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Does Affective Processing Require Awareness? On the Use of the Perceptual Awareness Scale in Response Priming Research

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Masked priming paradigms are frequently used to shine light on the processes of nonconscious cognition. Introducing a new method to this field, Lähteenmäki et al. (2015) claimed that affective priming requires awareness. Specifically, they administered a subjective rating task after the priming task in each trial to directly assess awareness of the prime. Their main result was a lack of priming for subjectively unaware primes. In four experiments, we compared their method with the traditional paradigm, that is, a single-task priming phase followed by a direct test of prime recognition. We used faces with anger versus sadness expressions as primes and targets; emotion categorization was the task. In contrast to Lähteenmäki et al., primes and targets were drawn from different sets, such that priming effects can be unequivocally attributed to the processing of evaluative features. In Experiments 1a, b, we followed their approach of using different prime durations to produce variance in awareness ratings. With a duration of 40 ms, significant priming effects for subjectively unaware primes were found. This duration was also associated with priming effects in the traditional paradigm with near-zero objective prime categorization, suggesting that priming does not require awareness. In Experiment 2a, employing a constant 40-ms duration, we replicated the traditional effect. However, the parallel Experiment 2b with subjective awareness ratings produced a null result at a sharply increased response time level. We conclude that the claim that affective processing requires awareness is not justified. Subjective trial-by-trial visibility ratings can severely alter processing strategies in response priming paradigms.

Public Significance Statement
There has been a long-standing debate about whether certain features of a stimulus (e.g., the affective meaning) are processed if the stimulus is presented very briefly and immediately followed by a meaningless subsequent stimulus (i.e., a "mask"), so that the stimulus does not enter awareness. The present article is situated within this debate as a reply to an article which concluded that "affective processing requires consciousness." Re-analyzing the original data and responding with new experiments to their concerns (using more appropriate technical parameters), the present article provides evidence that affective processing without awareness is possible. It also extends scientific knowledge by specifying the specific assessment procedure(s) of consciousness that may influence the assessment of nonconscious processing.

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

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Masked priming paradigms are frequently used to elucidate the influences of nonconscious cognition and its underlying mechanisms (Van den Bussche et al., 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 nonconscious 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 et al., 2007; Spruyt et al., 2012; Wentura & Degner, 2010a), or visuomotor processing (Vorberg et al., 2003; Zovko & Kiefer, 2013). These findings suggest that the concept of nonconscious 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 et al., 1995; Klauer et al., 1998; Merikle et al., 2001; Schmidt & Vorberg, 2006; Wiens, 2006).

Lähteenmäki et al. (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, in which masked-prime awareness is psychophysically assessed in a separate session after the priming experiment, and suggested an alternative, namely a trial-by-trial awareness rating procedure (Ramsøy & Overgaard, 2004). On a theoretical level, they emphasized the difference between the processing of nonemotional 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" (p. 339).

Lähteenmäki et al.'s (2015) article is inspiring and provides 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 as far as it relates 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 stimulus onset asynchronies, 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-effects model approach that allows for the concurrent examination of the separate contributions of prime duration and prime awareness. We then present new empirical data further challenging Lähteenmäki et al.'s claim that affective processing requires 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 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 their brief and masked presentation and implicit processing (see e.g., Kiefer, 2002; Kiefer & Martens, 2010; Rohr et al., 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. 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 an explicit 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 nonawareness) of the task-relevant prime feature.

Evidence from the two tasks can be combined in various ways. Typically, nonconscious 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 interindividual 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 awareness rating scale employed by Lähteenmäki et al. (2015) and similar variants (Dienes & Seth, 2010; Ramsøy & Overgaard, 2004; Sandberg et al., 2010; Szczepanowski et al., 2013; Szczepanowski & Pessoa, 2007; Timmermans et al., 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 would be inadequate. The authors argued that this technique does not control for trial-wise awareness; as such, supposed evidence for nonconscious processing might be the consequence of averaging across conscious trials (that show the priming effect) and nonconscious trials (that do not show the effect). Furthermore, they argued that fixed prime and mask durations do not take into account interindividual variability in awareness (if awareness is only calculated over the entire sample). Thus, the authors proposed the use of 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*

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

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 1–3 explored the relationship between masked-prime awareness 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). Any priming effects that were to emerge were put into relation with the awareness rating. Particularly, the second empirical part motivated our research. From a masked-processing perspective (see below), this part is especially relevant as it links an objective indirect measure of stimulus processing (i.e., the priming effect) to a direct subjective awareness rating. It is typically assumed that direct access (e.g., in explicit forced-choice recognition) necessitates conscious awareness, whereas nonconscious processing can take place indirectly (e.g., Merikle & Reingold, 1991).

Masked-Prime Processing and Trial-Wise Awareness

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 not (incongruent trials). Participants have to categorize the target according to its valence. Typically, a congruency effect is found, that is, responses are faster and/or more accurate for congruent compared to incongruent trials, thereby indicating processing of the prime. Since its introduction by Fazio et al. (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 processed (i.e., elicit a priming effect; e.g., Draine & Greenwald, 1998; Kiefer et al., 2015, 2017; 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 vs. 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, nonresponse-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; geometric shapes, Martens et al., 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 et al., 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 attracted considerable interest (e.g., Draine & Greenwald, 1998; Hermans et al., 2003; Kiefer et al., 2015; Klauer et al., 2007; Klinger et al., 2000; Wentura & Degner, 2010a).[2]

As mentioned earlier, (non)awareness of masked primes in this paradigm is typically tested by comparing results of the indirect priming task with the results of a direct explicit test that involves an almost exact repetition of the priming task sequence, now with the instruction to categorize the prime. If a significant masked-priming effect is obtained, there are several ways to infer nonconscious prime processing based on the direct test (Draine & Greenwald, 1998; Schmidt & Vorberg, 2006; see above). The most straightforward approach is to establish chance-level discrimination in the direct test (see e.g., Ortells et al., 2013; Schmidt, 2007; Schmidt & Vorberg, 2006, for discussions of this approach). Given the criticisms directed at this approach (see also below), Lähteenmäki et al. (2015) proposed an alternative procedure and asked (some of) their participants to rate prime awareness with the PAS 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 from trials associated with some level of subjective prime awareness presents a challenge for researchers who assume that nonconscious processing exists, based on other methods to assess prime awareness. We will expand on the different awareness methods further below.

In that regard, it is already important to note that if the null findings of Lähteenmäki et al. (2015) had been restricted to PAS Level 1 (i.e., if Level 2 had produced a priming effect), proponents of nonconscious priming would presumably have simply 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).

In this vein, some researchers have critically evaluated the use of the PAS (Dienes & Seth, 2010; Schmidt, 2015; Zehetleitner & Rausch, 2013), with ongoing debates on the adequate assessment of phenomenal consciousness (e.g., Rausch & Zehetleitner, 2016). Specifically, 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 experiences and relevant conscious visual content (Dienes & Seth, 2010).

[2] The diversity of semantic categorization is also reflected in Lähteenmäki et al.'s (2015) comparison of evaluative categorization with nonevaluative semantic categorization ("animal or object?"); we will return to this issue in the General Discussion section.

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 nonsensory conscious content (e.g., a feeling of familiarity), and participants seem to also include task-irrelevant experiences in their subjective reports (Rausch et al., 2015). Despite these limitations associated with the interpretation of PAS ratings, a recent study showed that awareness thresholds determined from PAS ratings were highly comparable to thresholds derived from objective detection and discrimination performance, at least in observers trained to appropriately use the PAS (Kiefer, Frühauf, & Kammer, 2023). Most importantly, objective detection threshold was related to a PAS score of 1.5, that is, in-between PAS Level 1 (not seen) and PAS Level 2 (glimpse), while objective discrimination threshold was related to a PAS score of 2.5, that is, in-between PAS Level 2 (glimpse) and PAS Level 3 (partial awareness). This indicates that awareness of feature content requires PAS Levels 3 or 4.

Therefore, a PAS rating of 2 ("I saw a glimpse of something, but don't know what it was") should not be considered evidence of phenomenal awareness of the content of the masked prime, because an impression devoid of content may involve low-level sensory-experiences such as motion or flicker (for a discussion with regard to the phenomenal content of masked stimuli, see also Koster et al., 2020). 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 (see also Schmidt & Biafora, 2024).

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") to 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. In line with this assumption, Lähteenmäki et al. (2015) reported significant priming effects only for PAS ratings 3 and 4 (pp. 352f, 355).

We are very explicit about this detail at this early point of the article because parts of Lähteenmäki et al.'s (2015) article 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, even though the authors did not stringently use this demarcation line (see above). Certainly, the question of whether or not a nonidentifiable glimpse should be considered a conscious experience of that stimulus is a general one. As laid out above, we hold the same opinion as other 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.'s (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 article, focusing primarily on Lähteenmäki et al.'s (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, as described above, 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.'s call into question the validity of the Level 1 versus Level 2 distinction. Furthermore, we will argue that the priming experiments by Lähteenmäki et al. (2015) are characterized by some questionable parameter settings, and that a crucial analysis is missing from their investigation. Based on new experimental data, we will show that masked-priming effects can be obtained at levels of subjective unawareness (i.e., PAS Levels 1/2).

