

Priming of awareness or how not to measure visual awareness

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A foundational issue in the study of unconscious processing concerns whether the stimuli of interest are truly out of awareness. Objective methods employing forced choice are typically championed as the gold standard and widely thought to be conservative. Here, however, as a case study, we demonstrate an underestimation of awareness in a collection of studies on unconscious cognitive control. Specifically, we found that (a) in addition to genuine unawareness, chance performance could be due to a failure to perform the task; (b) visual awareness for low-visibility trials was elevated when mixed with high-visibility trials compared with when presented alone as demonstrated in both objective awareness (forced-choice performance) and subjective awareness (rating based on a perceptual awareness scale); and (c) the elevation effect was partly due to a shape-specific template enhancement at both the block and intertrial levels. We term the awareness elevation effect *priming of awareness*: Visual priming fundamentally alters awareness, boosting otherwise invisible objects into consciousness. These results implicate two key requirements for measuring awareness: (a) verify that participants are truly performing the awareness task and (b) use all types of trials in the awareness test as in the main experiment. Priming of awareness is consistent with an expanded model of awareness and top-down attention in which awareness is determined by (a) retinal stimulus strength and (b) both goal-dependent and goal-independent extra-retinal modulation.

Introduction

Measuring awareness: From subjective to objective methods

Unconscious processing typically refers to the perceptual and cognitive processing of subliminal

stimuli—information that observers could not report on when asked to. Understanding the scope and limits of unconscious processing is central to understanding the functions and mechanisms of consciousness (Kouider & Dehaene, 2007; Lin & He, 2009; Lin & Murray, 2013, 2014). Beneath this quest lies a foundational methodological issue: How to ensure that the processing is indeed unconscious (Greenwald, 1992; Seth, Dienes, Cleeremans, Overgaard, & Pessoa, 2008)?

Earlier work typically adopted participants' reports of seeing or not seeing as evidence of being aware or unaware. This subjective method is now largely dismissed as it conflates awareness with the idiosyncratic criterion for awareness; participants who have low confidence may report a stimulus as invisible when, in fact, they can partially see it (Bjorkman, Juslin, & Winman, 1993). To circumvent this response bias, more refined subjective methods have been proposed. For instance, by measuring participants' ability to discriminate between correct and incorrect responses, bias-free subjective unawareness can be claimed if “subjective sensitivity”—i.e., the difference between Type 2 hit and false alarm rates, operationalized as the proportion of high-confidence trials corresponding with correct and incorrect responses, respectively—is null (Kolb & Braun, 1995; Kunimoto, Miller, & Pashler, 2001).

Even more stringent measures are objective methods employing forced choice. Indeed, compared with subjective methods, objective methods are widely thought to be conservative: While denying perceiving the stimulus and in a state of bias-free subjective unawareness, participants may nevertheless perform better than chance in a forced-choice task (Kunimoto et al., 2001). Accordingly, if subjective methods are the standard for measuring awareness, such above-chance performance can be claimed to partially reflect unconscious processing as in studies of blindsight patients (Poppel, Held, & Frost, 1973; Weiskrantz, Warrington, Sanders, & Marshall, 1974). However, if objective methods are to be used, such above-chance perfor-

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mance actually reflects partial awareness, not unconscious processing.

In theory, to neither underestimate nor overestimate unconscious processing, performance in the direct test of awareness should exclusively exhaust the capacity of conscious processing—being maximally sensitive to conscious processing but minimally sensitive to unconscious processing (Reingold & Merikle, 1988). A liberal method errs on the side of *exhaustiveness*—not exhausting the capacity of conscious processing—and may thus lead to underestimation of awareness. For example, in dichotomous subjective scales of not-see/see (or guess/not-guess, low-/high-confidence), an ostensible unaware outcome could reflect an insensitivity to conscious processing. A conservative method, on the other hand, errs on the side of *exclusiveness* as it could reflect unconscious processing and may thus lead to overestimation of awareness. For instance, in objective forced-choice tasks, an ostensible aware outcome could reflect a contribution from unconscious processing.

Therefore, given that forced-choice performance could partly benefit from unconscious processing, objective methods employing forced-choice have been widely thought to be conservative and typically championed as the gold standard for establishing unawareness, providing convincing evidence for, if not underestimation of, unconscious perception and cognition.

