, we found no priming for the shortest prime duration (i.e., 20 ms), significant priming with a prime duration of 40 ms, which was not moderated by PAS ratings (and still significant if analyses were restricted to PAS ratings < 3), and significant priming with a prime duration of 60 ms, albeit only under conditions of subjective awareness. These results do not only refute the claims by Lähteenmäki et al., they also provide additional evidence regarding the implications of introducing the PAS and the importance of even minor changes in prime duration: With a mask like ours, a prime duration of 20 ms (together with a 50 ms mask) was too short to yield priming effects, 40 ms (30 ms mask) seemed ideal to yield non-conscious priming effects, and 60 ms (10 ms mask) yielded priming effects that were influenced by prime visibility and task-strategic influences of the PAS. Our using different sets of primes and targets (in contrast to Lähteenmäki et al.) is important in this regard, as it rules out stimulus-response associations as an explanation for the priming effects. Furthermore, the results were obtained with stimuli from two negative emotion categories, thereby going beyond the often employed valence differentiation. Therefore, Lähteenmäki et al.'s conclusion that affective processing requires awareness is not justified. In fact, our data suggest the opposite.

Context of the present research

The present research was a collaboration of two groups that are both deeply involved in masked priming research. The present experiments were specifically conducted as part of a research program that focuses on the question of how sophisticated the fast and automatic processing of affective stimuli is. It is a typical assumption in this research area that fast and automatic processing of affective stimuli is rather coarse-grained, resulting only in a positive-negative categorization. The results obtained in the research program so far question this assumption. We have found evidence implying that masked affective stimuli are often processed up to the level of the (basic) emotion, or at least up to the level of emotion sub-groups (e.g., anger expressions that signal something negative for the observer in contrast to fear/sadness expressions that signal a negative state of the expresser themselves). These results were obtained using masked presentation and the standard procedure involving indirect and direct tests. With the present work, we aimed to extend this line of research to measures of subjective awareness. In the future, we will continue to examine the early automatic processing of affective stimuli (e.g., with different stimulus materials such as emotive images), and extend research using the PAS to different materials and paradigms in order to gain further insights into fast and automatic affective and semantic processing.

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Tables

Table 1

Priming effects (PE; standard errors in brackets) as a function of prime duration and PAS rating

	PAS samples											
				PAS								
	Overall			1, 2 ("unaware")			3, 4 ("aware")			no-PAS samples		
Prime Duration	PE	SE_{PE}	N	PE	SE_{PE}	N	PE	SE_{PE}	N	PE	SE_{PE}	N
Experiment 1												
20 ms	-2	[7]	53	-5	[10]	42	20	[10]	21	-2	[4]	52
40 ms	14	[7]	53	18	[9]	41	13	[9]	24	7	[4]	52
60 ms	6	[8]	53	-3	[10]	40	27	[9]	28	19	[5]	52
Experiment 2												
20 ms	-5	[7]	63	-3	[7]	63	38	[64]	13	3	[4]	63
40 ms	16	[7]	63	15	[8]	63	8	[16]	30	-10	[4]	63
60 ms	2	[9]	63	-5	[12]	61	12	[24]	40	7	[4]	63
Overall												
20 ms	-4	[5]	116	-4	[6]	105	27	[25]	34	1	[3]	115
40 ms	15	[5]	116	16	[6]	104	10	[10]	54	-2	[3]	115
60 ms	4	[6]	116	-4	[8]	101	18	[14]	68	12	[3]	115

Note. 'N' denotes the effective sample size for a given priming effect; reductions in sample sizes in PAS 1,2 and 3,4 columns are caused by removal of participants due to low number of observations (< 5) in the respective cell.