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

Before proceeding to the re-analysis, it is worth mentioning that there are a number of further 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." However, typical SOAs in evaluative response priming with backward masking as employed by Lähteenmäki et al. are below 100 ms. For example, Greenwald et al. (1996) found a rapid decline in masked-priming effects for SOAs exceeding 100 ms (see also Kiefer & Brendel, 2006; Kiefer & Spitzer, 2000). Only in an *unmasked* evaluative priming paradigm, a SOA of 150 ms would have been a reasonable choice; however, a SOA of 300 ms—although sometimes used in the literature—would have arguably been a poor choice even for an unmasked paradigm (see Hermans et al., 2001). Thus, for a stringent test of whether masked affective priming requires awareness, a shorter SOA should be used.

[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.

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 the expectations given the literature on visual masking (Bachmann & Francis, 2014; Breitmeyer & Ögmen, 2006). Specifically, the chosen prime durations of 10 ms or 80 ms are rarely used in masked-priming studies to investigate nonconscious 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 nonconscious processes at all because of very limited stimulus energy (see e.g., Vorberg et al., 2003). Thus, studies aiming to find masked-priming 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 mask duration, and stimulus characteristics that determines the grade of (non)awareness. Mask duration in Lähteenmäki et al.'s study was very long compared to the prime (i.e., 140 ms in the 10 ms prime condition) so that one can assume that the signal-to-noise ratio was strongly biased toward noise. Thus, 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 and mask durations and a shorter SOA would be desirable to obtained a masked-priming effect.

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 Experiment 4 and p. 354f for Experiment 5). Thus, it is conceivable that the correlation of prime awareness and priming scores was simply a spurious one, 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.'s (2015) Experiments 4 and 5 is presented in Supplemental Material 1. In a nutshell, LMM analyses of both experiments showed a three-way interaction of awareness, duration, and priming.[4] 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 duration 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 (i.e., trials with PAS ratings of 3 or 4): 59.6% and 83.4% of all valid trials in Experiments 4 and 5, respectively. 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 Experiment 5), the status of the masked-priming effect under conditions of subjective unawareness was statistically ambiguous (see Supplemental Material 1 for further explanation). Thus, a replication study including the PAS suggests itself.

An Additional Critical Detail for Inferences Regarding Underlying Mechanisms

On p. 352, Lähteenmäki et al. (2015) revealed a further important detail about their priming experiments. They wrote: "Each picture was presented 16 times in each block (four times as a nonconscious prime, four times as conscious prime, and eight times as probe)." This means that the same stimuli appeared as the target (i.e., "probe") 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 et al., 2003). If, on a different trial, the same stimulus is then presented as a prime, it can serve as a retrieval cue for the prior 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 regards to the 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

[4] Following the logic outlined above, a rating of 3 or 4 was coded as aware, a rating of 1 or 2 as unaware.

tulip (two positive words) had been evaluated, a hybrid nonword prime combining both words (*hulip*) was processed as if it were a positive word; even a hybrid word prime that itself is clearly negative (*tumor*) was processed like a *positive* word. Furthermore, evaluative masked-priming effects with often-repeated pictorial primes that also served as targets have been shown to depend exclusively on stimulus–response 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 new response priming experiments that employed (or, as a control, did not employ) a trial-by-trial PAS rating. Importantly, we used task parameters that we deemed more adequate to test nonconscious affective processing, based on the outlined theoretical and empirical literature.

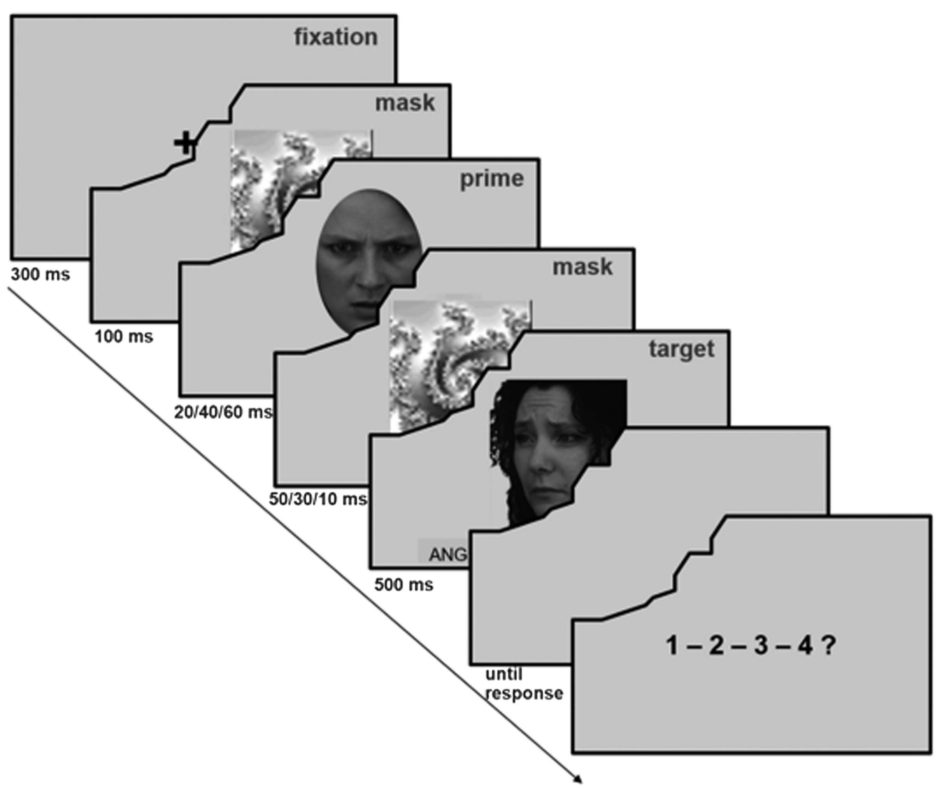
Overview of Experiments

We report 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 versus 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), facelike 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 et al., 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 et al., 2015; Rohr & Wentura, 2014), we used a black-and-white fractal image (see Figure 1).

Figure 1
Schematic Depiction of a Priming Trial in Experiment 1



Note. In Experiment 1a, facial stimuli were colored; see Text. "ANG" is the truncated "ANGER". (The response alternatives were displayed at the bottom of the target screen, left, and right according to the key assignment.)

We decided to use three prime durations in Experiments 1 and 2 (20 ms, 40 ms, and 60 ms) and a constant SOA of 70 ms (i.e., the different prime durations were associated with different signal-to-noise ratios). Mask duration was 50/30/10 ms with prime durations of 20/40/60 ms, respectively. Given our earlier research using this kind of masking,[5] we expected a prime 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 clear above-chance performance in the direct test (see also Kiefer, Harpaintner, et al., 2023, for this rationale). In Experiments 2a and 2b, we omitted the prime duration variation and used only the 40 ms condition.

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 or categorized by participants as angry or sad before the direct test. Moreover, to avoid perceptual priming, primes were cropped frontal-view images and targets 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—in the way Lähteenmaki et al. did it—*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 et al., 1989; Kahan, 2000).

Indeed, recently we (Kiefer, Harpaintner, et al., 2023) conducted a masked *semantic priming* experiment with the lexical decision task (e.g., testing whether the prime "bread" facilitates lexical processing of "butter"; McNamara, 2005), comparing a standard condition (i.e., trials without PAS rating) with a PAS rating condition. For the standard condition, typical masked-priming effects were found. By contrast, these were entirely absent in the overall analysis (i.e., disregarding PAS ratings) of the PAS-present sample (which, by the way, produced markedly longer reaction times). This finding strongly suggests that assessing subjective perceptual experience on a trial-by-trial basis heavily interferes with the processes typically underlying masked semantic priming, presumably due to attentional demands associated with concurrent prime identification.

Lähteenmäki et al. (2015) conceded as well that there were differences between those participants performing the standard task and those performing the priming-plus-rating task with regard to response latencies. Therefore, subsamples in the present Experiments 1a and 1b received the priming task with or without the PAS ratings, respectively; differences between PAS-present and PAS-absent samples can shed light on the validity issue (as in Kiefer, Harpaintner, et al., 2023). Furthermore, we conducted two separate experiments with a sequential Bayes factor logic (see e.g., Schönbrodt et al., 2017), with Experiment 2a being a standard experiment in the tradition of Wentura and Rohr (2018) and Experiment 2b again including PAS ratings. In this experiment, prime durations were restricted to 40 ms only, as this condition should most likely yield a masked-priming effect without awareness. As usual in the standard paradigm (see above), all participants across all experiments were given an objective prime categorization task at the end of the experiment.

To anticipate, Experiments 1 and 2 tell somewhat different stories with regard to the use of the PAS in evaluative priming research; however, both do not corroborate the story of Lähteenmäki et al. (2015). Contrary to Lähteenmäki et al., Experiment 1 (with its variation of prime durations) yielded evidence for priming in trials with subjectively unaware primes (i.e., trials with PAS ratings 1 or 2). Using only a single prime duration in Experiment 2b resulted in no priming at all, neither for PAS Levels 1 and 2, nor for Levels 3 and 4. Experiment 2a (i.e., PAS-absent sample), however, replicated the priming effect. Thus, Experiment 2 indicates that the method introduced by Lähteenmäki et al. is not a valid one, at least in some contexts.