A new look at objective methods: A case study of unconscious cognitive control

But are objective methods always conservative? Consider a typical experiment in unconscious cognitive control. As Figure 1 illustrates, it consists of two phases: a main experiment using a go/no-go task, which is then followed by an experiment measuring awareness (e.g., van Gaal, Ridderinkhof, Scholte, & Lamme, 2010). In the go/no-go task (Figure 1A), a diamond or square is presented and then masked; its duration is either very brief (hence strongly masked, referred to as “invisible”) or long (hence weakly masked, referred to as “visible”). When the target is invisible, participants are asked to press a button (“go”); when the target is visible, the response depends on the shape: Participants also press a button if it is a diamond (“go”) but withhold response if it is a square (“no-go”). The critical finding concerns the invisible target: Participants respond more slowly when it is a square than when it is a diamond even though the two could not be consciously differentiated and both are go trials. This is taken as evidence for unconscious inhibitory control—as if participants unconsciously try to stop responses but fail. To test whether participants are aware of the

shape or not, an awareness test (Figure 1B) is conducted, which typically consists of strongly masked trials (without weakly masked trials); the participants’ task is to determine whether a square or a diamond is presented in each trial. Thus, if performance is not significantly better than chance, participants would be considered to be “unaware” of the shape (otherwise, data from those participants who perform above chance would be excluded).

In such an awareness test, accepting chance performance as evidence of unawareness makes two untested assumptions. It first assumes that chance performance in strongly masked trials is due to (desirable) genuine unawareness rather than (undesirable) failure to perform the awareness task—a common assumption in almost all studies testing objective unawareness. Yet, with chance performance and a task different from the main experiment, one cannot ascertain that participants are truly performing the awareness task. In particular, participants may not understand the task (e.g., lack of adequate instruction), they may not be motivated to perform very difficult trials, or they may simply respond in a way that does not reflect target awareness (e.g., anticipatory responses).

It further assumes that visual awareness is the same whether the strongly masked trials are presented alone (as in a typical awareness test, referred to as the *single* condition) or randomly intermixed with weakly masked trials (as in the go/no-go task during the main experiment, referred to as the *mixed* condition). This assumption is predominantly held in studies of unconscious cognitive control, including those involving go/no-go tasks (in which the awareness test includes either only strongly masked trials or a smaller proportion of weakly masked trials than in the main experiment; see, e.g., Chiu & Aron, in press; Cohen, van Gaal, Ridderinkhof, & Lamme, 2009; van Gaal, Lamme, Fahrenfort, & Ridderinkhof, 2011; van Gaal, Ridderinkhof, Fahrenfort, Scholte, & Lamme, 2008; van Gaal et al., 2010; van Gaal, Ridderinkhof, van den Wildenberg, & Lamme, 2009) and priming tasks (e.g., D’Ostilio & Garraux, 2012; Hughes, Velmans, & De Fockert, 2009).

The current study

Testing these assumptions has important methodological and conceptual implications: the results would not only directly affect whether (and to what extent) one can claim unconscious processing, but also inform a fundamental theoretical issue in consciousness, namely what determines conscious awareness. It is usually assumed that awareness depends on stimulus strength and top-down attentional amplification (Dehaene, Changeux, Naccache, Sackur, & Sergent, 2006).