Table 2

Distribution of PAS ratings (proportions) and mean ratings with standard deviations across prime-duration conditions (incl. catch trials)

	PAS Rating					
Prime Duration	1	2	3	4	M[a]	SD[a]
Experiment 1 (N=53)						
Catch trials	0.46	0.23	0.12	0.18	2.04	1.15
20 ms	0.29	0.40	0.12	0.18	2.20	1.06
40 ms	0.28	0.40	0.14	0.18	2.23	1.05
60 ms	0.26	0.37	0.19	0.18	2.29	1.04
Experiment 1 (N=34)[b]						
Catch trials	0.69	0.30	0.01	0.00	1.33	.50
20 ms	0.44	0.54	0.02	0.00	1.68	.56
40 ms	0.41	0.53	0.05	0.00	1.80	.65
60 ms	0.39	0.50	0.12	0.00	1.99	.81
Experiment 2 (N=63)						
Catch trials	0.69	0.29	0.02	0.00	1.33	.52
20 ms	0.27	0.67	0.06	0.00	1.79	.55
40 ms	0.21	0.64	0.13	0.02	1.97	.65
60 ms	0.16	0.52	0.23	0.08	2.24	.82

[a]Means and SDs were calculated across the full sample of trials (i.e., N = 53/34/63 × 420).
[b]Excludes participants with more than 40% PAS ratings > 2 in catch trials (see text).

Table 3

Results of the linear mixed-model analyses

Fixed Factor	Weight	SE	df	*t*	*p*
Only prime duration					
Intercept	928.5	26.7	115.0	34.78	<.001
Priming (P)	-2.6	1.5	35760.1	-1.66	.096
Prime Dur. (D1)	-5.1	2.2	35760.2	-2.37	.018
Prime Dur. (D2)	-12.7	1.9	35760.2	-6.76	<.001
P × D1	4.5	2.2	35760.1	2.06	.039
P × D2	-2.6	1.9	35760.1	-1.40	.162
Only PASd					
Intercept	948.7	27.1	115.5	35.0	<.001
Priming (P)	-4.0	1.8	35762.2	-2.28	.023
PASd	40.2	2.5	35876.9	15.98	<.001
P × PASd	-2.9	1.8	35762.4	-1.63	.103
Full model					
Intercept	946.0	27.0	115.6	35.02	<.001
Priming (P)	-3.6	1.8	35754.2	-1.99	.047
PASd	36.5	2.7	35866.2	13.75	<.001
Prime Dur. (D1)	-2.0	2.8	35772.0	-0.73	.464
Prime Dur. (D2)	-11.5	2.1	35762.7	-5.44	<.001
PASd × D1	-5.1	2.8	35771.6	-1.85	.064
PASd × D2	-7.3	2.1	35761.9	-3.42	<.001
P × PASd	-2.5	1.8	35754.4	-1.40	.163
P × D1	3.7	2.7	35754.1	1.37	.170
P × D2	-1.3	2.1	35754.2	-0.61	.543
P × PASd × D1	-0.2	2.7	35754.3	-0.06	.956
P × PASd × D2	3.6	2.1	35754.4	1.73	.084

(Table continued on next page)

(Continuation of Table 3)

For 20 ms Prime Duration					
Intercept	944.6	29.0	105.8	32.62	<.001
Priming (P)	-0.3	3.5	1465.6	-0.10	.922
PASd	22.1	9.8	38.82	2.26	.030
P × PASd	-3.1	3.5	2387.2	-0.90	.370
For 40 ms Prime Duration					
Intercept	940.3	27.7	113.1	33.95	<.001
Priming (P)	-7.8	3.2	318.5	-2.46	.014
PASd	26.5	7.6	40.9	3.51	.001
P × PASd	-0.9	3.1	945.1	-0.30	.762
For 60 ms Prime Duration					
Intercept	967.5	29.7	116.9	32.58	<.001
Priming (P)	-5.5	3.3	124.4	-1.66	.099
PASd	47.2	8.8	87.6	5.34	<.001
P × PASd	-8.0	3.4	80.8	-2.35	.021

Note. Coding of predictor variables was as follows: *Priming* (P): -1 = incongruent, +1 = congruent; *PASd*: -1 = [1,2], +1 = [3,4]; *Prime Duration* (D1): 1 = 20 ms, -.5 = 40 ms, -.5 = 60 ms; *Prime Duration* (D2): 0 = 20 ms, 1 = 40 ms, -1 = 60 ms. The overall analyses (depicted in the top panels of the table) included only random intercepts (but not random slopes) for participants, because the analyses including random slopes did not converge (see text).