Experiment 1

In Experiment 1a, we used the task parameters described above. Most importantly, half the participants were given the PAS, that is, a trial-by-trial subjective awareness rating after the priming task, while the other half completed the priming task in its conventional form. All participants were administered an objective forced-choice prime recognition test afterward.

Experiment 1b was an almost exact replication (see below for details on minor changes); Experiment 1b was preregistered (https://osf.io/w596s). 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 Supplemental Material 2.

Method

Transparency and Openness

We report how we determined our sample size, any data exclusions, all manipulations, and all measures in this study. Raw data and analysis scripts for the experiment can be found at https://osf.io/9t4pk (Wentura et al., 2024). Experiments 1a was not preregistered. This is due to the fact that this experiment was conducted in 2017 (i.e., at a time when preregistration was not the default). Experiments 1b, 2a, and 2b were preregistered before the data were collected (see links above and in the Participants section of Experiment 2).

[5] 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 (Experiment 1) and 20 ms (Experiment 2) and found priming effects. However, as mentioned earlier, in that study we used a different mask. With the fractal mask used here, we previously 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).

Participants

Experiment 1a. One hundred five nonpsychology students from Saarland University ($n_1 = 53$ for the PAS-present sample, $n_2 = 52$ for the PAS-absent sample; 78 women, 27 men, 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 an excessive error rate (ER; i.e., 38%, which is a far-out value in the distribution of mean error rates) and one due to an extremely long mean RT (i.e., $M > 1{,}500$ ms, which is a far out value in the distribution of mean RTs). 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. Therefore, we downscaled our expectation a bit. With $N = 52$, priming effects of $d_Z = 0.40$ (i.e., effects between small and medium according to Cohen, 1988) can be detected with power $1 - \beta = .80$ ($\alpha = .05$). With $N = 105$, differences in priming effects between the two samples of $d = 0.55$ (i.e., medium effects) can be detected with power $1 - \beta = .80$ ($\alpha = .05$). Power calculations were done with G*Power (Faul et al., 2007).

Experiment 1b. Based on Wentura and Rohr (2018) and Experiment 1a, 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 $\alpha = .05$, a sample size of 63 participants is needed (for the PAS-present sample). We recruited $N = 126$ nonpsychology undergraduate students from Saarland University, who participated for a remuneration of €10 ($n_{\text{PAS}} = 63$; $n_{\text{PAS-absent}} = 63$; 78 women, 48 men; 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

We employed a 2 (prime emotion: anger vs. sadness) × 2 (target emotion: anger vs. sadness) × 3 (prime duration: 20 ms vs. 40 ms vs. 60 ms) × 2 (PAS group: PAS-present vs. PAS-absent) design with the first three factors varied within participants and the last factor varied between participants. For the sake of complexity reduction, the two factors of prime emotion and target emotion are henceforth reduced to the factor "priming" with levels congruent (anger/anger; sad/sad) versus incongruent (anger/sad; sad/anger).

Materials

We used the same stimuli as in our previous research (Rohr et al., 2012; Wentura et al., 2017; Wentura & Rohr, 2018). Face stimuli were taken from the Karolinska Directed Emotional Faces database (Lundqvist et al., 1998). The prime pictures showed frontal views of the faces, whereas target pictures depicted profile views to avoid perceptual priming. All pictures were set to a size of 250 × 250 pixel (approx. 77 × 77 mm). Four instances of each expression (anger, sadness, eight pictures in total) served as primes. Additional instances taken from different individuals (i.e., five men and five women in left and right profile view for each expression; 40 pictures in total) served as targets.[6] 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). In the practice blocks, neutral expressions of the same individuals served as primes. In the direct awareness test, 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 1b, primes and targets were presented in black-and-white instead of colored versions, because pretesting 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 personal computers with 17'' cathode ray tube 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 correctly categorize the presented emotional expressions as fast as possible. The response categories (i.e., anger, sadness) were assigned to the "D" key and the "L" key of a standard German QWERTZ keyboard. Reminders of the response categories were presented at the bottom of the blank screen following the target. Assignment of response keys to emotions was counterbalanced across participants (see Figure 1).

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

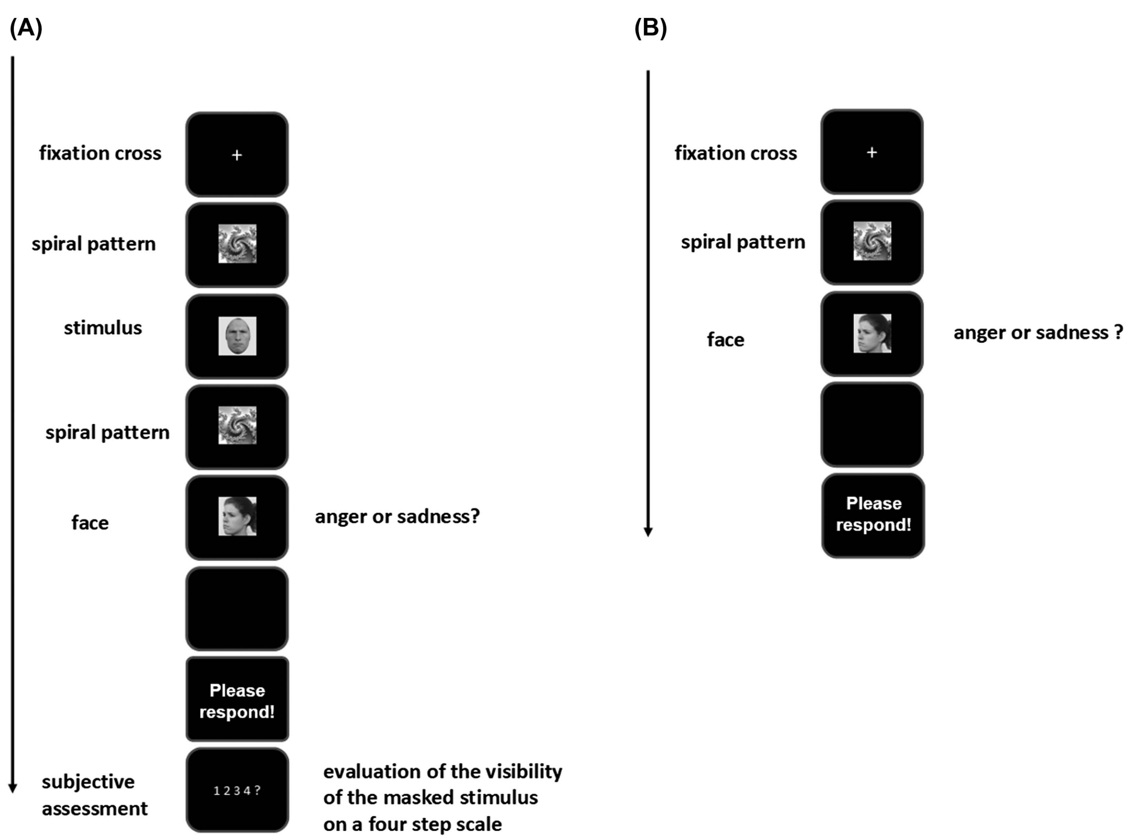
[6] Due to a programming error, additional pictures from three further individuals (one woman and two men) were included in the "anger" target picture pool, resulting in a slightly lower presentation frequency of each picture in the anger target category compared to the sadness target category. The general balance of prime-target conditions was, however, maintained throughout the experiment. The error was rectified in Experiment 1b.

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 1a, some participants had high rates of PAS ratings 3 and 4 in catch trials; therefore, PAS instructions were revised for Experiment 1b. 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 (Figure 2). 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,

Figure 2
Schematic Depictions of the Priming Trials as Shown to (A) Participants in Experiment 1b (PAS-Present) and 2b and (B) in Experiment 1b (PAS-Absent) (B)



Note. PAS = Perceptual Awareness Scale.

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 section).

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 PAS-absent condition differed only with regard to the PAS rating, which was completely omitted. In Experiment 1b, the PAS-absent 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 the standard priming experiments). Furthermore, participants in the PAS-absent 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 1b: First, no reminders of response assignment were presented throughout the experiment; second, the random presentation of targets was restricted to no more than five 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. 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 $p < .05/3$). We additionally report Bayes factors for the priming effects obtained. We used JASP (JASP-Team, 2020) to calculate the Bayes factors with the default prior (i.e., Cauchy scale with scale parameter of $r = \sqrt{2}/2 = .707$). Analyses of the direct test data are provided in Appendix C.

Priming Effects in PAS and PAS-Absent Samples

Error rates were 8.8% ($SD = 5.5\%$) in Experiment 1a and 11.7% ($SD = 6.8\%$) in Experiment 1b. Trials with RTs below 150 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 1a and 4.3% of trials in Experiment 1b.

Mean RTs of correct trials and error rates are reported in Table A1. Analyses related to the moderation of basic response speed by PAS-presence versus PAS-absence are presented in Appendix A as well. Here, it should only be noted that mean RTs are sharply increased in the PAS-present samples: 819 ms (compared to 633 ms for PAS-absent) in Experiment 1a; 1,021 ms (compared to 694 ms for PAS-absent) in Experiment 1b.