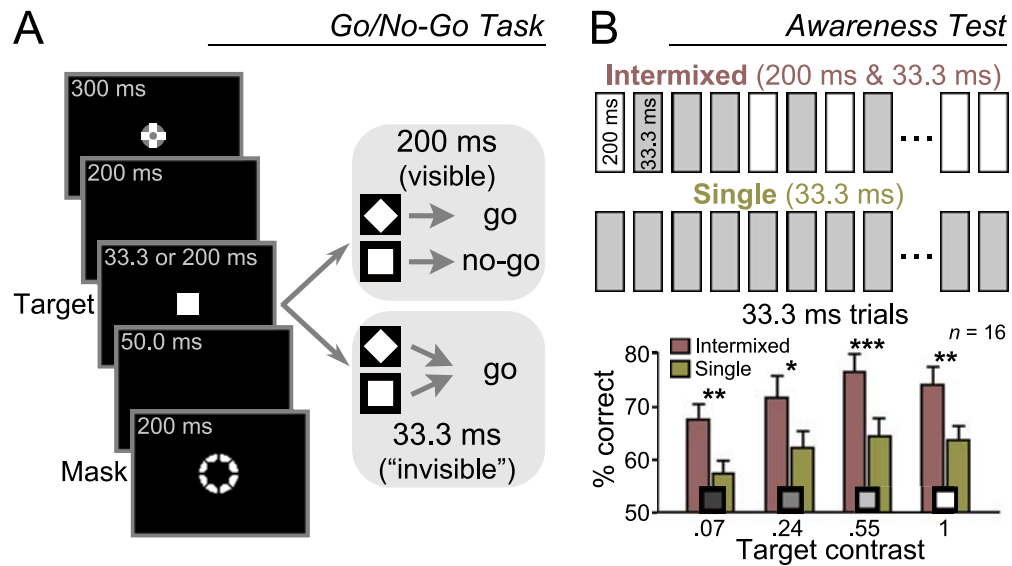


Figure 1. Experiment 1 (a go/no-go task followed by an awareness test). (A) Procedure and design of the go/no-go task. A square or diamond, serving as the target, was presented and then masked. It was presented either for 33.3 ms and thus was strongly masked or for 200 ms and thus was weakly masked. When strongly masked, the target could be one of four contrast levels (.07, .24, .55, or 1). When the target was invisible, participants were asked to press a button (“go”); when the target was visible, the response depended on the shape: Participants also pressed a button if it was a diamond (“go”) but were to withhold response if it was a square (“no-go”). The critical question concerned the invisible target: Would participants respond more slowly when the target was a square than when it was a diamond even though the two could not be consciously differentiated and both were go trials. Square and diamond were counterbalanced across participants as no-go signals. (B) Design and results of the awareness test. Participants were asked to discriminate whether the target was a square or a diamond. There were two different types of blocks: one in which 200-ms trials and 33.3-ms trials were randomly intermixed as in the go/no-go task (*intermixed*) and the other in which all were 33.3-ms trials (*single*). Across all four target contrast levels, forced-choice performance for the 33.3-ms trials was much better when they were randomly intermixed with 200-ms trials than when they were presented alone. *, **, and *** are statistically significant differences at the level of $p < 0.05$, 0.01, and 0.001, respectively. Error bars are SEM.

If so, then, given that these factors are matched between the single and mixed conditions, there should be no difference between the two conditions.

Therefore, the current study tested whether the two assumptions held and why. The results reported here reveal that the two assumptions above are incorrect, and when awareness is correctly assessed, the awareness test described above underestimates the true extent of participants’ awareness. The results are summarized as follows.

First, in traditional designs, it was unclear whether chance performance was due to true unawareness or due to failure to perform the task. Experiment 1A and B demonstrated a simple but effective solution: using weakly masked trials (as in the go/no-go task) as catch trials. Good performance in these trials reassured task understanding and the participants being properly motivated whereas poor performance indicated a failure to perform the task. Using this revised design, Experiment 1A and B also showed that discrimination performance of strongly masked trials was much better in the mixed condition than in the single condition.

This observation was corroborated by Experiment 2, showing that elevation in objective awareness was coupled with an increase in subjective awareness (as measured by a perceptual awareness scale).

Experiment 3 further showed that this effect was due to shape-specific template enhancement that manifested across successive trials and within blocks. Eliminating shape-specific priming could abolish the elevation effect, providing evidence that a difference in task difficulty between the mixed and single blocks is *not sufficient* for awareness elevation (Experiment 4). Experiment 5 found a similar awareness elevation effect when the contribution from task difficulty was removed but priming was preserved (i.e., having participants passively view the weakly masked trials without responding to them in interleaved blocks), suggesting that task difficulty is *not necessary*, and priming is sufficient, for awareness elevation. In addition, by measuring reaction times (RTs), Experiment 5 was able to detect anticipatory responses—responses that did not reflect target awareness (RTs < 100 ms). The results showed that anticipatory responses did exist,

suggesting that it is methodologically necessary to exclude these responses. After excluding them, awareness elevation was still evident without a noticeable reduction in magnitude.

We therefore refer to performance elevation in the mixed condition as *priming of awareness*: Priming fundamentally alters awareness, boosting otherwise invisible objects into awareness. Together, these findings lead us to propose (a) two methodological recommendations for measuring awareness and (b) a revised conceptual model of awareness that builds on an expanded concept of top-down attention.

Experiment 1A and B: Mixing strongly masked and weakly masked stimuli elevates objective awareness

The experimental design of Experiment 1A and B was modeled after van Gaal et al. (2010). The goal was to examine a potential methodological issue identified in this sort of design: an indirect task (in this case, a go/no-go task) that mixes strongly masked trials and weakly masked trials and a direct task (in this case, a forced-choice task) that only includes strongly masked trials. The basic flow in both experiments involved first the indirect go/no-go task and then the direct awareness task using forced choice. The awareness test had two types of blocks: one that consisted only of strongly masked trials as in van Gaal et al. (2010) and the other that intermixed strongly masked trials with weakly masked trials as in the indirect go/no-go task. The second type of block was crucial as it not only allowed one to ascertain experiment participation in the awareness task, but also provided a test regarding whether awareness of strongly masked stimuli was indeed independent of weakly masked stimuli.

Method

Observers and apparatus

Twenty-five participants (13 females, 12 males; average age = 19.5) with normal or corrected-to-normal vision participated in Experiment 1A; a new group of 16 participants (seven females, nine males; average age = 19.4) participated in Experiment 1B. The experiments were conducted in accordance with the IRB approved by the University of Washington.