Table 4

Mean *d'* (standard errors in parentheses) as a function of group and prime duration

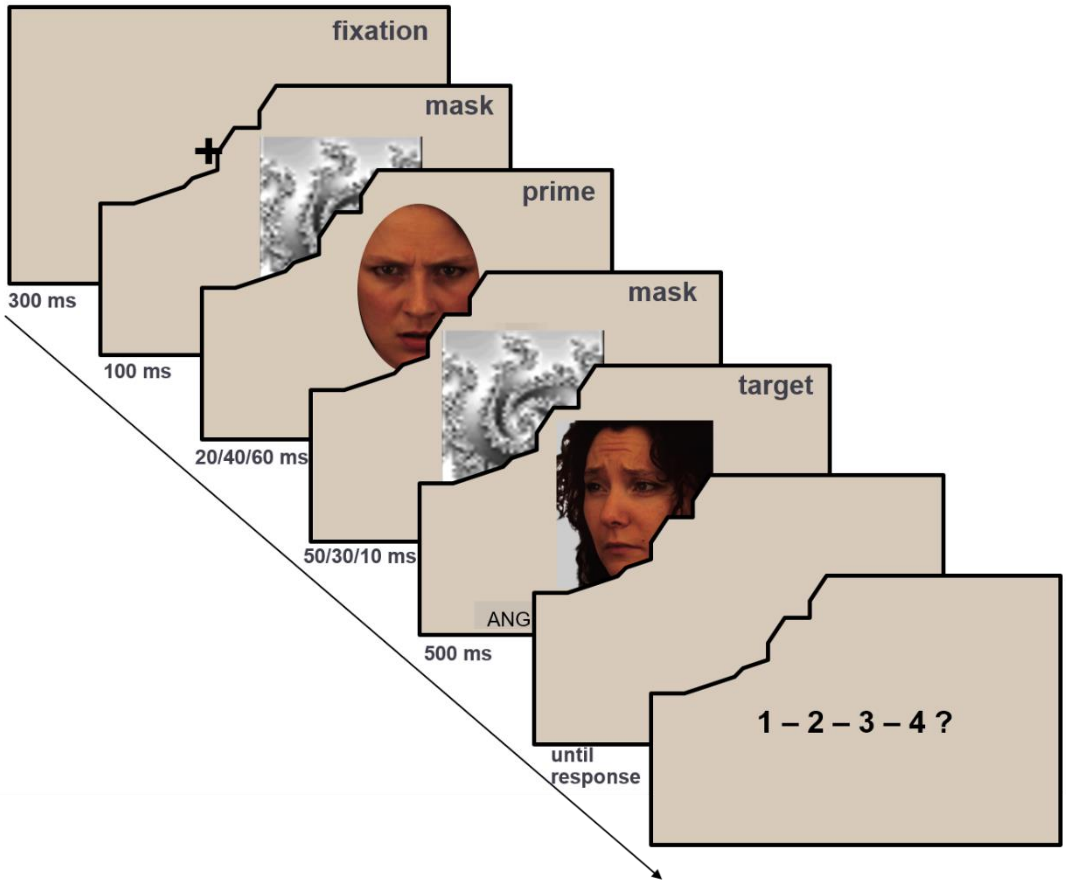
	Prime Duration					
	20 ms		40 ms		60 ms	
PAS samples						
Experiment 1	-0.06	(0.06)	0.06	(0.04)	0.38	(0.10)
Experiment 2	0.01	(0.03)	0.02	(0.03)	0.23	(0.05)
Overall	-0.02	(0.03)	0.04	(0.02)	0.30	(0.05)
no-PAS samples						
Experiment 1	0.05	(0.04)	0.08	(0.06)	0.38	(0.11)
Experiment 2	0.05	(0.04)	0.09	(0.03)	0.13	(0.04)
Overall	0.05	(0.03)	0.08	(0.03)	0.24	(0.05)

Figure captions

Figure 1

Schematic depiction of a priming trial in Experiment 1. (In Experiment 2, facial stimuli were in black-and-white.)