For the sake of simplicity, we directly analyzed priming scores (i.e., $RT_{incongruent} - RT_{congruent}$); see Table 1. A 3 (prime duration: 20 ms vs. 40 ms vs. 60 ms) × 2 (PAS group: PAS-present vs. PAS-absent) × 2 (Experiment: 1a vs. 1b) multivariate analysis of variance (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

Table 1
PEs as a Function of Prime Duration and PAS Rating (Experiment 1)

	PAS-present sample									PAS-absent sample		
				PAS								
	Overall			1, 2 ("unaware")			3, 4 ("aware")					
Prime duration	PE	SE_{PE}	N	PE	SE_{PE}	N	PE	SE_{PE}	N	PE	SE_{PE}	N
Experiment 1a												
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 1b												
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 Levels 1,2 and 3,4 columns are caused by removal of participants due to low number of observations (<5) in the respective cell. PE = priming effects; PAS = Perceptual Awareness Scale; *SE* = standard errors.

duration × PAS group, $F(2, 226) = 5.59$, $p = .004$, $\eta_p^2 = .047$, all other Fs < 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 et al., 2011).

For the 40 ms condition, the analysis yielded a significant constant effect (i.e., a positive priming effect), $F(1, 229) = 5.18$, $p = .024$, $\eta_p^2 = .022$, $BF_{+0} = 1.72$, which, however, was significantly moderated by PAS group, $F(1, 229) = 9.14$, $p = .003$, $\eta_p^2 = .038$. Most importantly for the subsequent PAS-related analyses, the priming effect in the PAS-present 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 PAS-absent sample was absent, $|t| < 1$. There was "strong" evidence against the hypothesis of a positive priming effect at 40 ms duration in this group, $BF_{0+} = 15.43$.

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 nonsignificant interaction term did not suggest follow-up analyses, it should be noted that the priming effect for the PAS-present sample was not significant, $|t| < 1$ ($BF_{0+} = 5.00$, i.e., "substantial" evidence for the null), whereas the corresponding effect for the PAS-absent 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).

In addition, 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 Fs < 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).

Thus, to conclude, the duration condition of most interest, the 40 ms prime condition yielded a priming effect as expected; however, only in the PAS-present sample. The 20 ms condition yielded no effect, and the 60 ms prime condition yielded a prime effect, which was more pronounced for the PAS-absent sample. In that regard, please note that the PAS-absent samples in Experiments 1a and 1b yielded heterogeneous results (see Table 1) in contrast to the PAS-present sample (which showed very homogeneous results across the two experiments). 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—i.e., PAS-absent samples), we report further analyses of the PAS-absent samples in Appendix B. The main point is that the overall null effect in the 40 ms condition results from opposite effects in Experiments 1a and 1b. We will briefly address this result in the discussion.

Priming and Subjective Prime Awareness (PAS Rating)

PAS Ratings. Table 2 shows the distribution of PAS ratings across prime durations. As already indicated in the Procedure section, in Experiment 1a, 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 1b (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 (Helmert contrast-coded) by using LMM analysis (using the lmerTest package, Kuznetsova et al., 2016, based on lme4, Bates et al., 2015, of the

Table 2
Distribution of PAS Ratings (Proportions) and Mean Ratings (Standard Deviation) Across the Prime Duration Conditions

	PAS rating					
Prime duration	1	2	3	4	*M*[a]	*SD*[a]
Experiment 1a ($N = 53$)						
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 1a ($N = 34$)[b]						
Catch trials	0.69	0.30	0.01	0.00	1.33	0.50
20 ms	0.44	0.54	0.02	0.00	1.68	0.56
40 ms	0.41	0.53	0.05	0.00	1.80	0.65
60 ms	0.39	0.50	0.12	0.00	1.99	0.81
Experiment 1b ($N = 63$)						
Catch trials	0.69	0.29	0.02	0.00	1.33	0.52
20 ms	0.27	0.67	0.06	0.00	1.79	0.55
40 ms	0.21	0.64	0.13	0.02	1.97	0.65
60 ms	0.16	0.52	0.23	0.08	2.24	0.82
Experiment 2b ($N = 122$)						
Catch trials	0.66	0.32	0.02	0.00	1.36	0.53
40 ms	0.25	0.66	0.08	0.01	1.84	0.59

Note. PAS = Perceptual Awareness Scale.
[a] Means and standard deviation are calculated across the sample of trials (i.e., $N \times 420$). [b] Excludes participants with more than 40% PAS ratings in (3, 4) in catch trials (see text).

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 noncatch trials (Helmert 1);[7] $\beta = -0.065$ ($SE = 0.009$), $t(115.00) = 7.30$, $p < .001$, for 20 ms versus 40/60 ms (Helmert 2); and $\beta = -0.086$ ($SE = 0.011$), $t(115.00) = 7.66$, $p < .001$ for 40 ms versus 60 ms (Helmert 3). Arguably due to the abovementioned issue with the use of the PAS in a subsample of Experiment 1a, 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 1a, with $\beta = -0.050$ ($SE = 0.012$), $t(52.00) = 4.08$, $p < .001$, for catch trials versus noncatch trials; $\beta = -0.020$ ($SE = 0.008$), $t(52.00) = 2.37$, $p = .021$, for 20 ms versus 40/60 ms; and $\beta = -0.028$ ($SE = 0.009$), $t(52.00) = 2.96$, $p < .005$, for 40 ms versus 60 ms. The same was true for Experiment 1b: $\beta = -0.166$ ($SE = 0.013$), $t(62.00) = 12.74$, $p < .001$, for catch trials versus noncatch trials; $\beta = -0.102$ ($SE = 0.013$), $t(62.00) = 7.88$, $p = .001$, for 20 ms versus 40/60 ms; and $\beta = -0.135$ ($SE = 0.017$), $t(62.00) = 8.02$, $p < .001$, for 40 ms versus 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 *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 for repeated measures, the effective sample size would be too small for a meaningful analysis).

Nevertheless, the following points are obvious: First 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, $BF_{+0} = 7.91$. (For 20 ms and 60 ms prime duration and PAS in [1, 2], there was no priming, both $|t|s < 1$; $BF_{0+} = 14.18$ and $BF_{0+} = 12.94$, respectively).

Second, the mean priming scores for PAS Levels 3 and 4 are numerically at the same level as typical priming scores, irrespective of prime duration. However, error variance was substantial (thus, all $|t| < 1.24$, $BF_{0+} = 1.83$, 2.25, 2.04 for 20, 40, 60 ms, respectively). Therefore, one might be inclined to use the aggregation logic of Lähteenmäki et al. (2015), who regarded trials with equal PAS rating as equivalent, irrespective of duration. Thus, we calculated a priming effect based on all trials with PAS Levels 3 and 4, irrespective of prime duration (again with a minimal trial number of 5 per condition). We obtained a sample of $N = 71$ and the priming effect was based on an average of 63 trials per condition (i.e., congruent and incongruent trials, respectively). This priming effect had a mean of $M = 14$ ms ($SE = 14$ ms), $t(70) = 1.04$, $p = .304$, $d_Z = 0.12$, $BF_{0+} = 2.72$ ("anecdotal evidence" against the hypothesis of a positive priming effect). However, this analysis provides an over-estimation. Excluding one extreme outlier (785 ms; i.e., more than 12 interquartile ranges above the third quartile), the mean priming effect was $M = 3$ ms ($SE = 8$ ms), $t(69) = 0.37$, $p = .715$, $d_Z = 0.04$, $BF_{0+} = 5.57$ (i.e., "substantial evidence" against the hypothesis of a positive priming effect). To conclude, aggregated across durations, PAS Levels 3/4 did not yield priming effects; for PAS Levels 1/2, priming with 40 ms duration, as a priori expected, was significant (and nonsignificant for the 20 and 60 ms conditions).

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 versus 40/60 ms (D1) and 40 ms versus 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 nonconvergence (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; there were no significant moderations of any term 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 nonsignificant ($p = .096$); 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 ($p = .023$) which was not moderated by PAS.

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 significant effect of prime duration in the analysis without the PASd factor (see above), we considered this sufficient justification 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

[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.

Table 3
Results of the Linear Mixed-Model Analyses (Experiment 1a, b Combined)

Fixed factor	Weight	*SE*	*df*	*t*	*p*
Only prime duration					
Intercept	928.5	26.7	115.0	34.78	<.001
P	−2.6	1.5	35760.1	1.66	.096
PD1	−5.1	2.2	35760.2	2.37	.018
PD2	−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
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
P	−3.6	1.8	35754.2	1.99	.047
PASd	36.5	2.7	35866.2	13.75	<.001
PD1	−2.0	2.8	35772.0	0.73	.464
PD2	−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
For 20 ms prime duration					
Intercept	944.6	29.0	105.8	32.62	<.001
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
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
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). PAS = Perceptual Awareness Scale; PASd = PAS dichotomized; *SE* = standard error.

60 ms, the emerging pattern corresponded with the hypothesis of Lähteenmäki et al. (2015).[8] However, for the 40 ms condition, a pattern was found that suggests affective priming does not require conscious awareness. We will address this pattern of findings in the General Discussion section.