The stimuli were presented on a 19-in. CRT monitor (ViewSonic G90fB at 60 Hz and 1024 × 768 pixels; peak luminance: 47.1 cd/m²; black level: 0.14 cd/m²). Observers sat approximately 50 cm from the monitor

with their heads positioned in a chin rest in an almost dark room.

Procedure of Experiment 1A

After fixation training (as described in, e.g., Lin, 2013, 2014; Lin & He, 2012; Lin & Murray, 2013, 2014), participants took part in a go/no-go task and then in an awareness test.

The go/no-go task: Figure 1A illustrates each trial in the go/no-go task. A central fixation mark was first presented for 300 ms against a black background (luminance = 0.14 cd/m²); it was a combination of a bull's-eye and crosshairs (diameter of inner circle = 0.16°; diameter of outer circle = 0.50°; luminance = 47.1 cd/m² for crosshairs and 11.5 cd/m² for bull's-eye), on which the participants were told to fixate. The fixation was followed by a blank screen for 200 ms. A target shape, equally likely to be a square (size = 0.47°) or a diamond (size = 0.47°), was then presented for 33.3 ms (referred to as a strongly masked target) or 200 ms (referred to as a weakly masked target). For the 33.3-ms strongly masked target, its luminance was randomly assigned one of four levels (3.28, 11.5, 25.8, or 47.1 cd/m²); for the 200-ms weakly masked target, the luminance was always at the highest level (47.1 cd/m²). Thus, calculated from the formula (target luminance – background luminance)/background luminance, the four Weber contrasts of the strongly masked target were 22.4, 81.1, 183.3, and 335.4; for simplicity, the four levels were normalized to the highest contrast and referred to as levels .07, .24, .55, and 1. After a 33.3-ms blank, the target was masked by an annulus lasting 200 ms (luminance = 47.1 cd/m²; diameter = 0.80°), which was then followed by a 1200-ms blank. For half of the participants, the task was to withhold response if the annulus was preceded by a visible square (“no-go”) but respond by pressing the “Enter” key as quickly as possible if it was preceded by a visible diamond (“go”) or by an invisible shape (“go”); for the other half of participants, the diamond, rather than the square, signaled no-go. Thus, the no-go signal was counterbalanced across participants. Each incorrect response was followed by two sounds and a 5-s pause.

After 12 practice trials (in one block), participants went through 800 go/no-go trials (in 10 blocks): 400 weakly masked trials (all with the highest contrast) and 400 strongly masked trials (100 trials for each of the four contrast levels).

The awareness test: Immediately after the go/no-go task, participants went through an awareness test, which used the same spatial and temporal parameters as in the go/no-go task. The task was to indicate whether the target was a square or a diamond by clicking one of two buttons on a mouse; participants were told that “response time is not important” and were asked to “respond as accurately as possible.” Each

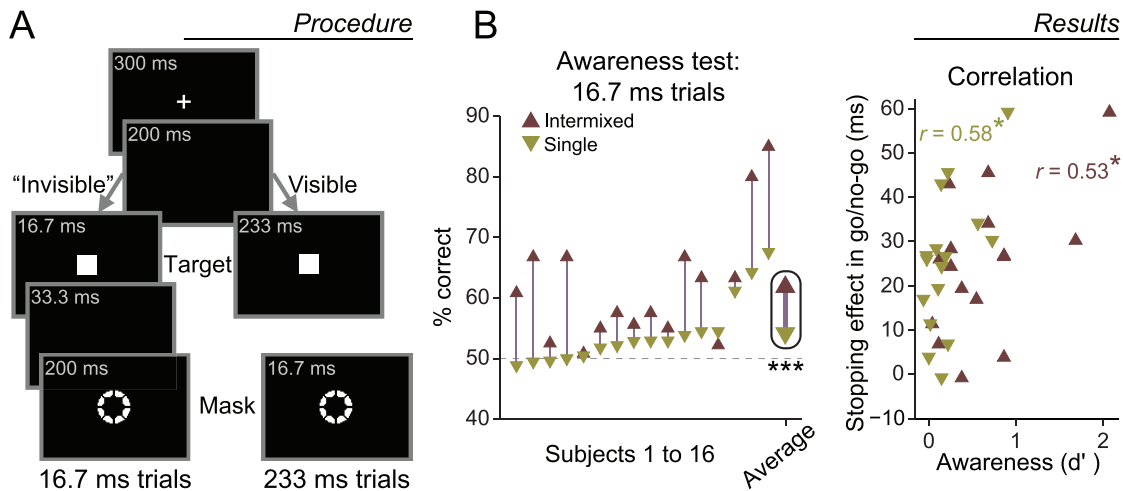


Figure 2. Experiment 1B (a go/no-go task followed by an awareness test). (A) The procedure and stimulus parameters were completely identical to van Gaal et al. (2010), which differed from Experiment 1 primarily in the visible trials (the target was 233 ms, no blank screen was presented afterward, and the mask was 16.7 ms). (B) Forced-choice performance for the 16.7-ms trials was much better when they were randomly intermixed with 233-ms trials than when they were presented alone (left); both performances correlated with the stopping effect (right). $*p < 0.05$; $***p < 0.001$.

trial ended 1200 ms after the offset of the masks or until response (but up to 36 s), whichever was later.