Figure 1


fixation
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Appendix A

Basic response speed as a function of duration, PAS group, and experiment

Table A1 shows the mean response times in milliseconds and error rates as a function of priming condition, prime duration, and PAS group. Analyses concerning the priming effects are reported in the main text; results related to the moderation of basic response speed are reported here for reasons of succinctness. A 3 (prime duration: 20 ms vs. 40 ms vs. 60 ms) × 2 (PAS group: PAS present vs. PAS absent) × 2 (Experiment: 1 vs. 2) MANOVA for repeated measures with PAS group and experiments as between-participants factors, mean RT as the dependent variable, and priming conditions collapsed yielded the following results.

There were several significant between-participants effects with regard to basic speed. Experiment 2 yielded slower responses than Experiment 1, $F(1,227) = 22.96$, $p < .001$, $\eta_p^2 = .092$. PAS samples had slower responses than no-PAS samples, $F(1,227) = 87.07$, $p < .001$, $\eta_p^2 = .277$; this effect was more pronounced in Experiment 2, $F(1,227) = 6.67$, $p = .010$, $\eta_p^2 = .029$. These results are obviously due to the dual-task demands in the PAS samples and the improved instructions in Experiment 2.

Moreover, response times were moderated by the within-participants factor prime duration, $F(2,226) = 10.13$, $p < .001$, $\eta_p^2 = .082$. This effect was significantly moderated by the between-participants factors, with $F(2,226) = 11.10$, $p < .001$, $\eta_p^2 = .089$, for the duration × PAS group interaction; $F(2,226) = 3.53$, $p = .031$, $\eta_p^2 = .030$, for the duration × experiment interaction; and $F(2,226) = 3.70$, $p = .026$, $\eta_p^2 = .032$, for the duration × PAS group × experiment interaction. To break down the three-way interaction, we conducted 3 (prime duration: 20 ms vs. 40 ms vs. 60 ms) × 2 (Experiment: 1 vs. 2) MANOVAs for repeated measures, separately for the PAS and no-PAS samples.

PAS. The main effect of duration was significant, $F(2,113) = 11.92$, $p < .001$, $\eta_p^2 = .174$; it was moderated by experiment, $F(2,113) = 3.73$, $p = .027$, $\eta_p^2 = .062$. Both effects were predominantly due to the second Helmert contrast (i.e., 40 ms vs. 60 ms), with $F(1,114) = 23.97$, $p < .001$, $\eta_p^2 = .174$, for the main effect, and $F(1,114) = 7.10$, $p = .009$, $\eta_p^2 = .059$, for the moderation by experiment (the first Helmert contrast yielded $F(1,114) = 2.32$, $p = .131$, $\eta_p^2 = .020$, for the main effect and $F < 1$ for the moderation by experiment). Responses in the 60-ms condition were $M = 11$ ms ($SE = 6$ ms; Experiment 1) and $M = 37$ ms ($SE = 8$ ms; Experiment 2) slower compared to responses in the 40-ms condition.

No-PAS. The main effect of duration was non-significant, $F(2,112) = 2.94$, $p = .057$, $\eta_p^2 = .050$. It was, however, moderated by experiment. For Experiment 1, there was no effect of duration, $F < 1$. For Experiment 2, the effect of duration was significant, $F(2,61) = 6.02$, $p = .004$, $\eta_p^2 = .165$; this was entirely due to the first Helmert contrast (i.e., 20 ms vs. 40/60 ms), $F(1,62) = 12.15$, $p < .001$, $\eta_p^2 = .164$ ($F < 1$ for the contrast 40 ms vs. 60 ms). Responses were a bit slower in the 20 ms condition.