In anticipation of the General Discussion, we briefly address a secondary finding: In all duration conditions, PAS Levels 3 and 4 (in comparison to PAS Levels 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.

Discussion

Experiment 1 provided evidence against the claim that affective processing requires awareness. As expected, we found 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 nonawareness, that is, trials with PAS ratings 1 or 2. Consequently, Lähteenmäki et al.'s (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.

Furthermore, 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. There are, however, two details that need further consideration. First, it is surprising—given the results for the 40 ms duration—that for the 60 ms prime duration a pattern appeared that seemingly corresponded with the hypothesis of Lähteenmäki et al. (2015), at least in the LMM analysis. That is, the priming factor is moderated by PAS in the sense that there is no priming for PAS in (1, 2), but priming for PAS in (3, 4). But note, there is an alternative description of this interaction (see Table 3). For the 60 ms prime duration condition, the following holds: If PAS is in (3, 4), there is a sharp increase of (2 × 47 ms =) 94 ms in RTs compared to trials with PAS in (1, 2). This increase is significantly larger for incongruent trials (M = 110 ms) compared to congruent trials (M = 78 ms). Thus, one might speculate about a different kind of priming mechanism: If a prime is rudimentarily consciously perceived (i.e., the state that finally end up with PAS in [3, 4]), participants might try to keep the percept in working memory until the rating. During this span, the target percept enters working memory. Rivalry between incongruent working memory entries might prolong the target-related response. We will elaborate this discussion in the General Discussion section.

Second, besides the marked increase in mean RT, the most obvious consequence of the between-participants PAS group factor (PAS-present vs. PAS-absent) was an ostensible shift in the priming effect with regard to the prime duration: In the PAS-absent sample, that is, the standard procedure of masked priming, we only found priming at the 60 ms prime durations, whereas the PAS condition already produced a priming effect with a 40 ms duration.

However, taking a closer look revealed that the null result for the 40 ms duration condition in the combined analysis of the two PAS-absent samples resulted from heterogeneous results in Experiments 1a and 1b. In fact, Experiment 1b yielded a reversed significant priming effect (see Appendix B) and Experiment 1a a significant, "normal" positive priming effect. Reversed effects in response

[8] We are aware that this result seems prima facie to be in contrast to the analysis using aggregated trials, see in Table 1 the priming effect of 18 ms, SE = 14 ms, for 60 ms duration and PAS ratings 3 of 4, which was not significant, $t(67) = 1.24$, $p = .220$, $d_Z = .15$. Note, that in this case—that is, a case with extremely varying number of trials between participants—the traditional analysis and the LMM analysis can be expected to substantially diverge. For example, in the analysis using aggregates, participants are equally weighted irrespective of number of valid trials. Moreover, the LMM analyses are characterized by shrinkage, that is, by a tendency to bias the estimation of parameters (e.g., slopes) toward the mean. Thus, there is no unresolved contradiction.

priming designs have occasionally been found (see e.g., Wentura & Rothermund, 2003) and they can be elegantly explained (Klauer & Dittrich, 2010, Klauer et al., 2009). In a nutshell, if one assumes that responses in binary decision tasks might be given on the basis of a *relative increase* in evidence 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 emerge.

Applying this theory to the present data, we would have to assume that participants in the PAS-absent sample of Experiment 1a aligned the evaluation window with the first mask, such that the prime event was included in the window. This is plausible as the onset of the mask might signal as a cue for the upcoming target, and it is the effect typically observed. However, participants in the PAS-absent sample of Experiment 1b seem to have set a narrower evaluation window that aligned with the second mask, yielding a negative priming effect. This is plausible as the PAS-absent sample in Experiment 1b received thorough instructions and a figure detailing the timing of the critical event (i.e., target presentation), which was not included in Experiment 1a.

Put shortly, Experiment 1 provided initial evidence against the claim that affective processing requires awareness. However, typical masked-priming experiments use only one fixed duration. Thus, we wanted to examine whether the PAS can also be employed under such conditions.

Given the influence of instructions on the PAS-absent sample in Experiment 1, we returned to standard instructions in Experiment 2. Moreover, we fixed prime duration to a constant value (i.e., 40 ms) as in Wentura and Rohr (2018), as this condition is the one most likely yielded masked-priming effect without awareness

Experiment 2

Experiment 2 comprised two parallel studies, Experiments 2a and 2b. Experiment 2a was a replication of the PAS-absent condition of Experiment 1 with two changes: We set prime duration for all trials to 40 ms, and we refrained from including a graphical trial representation in the instructions, which is uncommon in masked-priming research, to avoid the reversed priming effects observed in Experiment 1b. Thus, Experiment 2a resembled a standard response priming experiment.

Experiment 2b was a variation of Experiment 2a; it also featured a constant prime duration but again included trial-wise PAS ratings (and detailed and extensive practice as in Experiment 1b). With Experiment 2, we aimed to corroborate the standard masked-priming effect under objective unawareness and test whether a fixed prime duration can also produce subjectively unaware masked-priming effects (i.e., significant priming at PAS Levels 1 and 2) given that typical masked-priming experiments use only one fixed duration. It is conceivable that some variation in prime duration is needed to give participants an anchor or some motivation to apply the PAS correctly.

Instead of a standard power-planning approach, we opted to apply sequential hypothesis testing with Bayes factors (see e.g., Schönbrodt et al., 2017), for the following reasons: On the one hand, increasing the number of trials from 120 to 360 might result in an increased (but not perfectly predictable) effect size. On the other hand, it may be unreasonable to assume that processing in the 40 ms condition remains identical regardless of whether the condition is embedded in a random sequence of trials with varying prime durations or whether it is the only condition.

Method

Participants

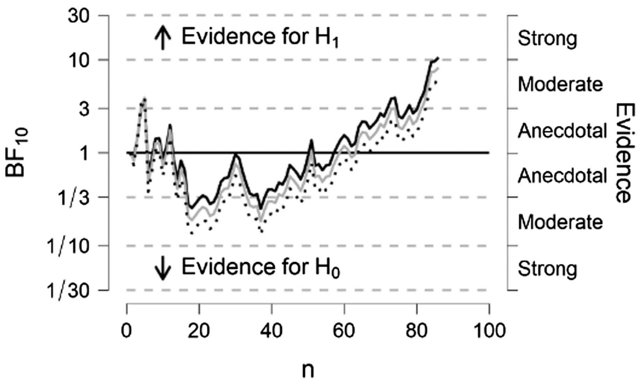
Experiment 2a. Experiment 2a was preregistered (https://osf.io/5eu4z). We used the Jeffreys–Zellner–Siow default Bayes factor (BF) for *t* tests (see Rouder et al., 2009), with a scale parameter of $r = \sqrt{2}/2 = .707$ for the effect size prior, and we aimed to achieve a Bayes factor of either $BF_{10} > 10$ in favor of the hypothesis that there is a priming effect or a Bayes factor of $BF_{01} > 10$ in favor of the null hypothesis.[9] We initially collected data from 30 participants and calculated the Bayes factor for the first time; we then continued data collection and calculated the Bayes factor after each day of data collection. We stopped at a sample size of $N = 86$, with $BF_{10} = 10.57$ ($BF_{+0} = 21.11$; see Figure 3 for the BF evolution); thus, evidence was in favor of a positive priming effect. Figure 3 shows an additional sensitivity analysis; we computed the BF for the other two default priors that are recommended (see e.g., Schönbrodt et al., 2017; $r = 1$: $BF_{10} = 8.23$; $r = \sqrt{2}$: $BF_{10} = 6.15$). Hence, the conclusion can be considered robust with respect to reasonable variations of the effect size prior. During recruitment, we continuously checked our preregistered outlier criterion of having an accuracy rate of at least 60%; there were no outliers.

Experiment 2b. Experiment 2b was also preregistered (https://osf.io/fex3y). We again used the Jeffreys–Zellner–Siow default BF with the default $r = \sqrt{2}/2 = .707$ prior, and we aimed to achieve either $BF_{10} > 10$ in favor of the hypothesis that there is a priming effect for PAS Levels 1 and 2 (combined) or a Bayes factor of $BF_{01} > 10$ in favor of the null hypothesis (that there is no priming effect for PAS Levels 1 and 2, combined). Again, we initially collected 30 (valid, see below) data sets (and calculated an initial BF) and then continued data collection, calculating BF at the end of each day. In a slight deviation from preregistration, we stopped at a sample size of $N = 122$ with $BF_{01} = 8.16$ ($BF_{0+} = 15.39$; see Figure 4 for the BF evolution) after having again faced a prolonged period of poor participant recruitment due to a semester break. This decision seems justified given a sensitivity analysis (see Schönbrodt et al., 2017) showing that an alternative standard prior ($r = 1$; recommended, e.g., by Rouder et al., 2009) leads to a $BF_{01} = 11.42$ ($BF_0+ = 21.66$).[10]

[9] The preregistration specified a criterion at six instead of 10; we set this as acceptable criterion (see, e.g., Schönbrodt et al., 2017) in light of the difficulties with participant recruitment during the COVID-19 pandemic. We decided to increase the criterion to 10 to (a) stay in line with the preregistration of Experiment 2b and (b) because it demarks a further step (from "substantial evidence" to "strong evidence") in the heuristic classification scheme by Jeffreys (1961, see also Wagenmakers et al., 2011).