Crucially, two different types of blocks were used: intermixed and single. As Figure 1B illustrates, in the intermixed blocks, weakly masked and strongly masked trials were randomly intermixed just as in the go/no-go task; in the single blocks, however, only the strongly masked trials were included. The block order was counterbalanced across participants: Half of the participants went through intermixed–single–intermixed–single; the other half went through single–intermixed–single–intermixed. In total, there were 320 trials in four blocks (preceded by eight practice trials). The two intermixed blocks included 80 weakly masked trials (all with the highest contrast) and 80 strongly masked trials (20 trials for each of the four contrast levels); the two single blocks included 160 strongly masked trials (40 trials for each of the four contrast levels). Other aspects were the same as the go/no-go task.

Procedure of Experiment 1B

Experiment 1B was the same as Experiment 1A except for two major aspects. First, to be completely identical to van Gaal et al. (2010), the structure of the timing was changed: In the visible trials, the target duration was 233 ms, followed directly by a 16.7-ms mask; in the invisible trials, the target duration was 16.7 ms, followed by a 33.3-ms blank and then a 200-ms mask (Figure 2A). In addition, the fixation was changed to a cross, and the target was always at 47.1 cd/m^2 . Second, there were 480 go/no-go trials (in six

blocks with an additional 12 practice trials) and 480 forced-choice trials (in six blocks with an additional eight practice trials). To shorten the experimental time, each incorrect response in the go/no-go trials was followed by a 2-s (as opposed to 5-s) pause. To ensure adequate instruction for the forced-choice trials, instruction now included both written and oral explanations; additionally, a reminder regarding response mapping, “square = left click; diamond = right click,” was presented at the beginning of each block. This experiment was conducted during the beginning of the autumn quarter (Experiment 1A was conducted during the end of the spring quarter).

Data analysis

In the go/no-go trials, RTs lower than 100 ms and outside of three standard deviations in each condition were excluded (the same pattern held when these data were included).

Results and discussion: Experiment 1A

Assessing experiment participation by using catch trials

Participants’ experiment participation (such as their understanding of the task and following of the instruction) was assessed from their performance in the weakly masked (“catch”) trials. The criterion for proper experiment participation was set at 65% correct in these trials, based on two factors: (a) It should be higher than chance performance, which was 61.3% (binomial test with 80 trials, one-tailed, $p < 0.05$), and

(b) it was then adjusted based on the performance distribution (specifically to saddle a bimodal distribution). Accordingly, nine participants were excluded (three females, six males; average age = 20.2), and their accuracies ranged from 51.3% to 65.0% ($M = 58.3\%$, $SD = 4.6\%$); 16 participants (10 females, six males; average age = 19.1) were included, and their accuracies ranged from 85.0% to 100% ($M = 93.4\%$, $SD = 5.5\%$). The number of excluded participants is surprisingly high—indeed, the highest proportion of participants that we have excluded using this kind of catch-trial procedure—and several factors might contribute to it: Perhaps end-of-quarter participants were not as motivated, the experimental instructions could have been more thorough, and random sampling. In any case, the finding underscores the need for such a procedure to ensure proper experiment participation.

Single blocks undermined awareness measurement

We then tested whether target discrimination in strongly masked trials depended on whether they were presented alone or intermixed with weakly masked trials. Figure 1B shows the results. A repeated measures analysis of variance (ANOVA) on block type (intermixed vs. single) and target contrast revealed significant main effects of block type, $F(1, 15) = 41.92$, $p < 0.001$, $\eta_p^2 = 0.74$, and target contrast, $F(3, 45) = 5.01$, $p = 0.004$, $\eta_p^2 = 0.25$, without a significant interaction, $F(3, 45) = 12.29$, $p = 0.935$, $\eta_p^2 = 0.01$. These data thus indicated that discrimination performance of strongly masked trials was much better when they were intermixed with weakly masked trials than when alone, regardless of target contrast. The average elevation in performance across the target contrasts was positive for all the 16 participants with a mean of 10.5% and a range from 2.5% to 23.8%.