Table A1

Mean response times in milliseconds and error rates (in parentheses; in %) as a function of priming condition, prime duration, and PAS group

	Priming			
	Congruent		Incongruent	
Prime Duration	RT	ER	RT	ER
Experiment 1				
PAS				
20 ms	815	(0.08)	813	(0.09)
40 ms	808	(0.09)	823	(0.09)
60 ms	823	(0.09)	830	(0.08)
No PAS				
20 ms	634	(0.08)	632	(0.09)
40 ms	629	(0.09)	636	(0.09)
60 ms	624	(0.09)	643	(0.09)
Experiment 2				
PAS				
20 ms	1019	(0.11)	1014	(0.12)
40 ms	997	(0.10)	1013	(0.11)
60 ms	1041	(0.11)	1044	(0.11)
No PAS				
20 ms	699	(0.12)	702	(0.13)
40 ms	696	(0.11)	686	(0.12)
60 ms	686	(0.13)	693	(0.12)

Appendix B

An analysis of the no-PAS samples

As noted in the *Results* section, analyses of priming scores yielded no significant moderations by experiment, neither for the overall 3 (prime duration) × 2 (PAS group) × 2 (Experiment) MANOVA for repeated measures nor for the duration-specific analyses. Thus, the heterogeneities in priming effects across experiments in the no-PAS samples (see Table 1) might be considered random fluctuations. For the sake of full transparency and in view of possible meta-analyses (which will likely be constrained to standard priming procedures—i.e., no-PAS samples), here we report analyses constrained to the no-PAS samples.

For Experiment 1, a MANOVA for repeated measures on priming scores with the sole factor of prime duration yielded a main effect of duration, $F(2,50) = 3.86$, $p = .028$, $\eta_p^2 = .134$. The first Helmert contrast (i.e., 20 ms vs. 40/60 ms) was significant, $F(1,51) = 7.43$, $p = .009$, $\eta_p^2 = .127$, whereas the second Helmert contrast (i.e., 40 vs. 60 ms) was not, $F(1,51) = 2.42$, $p = .126$, $\eta_p^2 = .045$. Priming for 20 ms was not significant, $F < 1$, whereas the effect for 40/60 ms combined was significant, $F(1,51) = 21.54$, $p < .001$, $\eta_p^2 = .297$.

For Experiment 2, the corresponding analysis yielded a main effect of duration, $F(2,61) = 5.09$, $p = .009$, $\eta_p^2 = .143$. The first Helmert contrast (i.e., 20 ms vs. 40/60 ms) was not significant (due to the null effect for 20 ms, $F < 1$, and the opposing effects for 40 ms and 60 ms), $F < 1$, whereas the second Helmert contrast (i.e., 40 vs. 60 ms) was significant, $F(1,62) = 9.58$, $p = .003$, $\eta_p^2 = .134$. In the 40 ms condition, there was a significant negative effect, $F(1,62) = 5.13$, $p = .027$, $\eta_p^2 = .076$, whereas there was a non-significant positive effect in the 60 ms condition, $F(1,62) = 2.38$, $p = .128$, $\eta_p^2 = .037$.

Appendix C

An LMM analysis contrasting PAS = 1 with PAS > 1

Table A2 shows the results of an LMM analysis analogous to the analysis in the main text, but with a dichotomized PAS rating that contrasts PAS = 1 with the remaining levels (PAS ratings 2-4). Since there was no indication of three-way interactions (Priming × PASd × Prime Duration [D1] or Priming × PASd × Prime Duration [D2]), these higher-order terms were removed. There was a significant Priming × Prime Duration (D1) interaction, indicating that priming was different for 20 ms compared to 40/60 ms conditions. There was, however, no hint to a Priming × PAS interaction. This holds as well for separate analyses of the different duration conditions. In correspondence to our main analyses, for the 40 ms condition the overall priming effect was significant.