[10] Even Schönbrodt et al. (2017, p. 332) recommended $r = 1$ for sequential Bayes strategies. However, simulations by Schönbrodt et al. indicated that the power to confirm small effects is clearly increased for $r = .707$ compared to $r = 1$ with only a negligible increase of the false positive rate (if $BF > 10$ is taken as a stopping rule). Thus, we choose $r = .707$. However, what we underestimated is the price to be paid in terms of the effort to confirm the null hypothesis.

Figure 3
Evolution of the Bayes Factor in Experiment 2a for the Priming Effect



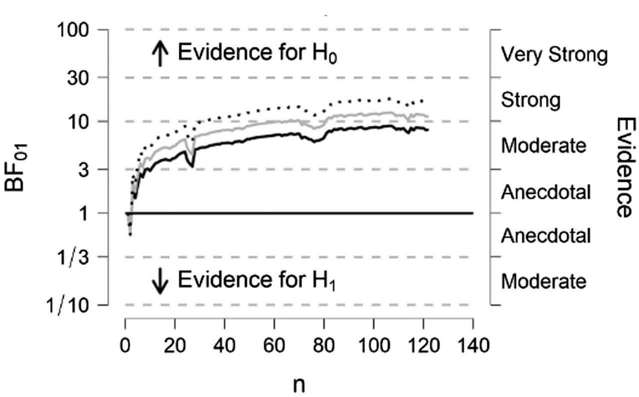
Note. Output from JASP (JASP-Team, 2020); the solid line refers to the chosen prior of $r = \sqrt{2}/2$; the grey line refers to $r = 1$, the dotted line refers to $r = \sqrt{2}$. H = hypothesis.

Thus, evidence was in favor of the null hypothesis. During recruitment, we continuously checked our preregistered outlier criteria. The data of $n = 20$ additional participants was discarded because they had a catch-trial PAS rating >2 (i.e., they indicated prime visibility when there was no stimulus) in more than 25% of trials. The data of one further participant were discarded because they had fewer than 20 trials with PAS ratings of 1 or 2 (combined). Finally, for one participant, there was a technical error during the priming phase.

Materials, Design, and Procedure

Experiment 2a was a replication of Experiment 1b (PAS-absent) with two changes: (a) prime duration was set to 40 ms for all trials; (b) we refrained from giving a graphical trial representation in the instruction phase (as used in Experiment 2). Experiment 2b was a replication of Experiment 1a (PAS-present) with prime duration set to 40 ms for all trials.

Figure 4
Evolution of the Bayes Factor in Experiment 2a for the Priming Effect in Trials With PAS Levels 1 or 2



Note. Output from JASP (JASP-Team, 2020); the solid line refers to the chosen prior of $r = \sqrt{2}/2$; the grey line refers to $r = 1$, see text; finally, the dotted line refers to $r = \sqrt{2}$. PAS = Perceptual Awareness Scale; H = hypothesis.

Results

Outlier criteria were identical to Experiments 1 (3.7% [Experiment 2a] and 4.2% [Experiment 2b] of trials were discarded due to outlying values). Mean error rates were 12.4% (SD = 6.4%) and 11.6% (SD = 6.5%) for Experiments 2a and 2b, respectively. Mean RTs and errors rates are reported in Table A1.

Experiment 2a

Table 4 shows the overall positive priming effect, $d_Z = 0.34$; $BF_{10} = 10.57$ ("strong evidence"). Table C1 shows the direct test result; d' was positive, but as low as in the other experiments, $d_Z = 0.27$; $BF_{10} = 1.93$ ("anecdotal evidence"). The correlation between the priming effect and d' was $r(84) = -.03$, $BF_{01} = 7.20$ ("substantial evidence" in favor of the null). That is, there was no hint of an increase in priming with increasing objective categorization performance. Appendix D gives a brief summary of the subjective prime awareness report (gathered by the funneled debriefing). Priming effects were not significantly different for subsamples indicating (vs. not indicating) some form of subjective awareness in a mnemonic self-report. For errors, the mean priming difference (i.e., $\text{errors}_{\text{incongruent}} - \text{errors}_{\text{congruent}}$) was $M = -0.3\%$ ($SE = 0.4\%$), $BF_{01} = 6.01$ ("substantial evidence" in favor of the null).

Experiment 2b

Table 4 shows the overall priming effect (i.e., disregarding the PAS rating); $BF_{01} = 4.71$ ("substantial evidence" in favor of the null; $BF_{0+} = 21.16$, i.e., "strong evidence" for the absence of a positive priming effect). Table 2 shows that the distribution of PAS ratings across catch and prime trials was comparable to Experiments 1a,b. Thus, the high degree of confusability between PAS Levels 1 and 2 is again given. In addition, Table 4 shows the priming effects for PAS Levels 1 and 2 (collapsed) and Levels 3 and 4 (collapsed), respectively, calculated analogously to Experiments 1a and 1b (i.e., minimal number of valid trials $n = 5$ for congruent and incongruent conditions, respectively). Numerically, for PAS Levels 1 and 2, the priming effect was close to zero, $BF_{01} = 8.16$ (i.e., as already reported above, "substantial evidence" for the null; $BF_{0+} = 15.39$, i.e., "strong evidence" for the absence of a positive priming effect). For PAS Levels 3 and 4, the priming effect was numerically negative, $BF_{01} = 1.26$ ("anecdotal evidence" for a priming effect; $BF_{0+} = 20.24$, i.e., "strong evidence" for the absence of a positive priming effect). As per the preregistration (though no longer indicated given the null result), Table 5 shows an LMM analysis. An analysis with the congruency factor as the sole predictor again showed clear absence of a priming effect in Experiment 2b. Adding PASd and the interaction term yielded no evidence for a moderation of priming by subjective awareness. However, we again observed that trials with a PAS ratings of 3 or 4 were associated with substantially increased target RTs. This increase of 148 ms corresponds to (and even tops) the increase for the 60 ms duration condition in Experiment 1.

Table 4
PEs in Experiments 2a and 2b as a Function of PAS Rating

				PAS					
	Overall			1, 2 ("unaware")			3, 4 ("aware")		
Prime duration	PE	SE_{PE}	N	PE	SE_{PE}	N	PE	SE_{PE}	N
Experiment 2a (PAS-absent)									
40 ms	6	2	86						
Experiment 2b (PAS-present)									
40 ms	−4	3	122	−2	4	122	−30	14	55

Note. "*N*" denotes the effective sample size for a given priming effect; reduction in sample size in the PAS Levels 3 and 4 column is caused by removal of participants due to low number of observations (<5) in the respective cell. PEs = priming effects; PAS = Perceptual Awareness Scale; *SE* = standard error.

Despite the low Bayes factors, the negatively signed effect of −30 ms ($SE = 14$ ms; $d_Z = .29$) may appear to the reader to be more than a negligible effect. However, it should be noted that the corresponding priming effect for errors is numerically positive (2.6%, $SE = 1.3$ ms; $d_Z = .27$; for the BF, see below). Thus, if one wants to discuss these effects, one has to discuss a speed–accuracy trade-off.

In the analysis of errors, the Bayes factors were $BF_{01} = 9.13$, $BF_{01} = 6.65$, and $BF_{01} = 1.05$ for overall priming, priming at PAS Levels 1 and 2, and priming at PAS Levels 3 and 4, respectively.

Discussion

Experiment 2a provided a clear masked-priming effect under standard conditions of the procedure. This confirms the assumption that the reversed priming effect in Experiment 1b indeed had something to do with the unusual instruction phase.

Further, Experiment 2b failed to reproduce the results we found in the PAS-present samples of Experiments 1a and 1b, with "strong evidence" for the null hypothesis. This result is also *not* compatible with the assumptions of Lähteenmäki et al. (2015), as according to their logic, the significant positive priming effect found in Experiment 2a should have had a counterpart in the overall analysis of Experiment 2b. However, neither an overall priming effect, nor a priming effect for subjectively aware primes was found. If at all, subjectively aware trials were associated with a speed–accuracy trade-off regarding the congruence factor.

Table 5
Results of the Linear Mixed-Model Analysis in Experiment 2b

Fixed factor	Weight	*SE*	*df*	*t*	*p*
Step 1					
Intercept	1003.6	23.6	121.0	42.46	<.001
P	2.4	1.70	5385.1	1.40	.161
Step 2					
Intercept	1076.2	27.9	112.5	38.52	<.001
P	7.6	3.2	99.0	2.40	.018
PASd	80.7	9.3	63.7	8.64	<.001
P × PASd	5.6	3.3	60.6	1.71	.093

Note. Coding of predictor variables was as follows: *Priming* (P): −1 = incongruent, +1 = congruent; *PASd*: −1 = (1, 2), +1 = (3, 4). PAS = Perceptual Awareness Scale; PASd = PAS dichotomized; *SE* = standard error.

General Discussion

This research was inspired by Lähteenmäki et al.'s (2015) claim that "affective processing requires awareness," which was based on a priming procedure that included a trial-by-trial subjective awareness measure. We reasoned that their study might not provide conclusive evidence to address this issue.