This effect was similar for the first two blocks (12.64%) and the last two blocks (8.34%), resulting in a main effect of block type, $F(1, 15) = 41.79$, $p < 0.001$, $\eta_p^2 = 0.74$, without an interaction with block order, $F(1, 15) = 1.32$, $p = 0.269$, $\eta_p^2 = 0.08$. On average, the elevation effect appeared to positively correlate with performance in the weakly masked trials, Pearson correlation, $r = 0.44$, $p = 0.089$; in other words, individual difference in awareness elevation was related to perceptual recognition of the weakly masked items.

Because the go/no-go task involved intermixing weakly masked and strongly masked trials, only the intermixed blocks provided an accurate estimation of visual awareness. The single blocks underestimated the true level of awareness by 10.2%, 9.2%, 12.2%, and 10.5% for the target contrasts of .07, .24, .55, and 1, respectively.

Underestimation of awareness corroded claims of unconscious processing

Unconscious processing is generally assessed from participants who are objectively unaware of the stimuli. In traditional designs, some participants would be deemed unaware when, in fact, they either just fail to perform the task or are actually aware (above-chance performance in an appropriate test). This leads to significant consequences: an inflation of unconscious processing at best and a false claim of unconscious processing at worst.

To illustrate, here we evaluated unconscious cognitive control in the go/no-go task. The logic for establishing an unconscious stopping effect is to show that, when they are objectively unaware of the target shape and thus consider both target shapes as go trials, participants are slower to respond to the target that is associated with no-go when visible than the target that is associated with go when visible. Our critical finding here was a positive correlation between the stopping effect (in the go/no-go task) and the target discrimination performance (in the awareness test): single blocks, $r(62) = 0.26$, $p = 0.042$; mixed blocks, $r(62) = 0.29$, $p = 0.022$. This finding suggests that underestimation of awareness inflates unconscious processing (see also Supplemental Online Materials: Experiment 1A for additional analyses).

It is worth pointing out, though, that we are not questioning the very existence of unconscious cognitive control. With apparently proper assessment of awareness, several studies have provided evidence for unconscious cognitive control, including unconscious inhibition from a similar go/no-go design (Lin & Murray, 2014) and priming (Wokke, van Gaal, Scholte, Ridderinkhof, & Lamme, 2011) even though controversies regarding its scope and limits remain (e.g., a recent study showed an inhibition effect using a neural index without a behavioral effect, Hepler & Albarracín, 2013).

Results and discussion: Experiment 1B

Experiment 1B replicated these findings using a procedure completely identical to van Gaal et al. (2010).

Assessing experiment participation by using catch trials

No participant was excluded based on the same criterion as in Experiment 1A (i.e., 65% correct in weakly masked trials); participants ranged from 68.3% to 100% correct ($M = 94.0\%$, $SD = 8.3\%$).

Single blocks undermined awareness measurement

As in Experiment 1A, Figure 2B shows that discrimination performance of strongly masked trials

was much better when they were intermixed with weakly masked trials than when presented alone, 61.8% versus 54.1%, $t(15) = 3.71$, $p < 0.001$, $d = 1.14$.

Underestimation of awareness corroded claims of unconscious processing

As Figure 2B shows, all participants performed above 50% in the awareness test as assessed by the intermixed blocks, suggesting that participants were likely to be aware of the targets. In addition, there was a positive correlation between the stopping effect and the target discrimination performance: single blocks, $r(14) = 0.58$, $p = 0.018$; mixed blocks, $r(14) = 0.53$, $p = 0.037$ (Figure 2B). In other words, underestimation of awareness inflates unconscious processing.

Experiment 2: Mixing strongly masked and weakly masked stimuli elevates subjective awareness

Experiment 1A and B indicated that, when measured by forced-choice discrimination, awareness of strongly masked stimuli was elevated when mixed with weakly masked stimuli within the same blocks compared with when presented alone. Objective performance, however, does not necessarily align with subjective awareness as participants could correctly guess even when they are unaware of the target. For instance, neurological patients have been reported to grasp objects without being subjectively aware of them (Goodale, Milner, Jakobson, & Carey, 1991)—a blindsight effect. Therefore, Experiment 2 tested whether mixing strongly masked and weakly masked stimuli elevates subjective rating of awareness.

Method

The method was the same as in Experiment 1B except as noted below. Sixteen participants (11 females, five males; average age = 18.5) participated. Only the awareness test was used without the go/no-go task. In each trial, participants first made a forced-choice discrimination regarding the target shape and then indicated the subjective visibility of the target using one of four options (Overgaard, Rote, Mouridsen, & Ramsøy, 2006; Zeki & Ffytche, 1998):

- 1) No experience (of the stimulus)
- 2) Brief glimpse (of the stimulus but could not recognize what it was)
- 3) Almost clear impression (of the stimulus)
- 4) Clear impression (of the stimulus).