In Table A3, we present priming effects for each PAS level, once unrestricted with regard to the number of valid trials (per participant) that enter into the aggregates and once with the minimal requirement of 10 valid trials for the congruent and incongruent conditions, respectively. As can be seen, except for PAS level 2, the unrestricted aggregates are very noisy. With the "minimum 10" requirement, however, all priming effects were positive, three of them being significantly above zero, $t(39) = 2.29$, p = .028, $d_Z = .36$ for PAS=1, $t(89) = 2.73$, p = .008, $d_Z = .29$ for PAS=2, $t(24) = 2.18$, p = .039, $d_Z = .44$ for PAS=3, $t(11) = 1.46$, p = .172, $d_Z = .42$ for PAS=4. We must admit, however, that the valid sample size is sharply reduced in these cases.

Table A2

Results of the linear mixed-model analyses with PAS = 1 versus PAS > 1 for Experiments 1 and 2 combined

Fixed Factor	Weight	SE	df	*t*	*p*
Intercept	918.3	26.8	115.5	34.26	<.001
Priming (P)	-2.6	1.8	35758.2	-1.42	0.155
PASd	19.8	2.4	35870.0	8.15	<.001
Prime Duration (D1)	-3.6	2.2	35759.5	-1.65	0.099
Prime Duration (D2)	-12.2	1.9	35758.5	-6.48	<.001
P × PASd	-0.1	1.8	35758.4	-0.04	0.965
P × D1	4.4	2.2	35758.1	2.04	0.041
P × D2	-2.6	1.9	35758.1	-1.39	0.165
For 20 ms Prime Duration					
Intercept	919.7	26.6	116.0	34.56	<.001
Priming (P)	0.8	3.0	11895.7	0.26	0.795
PASd	9.3	4.0	12007.6	2.34	0.020
P × PASd	2.0	3.0	11896.5	0.69	0.490
For 40 ms Prime Duration					
Intercept	910.0	25.9	116.7	35.11	<.001
Priming (P)	-6.4	3.1	11869.7	-2.06	0.040
PASd	16.2	4.2	11982.8	3.87	<.001
P × PASd	-2.2	3.1	11871.6	-0.70	0.487
For 60 ms Prime Duration					
Intercept	924.3	28.4	117.2	32.53	<.001
Priming (P)	-2.7	3.3	11760.8	-0.82	0.414
PASd	32.9	4.7	11873.9	7.07	<.001
P × PASd	0.4	3.4	11761.3	0.13	0.895

Note. Coding of predictor variables was as follows: *Priming* (P): -1 = incongruent, +1 = congruent; *PASd*: -1 = [1], +1 = [2,3,4]; *Prime Duration* (D1): 1 = 20 ms, -.5 = 40 ms, -.5 = 60 ms; *Prime Duration* (D2): 0 = 20 ms, 1 = 40 ms, -1 = 60 ms. All analyses were random intercepts analyses.

Table A3

Priming effects (PE; standard errors in brackets) for 40 ms prime duration as a function of PAS rating (Experiment 1 and 2 combined)

	PAS Rating											
	1			2			3			4		
	PE	SE_{PE}	N	PE	SE_{PE}	N	PE	SE_{PE}	N	PE	SE_{PE}	N
All trials	14	[20]	93	25	[9]	102	-17	[22]	68	-89	[37]	30
Min. 10 trials	28	[12]	40	23	[9]	90	20	[9]	25	19	[13]	12

Note. In the row “All trials”, aggregates for congruent and incongruent priming were calculated if at least a single trial had a valid RT (for a given participant); in the row “Min. 10 trials”, aggregates for congruent and incongruent priming were calculated if a minimum of 10 trials had a valid RT (for a given participant). In the columns termed ‘N’ the effective sample for a given priming effect is noted.