Beside temporal parameters (i.e., prime durations, SOAs), we emphasized that Lähteenmäki and colleagues simply did not test the hypothesis of whether "affective processing requires awareness" at all because they used primes that also appear as targets and did not use LMM analyses to separate the influences of prime duration and subjective (un) awareness.

Thus, we provided a re-analysis of their experiments as well as own empirical work targeting this question. Moreover, our own empirical work initially started with the tentative 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 to assess priming. However, there was some concern that the introduction of awareness ratings—that is, a change from a single task to a dual task—may lead to a change in the processes involved, which could invalidate results. This skepticism was fed by an experiment on *semantic priming* that we conducted in parallel to the present study (Kiefer, Harpaintner, et al., 2023). Therefore, we implemented a parallel structure of experiments: Experiments that included the PAS rating were accompanied by standard priming experiments without PAS ratings to have reference conditions. These reference conditions were basically replications of Wentura and Rohr (2018).

Overall, our experiments supported the claim that masked priming does not require awareness. In Experiment 1, we found clear evidence for priming at PAS Levels 1 and 2 in the PAS-present sample; in Experiment 2, we found a standard priming effect at 40 ms under conditions of little or no awareness (as in Experiments 1a and 2a, PAS-absent sample).

However, both experiments also yielded some unexpected results: In Experiment 1 (PAS-absent), the 40 ms prime conditions yielded a null result. As delineated in the discussion of Experiment 1 (and as can be seen in Table 1 and Supplemental Material 2), this result is explainable with a positively signed effect in the subsample of the aggregated experiment, that did not receive detailed instructions and an image of the procedure (Experiment 1a), and a negatively signed effect in the subsample that received a depiction of the trial procedure and very detailed instructions (Experiment 1b). Thus, this result is easily explainable from a evaluation window perspective (see Klauer et al., 2009 and discussion of Experiment 1).

Finding comparable overall priming effects (i.e., priming effects calculated without consideration of PAS ratings) for the PAS-present and PAS-absent samples would tentatively indicate that the PAS procedure does not interfere with priming; this would allow examination of the question whether priming effects can be found with subjectively unaware primes (i.e., PAS ratings 1 or 2) and/or only subjectively aware primes (i.e., PAS ratings 3 or 4; as Lähteenmäki et al., 2015, suggested). Indeed, the results of Experiments 1 (PAS-present) fit this logic: (a) we found overall substantial priming for the 40 ms prime duration (i.e., the procedure did not prevent priming effects from occurring), while (b) the PAS ratings themselves seemed to have had some validity (i.e., they reflected prime presence vs. absence and variation of prime duration in meaningful ways; see Table 2). However, it has to be acknowledged that adding the PAS rating sharply increased the mean target RT of the priming task.

Beyond, the results of Experiments 2b, with a constant prime duration of 40 ms, as well as our semantic priming study (Kiefer, Harpaintner, et al., 2023) question the employment of the PAS as a neutral tool to assess awareness in masked-priming research. We found a null effect for the PAS-present sample with a fixed prime duration of 40 ms in Experiment 2b (see also Kiefer, Harpaintner, et al., 2023). In the following, we will thus discuss the claim of whether masked priming requires awareness, and the usage of the PAS in masked priming separately. Both discussions lead us, however, to the conclusion that priming results obtained in studies involving the PAS procedure do not support the inference that "affective processing require awareness."

The PAS With Varying Prime Durations

To shortly repeat, in Experiment 1, using different task parameters than Lähteenmäki et al. (2015; i.e., prime durations of 20, 40, and 60 ms instead of 10 and 80 ms; a SOA of 70 ms instead of 150 or 300 ms; and primes and targets from distinct sets), there was no priming at the shortest prime duration (i.e., 20 ms) and the PAS rating did not moderate priming. For the 40 ms condition, we found a clear overall priming effect in Experiments 1a, b (PAS-present samples) even for PAS Levels 1 and 2, suggesting that priming effects can arise under conditions of subjective unawareness

However, if we assume for a moment that the 40 ms condition had been omitted from our design, one might have taken the 60 ms prime condition of Experiment 1 as support for Lähteenmäki et al.'s (2015) claim. Whereas for the lower PAS levels (i.e., 1 and 2) a priming effect was completely missing, for PAS ratings >2 there was a priming effect, at least in the LMM analysis. Thus, we find our a priori criticism of Lähteenmäki et al.'s (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) produced ambiguous results, probably due to individual differences in dividing attention and processing resources between the two tasks, which produced very long reactions times and may have interfered with the automatic priming processes. Indeed, participants were overall considerably slower in the target task (more than 180 ms in Experiment 1a and more than 320 ms in Experiment 1b; see Table A1), compared to the PAS-absent 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. Inclusion of an adequate intermediate prime duration thus turned 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 interstimulus interval with unknown consequences) or let the duration of the mask vary 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. This assumption is corroborated by the sharp increase of subjective and objective prime visibility from 40 ms to 60 ms prime durations.

Considering the sharp increases in reaction time, one might speculate about the potential underlying differences in processing prime and target if a PAS rating is instructed. Specifically, translating a subjective impression into a PAS rating might go along with a different kind of mechanisms underlying the priming effect as a by-product (for evidence in the domain of masked semantic priming, see Kiefer, Harpaintner, et al., 2023). In detail, 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. Thus, 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; see also Kiefer, Harpaintner, et al., 2023). Future research might put these speculations to the test.

Usage of the PAS With a Fixed Prime Duration

Experiment 2b aimed to test the consequences of removing the variation of prime (and mask) duration from a PAS-present experiment. As a result, no priming effect was found at all, even for subjectively aware primes (PAS Levels 3 and 4). An advocate of Lähteenmäki et al.'s (2015) position might argue that the main finding of Experiment 2b was an absence of priming for subjectively unaware trials (PAS Levels 1 and 2). However, the reference study (i.e., Experiment 2a) did yield a priming effect. Thus, if inclusion of the PAS procedure would have allowed for a valid assessment of priming effects, we should have also found a priming effect in Experiment 2b overall, and specifically on trials with PAS Levels 3 and 4. This was not the case. If at all, there was a hint toward a negatively signed priming effect for RTs that stands in contrast to a positive priming effect for errors; that is, we are faced by a speed–accuracy trade-off.

Thus, the results pattern of Experiment 2a versus Experiment 2b resembles what we found in our semantic priming study (Kiefer, Harpaintner, et al., 2023): A replication of the masked-priming effect with the standard procedure, but a failure to find the effect in the procedure including trial-by-trial PAS ratings.

Thus, a variation of prime durations seems necessary to find priming effects with a trial-by-trial awareness rating task. Using a constant prime duration—which seems most promising from the perspective of the standard approach (i.e., a duration short enough to produce zero or near-zero direct categorization performance, but with sufficient "prime energy" to cause effects; 40 ms in our specific case)—does not seem to be a sufficient condition to elicit priming effects if a rating task is included in the procedure. Why did it not work? The fact that the introduction of the PAS rating increased mean response times from roughly 650 ms (Experiment 2a) to 1,000 ms (Experiment 2b) makes clear that the validity of the priming task may be reduced because resources are severely burdened by one or more additional time-consuming processes. It may be that part of the rating task—for example, storing the fleeting impression of the mask-prime-mask sequence in working memory while not responding to the prime valence—is completed before the target-response is triggered. It is not very puzzling that such additional processes may "contaminate" the influence of the prime.

However, this argument could apply to the PAS-present samples of Experiments 1a and 1b as well. In this regard, the above-mentioned evaluation window account (Klauer et al., 2009) is critical: Even if the PAS procedure introduces additional processes, prime duration variation reduces controllability (of the evaluation window, of how attention and processing resources should be allocated) so that PAS-present conditions can yield priming effects. Importantly in this regard, priming effects at 40 ms and 60 ms prime duration might be based on different mixtures of processes: At 60 ms duration, with relatively high prime visibility, participants might store a prime representation in working memory; this temporarily maintained representation may give rise to a priming effect, whereas the possible influence of unaware processes has already faded because of the long reaction time. However, at 40 ms duration, with low prime visibility, the activated prime representation may not be actively maintained, and conscious access to the representation might not even be possible, such that the prime influences target processing via other processes.

If the prime duration is short but constant (i.e., in Experiment 2b), participants had to carefully evaluate visibility of the prime and translate this into a PAS rating on every trial; these processes can potentially interfere with priming, as we proposed for the long prime duration trials of Experiments 1a and 1b. This interpretation is supported by the fact that the target RT increase in trials associated with a PAS rating of "3" or "4" matched the corresponding increase in the 60 ms condition of Experiment 1 more closely than the increase in the 40 ms condition of Experiment 1. In other words, participants' effort to perceptually recognize the prime and differentiate it from target and mask may introduce "noise" into the priming process and thus the resultant effects. Note that prime durations of 40 or 60 ms are relatively short and typically produce only a short-lived visual experience, if the stimulus is visible at all. It must therefore remain an open question whether PAS ratings would also abolish priming effects at longer, but constant prime durations (80 ms and more), when primes are mostly or fully visible.