These options were presented on the screen right after the forced-choice task was completed; participants were asked to press the up-arrow and down-arrow keys on a keyboard to select one option and press the right-arrow key to confirm the choice. After the choice was confirmed, a 800-ms blank screen appeared before the next trial started.

Results and discussion

Assessing experiment participation by using catch trials

No participant was excluded based on the same criterion as in Experiment 1A (i.e., 65% correct in the weakly masked trials); participants ranged from 95.8% to 100% correct ($M = 98.8\%$, $SD = 1.5\%$).

Single blocks undermined both objective and subjective awareness measurement

Replicating Experiment 1A and B, Figure 3 shows that discrimination performance of strongly masked trials was much better when intermixed with weakly masked trials than when presented alone, 83.1% versus 59.3%, $t(15) = 13.17$, $p < 0.001$, $d = 3.29$.

More importantly, as the pie graphs in Figure 3 indicate, there was also a large shift toward higher visibility rating of the strongly masked targets in the intermixed blocks than in the single blocks (c.f., Peremen, Hilo, & Lamy, 2013). To quantify the effect, we calculated the proportion of high-visibility rating (3 and 4) across the two conditions and found that intermixed blocks increased the proportion of high-visibility rating from 11.0% to 53.2%, $t(15) = 8.04$, $p < 0.001$, $d = 2.01$.

But did this increase in the proportion of high-visibility rating really reflect higher subjective awareness or could it be due to response bias? In other words, it was possible that participants guessed more correctly in the intermixed blocks than in the single blocks, but their subjective awareness of the target remained the same with higher rating merely reflecting a response bias. To dissociate subjective awareness from response bias, we examined metacognitive sensitivity by applying Type 2 signal-detection analysis. At the heart of this approach is examining the extent subjective rating can predict response accuracy. For example, if subjective rating does not predict response accuracy, then participants have no access to the information that supports correct responses, and hence high confidence merely reflects a response bias; otherwise, subjective rating taps into subjective awareness.

To do so, we calculated Type 2 hit rate, Type 2 false alarm rate, and Type 2 sensitivity (d') as follows (see also Kunitomo et al., 2001):

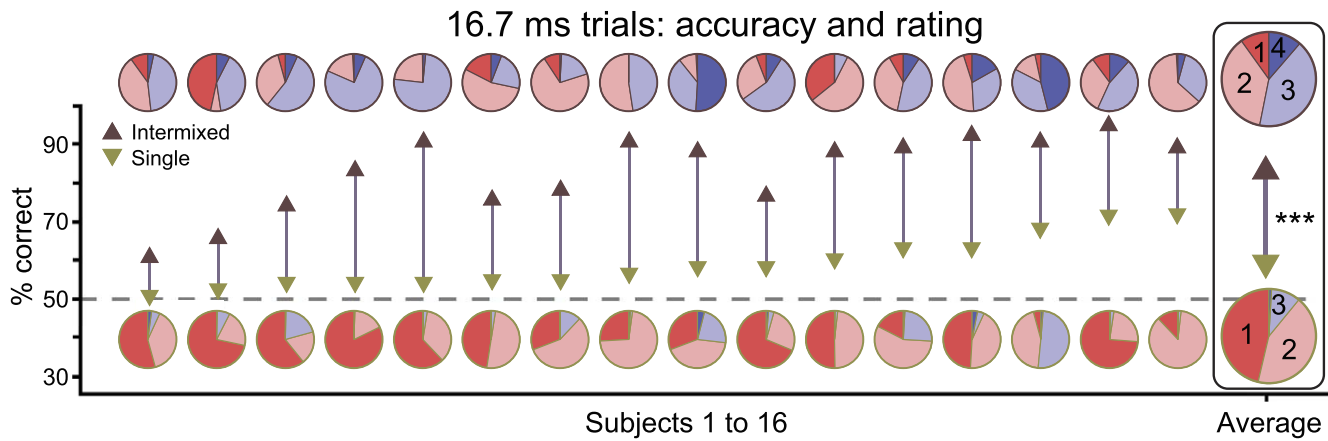


Figure 3. In each trial of Experiment 2, participants first performed a forced-choice task (target identity: square or diamond) and then underwent a subjective awareness task (target visibility according to a four-point scale: 1 = no experience, 2 = brief glimpse, 3 = almost clear impression, and 4 = clear impression). Mixing strongly masked and weakly masked stimuli not only enhanced discrimination accuracy, but also elevated subjective awareness of the strongly masked stimuli. *** $p < 0.001$.

$$\text{Type 2 hit rate} = p(\text{high rating} \mid \text{correct response}) \quad (1)$$

$$\begin{aligned} \text{Type 2 false alarm rate} \\ = p(\text{high rating} \mid \text{incorrect response}) \end{aligned} \quad (2)$$

$$\begin{aligned} \text{Type 2 sensitivity} = z(\text{type 2 hit rate}) \\ - z(\text{type 2 false alarm rate}) \end{aligned} \quad (3)$$

Figure 4 shows the results: Participants’ metacognitive sensitivity, which was free from response bias, was much higher in the intermixed blocks than in the single blocks, 1.34 versus 0.28, $t(15) = 3.83$, $p = 0.002$, $d = 0.96$.