What synopsis can be provided regarding the two lines or argumentation, that is, inferences based on PAS usage and valid implementation of the PAS? Most importantly, we argue that researchers ought to be cautious when applying a trial-by-trial PAS rating in a priming task. Introducing the PAS procedure into the priming task is not without side effects; in the worst case, it can ruin the assessment of masked-priming effects. Our experiments indicate that (a) varying prime durations and (b) including a condition that corresponds to the prime duration used traditionally to demonstrate masked priming seems essential to obtain priming effects when the procedure contains a PAS rating. However, the conclusion from our experimental work is exactly the opposite of what Lähteenmäki et al. (2015) proposed: Affective processing does *not* require awareness. Rather, Lähteenmäki and colleagues' null findings for subjectively unaware primes can be explained by the employed task parameters as well as changed processes underlying masked priming when the PAS is introduced.

Constraints on Generality and Other Limitations

A question that we cannot answer with the present study is whether its results will generalize to other types of affective stimuli, and whether affect-related processes might be involved in priming with 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 pushes processing to go beyond the processing of mere stimulus valence. However, whether our results generalize to other emotion categories or stimulus materials is a question for future research (e.g., stimulus processing times and thus optimal evaluation windows might be different). 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 category priming; Ortells et al., 2016) or contexts that assess emotion-related processing more directly (e.g., additional assessment of facial electromyography, see Rohr et al., 2018).

The participants in our study were students at the University of Saarland, Germany, and therefore human. Since our theoretical considerations and hypotheses concern general cognitive processes in humans, we had the opportunity to falsify our claims. It is not really necessary to mention that, as always in psychological research, there may be currently unseen or uninvestigated arguments why the effects presented in this article depend on further specific parameters; those who have these arguments will certainly take the chance to test their hypotheses.

Conclusions

With this article, we aimed to connect the research of trial-by-trial subjective awareness ratings in a priming task (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 claim that affective processing is limited to conditions of stimulus awareness. To summarize, in our reference conditions (i.e., standard priming procedures without subjective awareness ratings), we found masked-priming effects in a 2 (prime: angry face vs. fearful face) × 2 (target: angry face vs. fearful face) design with target emotion categorization as the task under conditions of zero or near-zero objective nonawareness (i.e., 40 ms prime duration and chance-level objective forced-choice categorization for most of the participants). This is in line with the earlier research (Wentura & Rohr, 2018).

Introducing the PAS rating into the task yielded different results depending on whether varying prime durations were used (Experiments 1a, b) or not used (Experiment 2b). With varying prime durations, we found 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. With a constant prime duration of 40 ms, no priming was found, despite significant priming in the reference (PAS-absent) condition. The results of the present study do not only refute the claims by Lähteenmäki et al. (2015), they also provide additional evidence regarding the implications of introducing the PAS and the importance of even minor changes in fixed versus varied prime duration.

Using different sets of primes and targets (in contrast to Lähteenmäki et al., 2015) is important in this regard, as it rules out stimulus–response associations as the basis of the priming effects. Furthermore, present 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. We conclude that affective processing does not require subjective awareness.

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 subgroups (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 automatic processing of affective stimuli (e.g., with different stimulus materials such as emotive images) and the underlying processes (i.e., cognitive vs. physiological, affect-related), and explore the relation of these processes to subjective and objective (un)awareness using different awareness measures, materials, and paradigms in order to gain further insights into automatic affective and semantic processing.

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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: 1a vs. 1b) multivariate analysis of variance (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.

Table A1

Mean Response Times in Milliseconds and Error Rates as a Function of Priming Condition, Prime Duration, and PAS Group

	Priming			
	Congruent		Incongruent	
Prime duration	RT	ER	RT	ER
	Experiment 1a			
PAS-absent				
20 ms	634	0.08	632	0.09
40 ms	629	0.09	636	0.09
60 ms	624	0.09	643	0.09
PAS-present				
20 ms	815	0.08	813	0.09
40 ms	808	0.09	823	0.09
60 ms	823	0.09	830	0.08
	Experiment 1b			
PAS-absent				
20 ms	699	0.12	702	0.13
40 ms	696	0.11	686	0.12
60 ms	686	0.13	693	0.12
PAS-present				
20 ms	1,019	0.11	1,014	0.12
40 ms	997	0.10	1,013	0.11
60 ms	1,041	0.11	1,044	0.11
	Experiment 2a			
PAS-absent				
40 ms	655	0.13	662	0.12
	Experiment 2b			
PAS-present				
40 ms	1,006	0.12	1,001	0.12

Note. Values in parenthesis are error rates in percentage. PE = priming effect (i.e., $RT_{incongruent} - RT_{congruent}$). PAS = Perceptual Awareness Scale; RT = response time; ER = error rate.

There were several significant between-participants effects with regard to basic speed. Experiment 1b yielded slower responses than Experiment 1a, $F(1, 227) = 22.96$, $p < .001$, $\eta_p^2 = .092$. PAS-present samples had slower responses than PAS-absent samples, $F(1, 227) = 87.07$, $p < .001$, $\eta_p^2 = .277$; this effect was more pronounced in Experiment 1b, $F(1, 227) = 6.67$, $p = .010$, $\eta_p^2 = .029$. These results are obviously due to the dual-task demands in the PAS-present samples and the improved instructions in Experiment 1b.

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: 1a vs. 1b) MANOVAs for repeated measures, separately for the PAS-present and PAS-present samples.

PAS-Present

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 1a) and $M = 37$ ms ($SE = 8$ ms; Experiment 1b) slower compared to responses in the 40-ms condition.

PAS-Absent

The main effect of duration was nonsignificant, $F(2, 112) = 2.94$, $p = .057$, $\eta_p^2 = .050$. It was, however, moderated by experiment. For Experiment 1a, there was no effect of duration, $F < 1$. For Experiment 1b, 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.

(*Appendices continue*)

Appendix B

An Analysis of the PAS-Absent Samples in Experiment 1

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 1a, 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 1b, 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 nonsignificant positive effect in the 60 ms condition, $F(1, 62) = 2.38$, $p = .128$, $\eta_p^2 = .037$.

Appendix C

Priming and Objective Prime Awareness: Analyses of the Direct Test Data of Experiment 1

For the sake of completeness, we report analyses on the direct prime categorization data—and how they relate to the priming data. 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 C1. We report the data separately for PAS-absent and PAS samples because priming effects differed somewhat across samples.

PAS Sample

A 3 (prime duration: 20 ms vs. 40 ms vs. 60 ms) × 2 (Experiment: 1a vs. 1b) 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 Fs < 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 nonsignificant, $t(115) = 1.64$, $p = .104$, $d_Z = 0.15$. 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).

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 $p(\chi^2) < .05$, priming was $M = 13$ ms, $SE = 8$ ms, $t(37) = 1.61$, $p = .116$, $d_Z = 0.26$.

PAS-Absent Sample

Direct categorization was roughly the same for the PAS-absent sample (see Table 4). (An overall analysis including PAS group as

Table C1
Direct Prime Categorization: Mean d′ as a Function of Group and Prime Duration

Group	Prime duration: 20 ms	40 ms	60 ms
Experiment 1			
PAS-absent	0.05 (0.04)	0.08 (0.06)	0.38 (0.11)
PAS-present	−0.06 (0.06)	0.06 (0.04)	0.38 (0.10)
Experiment 2			
PAS-absent	0.05 (0.04)	0.09 (0.03)	0.13 (0.04)
PAS-present	0.01 (0.03)	0.02 (0.03)	0.23 (0.05)
Experiment 2a			
PAS-absent		0.06 (0.02)	
Experiment 2b			
PAS-present		0.05 (0.02)	

Note. Standard errors are in parentheses. PAS = Perceptual Awareness Scale.

(*Appendices continue*)

a factor yielded no effects involving PAS group, all Fs < 1.25.) A 3 (prime duration: 20 ms vs. 40 ms vs. 60 ms) × 2 (Experiment: 1a vs. 1b) 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 nonsignificant, $t(114) = 1.91$, $p = .059$, $d_Z = 0.18$ ($|t| < 1$ 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 1a, in terms of effect sizes it was almost the same outcome, $t(51) = 3.63$, $p < .001$, $d_Z = 0.50$, for Experiment 1a, and $t(62) = 3.34$, $p = .001$, $d_Z = 0.42$, for Experiment 1b. 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 the 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), $t(29) = 5.05$, $p < .001$, $d_Z = 0.92$.

Appendix D

Results for the Subjective Awareness Data (Funneled Debriefing) in Experiment 2a

Item	"Yes"		"Yes" versus "no"		
	n	%	*t*(79–83)	*p*	*d*
Did you notice that there was a flicker in front of the images?	70	82.4	1.03	.308	0.29
Did you recognize anything in the flickering?	46	54.1	0.64	.523	0.14
And if so, what? ("Faces" mentioned in the answer)	15	17.6	0.24	.745	0.09
Faces were presented in the flicker. Could you tell that they were faces?	27	32.5	0.49	.622	0.12
These faces were also emotional facial expressions. Were you able to recognize the facial expressions?	20	24.7	0.65	.519	0.17

Note. Sample sizes were $N = 81–85$ due to missing values. The final columns refer to t tests comparing the respective "yes" and "no" samples with the dependent variable priming difference (i.e., $RT_{incongruent} - RT_{congruent}$). RT = response time.

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