Experiment 3: Weakly masked trials elevate awareness through template enhancement

Two conclusions emerge from Experiments 1 and 2: (a) It is necessary to ascertain participants’ proper participation in the awareness task, such as by including high-visibility trials, and (b) both objective and subjective awareness of strongly masked stimuli is elevated when they are mixed with weakly masked stimuli compared with when they are presented alone. But what causes awareness elevation?

To address this question, Experiments 3, 4, and 5 focused on the awareness elevation effect using the awareness test (without the go/no-go task as in Experiment 2). Specifically, these experiments tested a priming account in which the visibility of a strongly

masked stimulus is elevated following a weakly masked stimulus through template enhancement. Given that awareness elevation in Experiments 1 and 2 was found when the single blocks and intermixed blocks were interleaved, the elevation effect must be limited primarily to a single block containing weakly masked stimuli with little spillover across blocks. The priming account further suggested an effect at the intertrial level: Performance in a strongly masked trial would be better when preceded by a weakly masked trial of the same shape than of a different shape.

An intertrial-level effect was difficult to test in Experiments 1 and 2 for a number of reasons: (a) Only two types of trials (square and diamond) were used, (b) the order of strongly masked and weakly masked trials

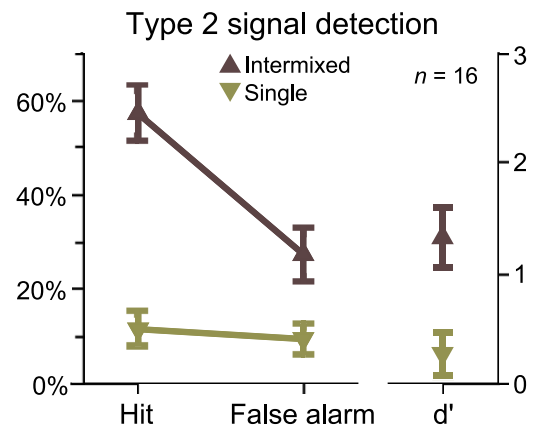


Figure 4. Metacognitive (Type 2) signal-detection analysis revealed that enhanced rating of awareness in the mixed condition compared with in the single condition was not due to response bias but reflected better metacognitive sensitivity—observer’s high ratings (3 and 4) discriminated between correct and incorrect responses. Error bars are SEM.

was randomized, and (c) (in Experiment 1A) four contrast levels were used for the strongly masked stimuli. In particular, when a stimulus could only be either a square or a diamond, unsped perceptual recognition is insensitive to intertrial shape-specific priming. For example, although a visible square in the last trial might enhance the template for square, performance benefit would be the same whether the current trial is a square or a nonsquare. This is because a primed square template would fuel a match response (i.e., square) when the target is a square and fuel a mismatch response (i.e., diamond) when the target is a nonsquare, improving performance in both cases.

Therefore, we devised a more sensitive design in Experiment 3, using four new stimuli that consisted of two shapes each and a shape-relation task (i.e., same vs. different shapes; see also Lin & Murray, 2014). Now, for example, a primed square–square template would fuel a match response (i.e., same) when the target is a pair of squares but fuel a mismatch response (i.e., not square–square, which could still be the same or different) when the target is a pair of diamonds, improving performance in the former but not the latter.

Experiment 3 also simplified the procedure in the mixed condition by interleaving each strongly masked trial with a weakly masked trial (Figure 5B). To further ensure the generalizability of the awareness-elevation effect, the experiment was conducted in a different room with a different setup and a new group of participants. As before, Experiment 3 compared performance of strongly masked trials between interleaved and single blocks.

Method

The method was the same as Experiment 1A except as noted below.

Observers and apparatus

Eighteen participants (10 females, eight males; average age = 19.8) participated in a new testing room. The stimuli were presented on a black-framed 21-in. CRT monitor (Sony G520 at 60 Hz and 1024×768 pixels; peak luminance: 106.0 cd/m^2 ; black level: 0.35 cd/m^2). Observers sat approximately 80 cm from the monitor with their heads positioned in a chin rest.

Structure of the experiment

The experiment consisted of two phases: fixation training and an awareness test. In the awareness test, as Figure 5A shows, the target consisted of two objects (luminance = 106.0 cd/m^2 ; Weber contrast = 301.9; contour-to-contour distance = 0.80°) presented for

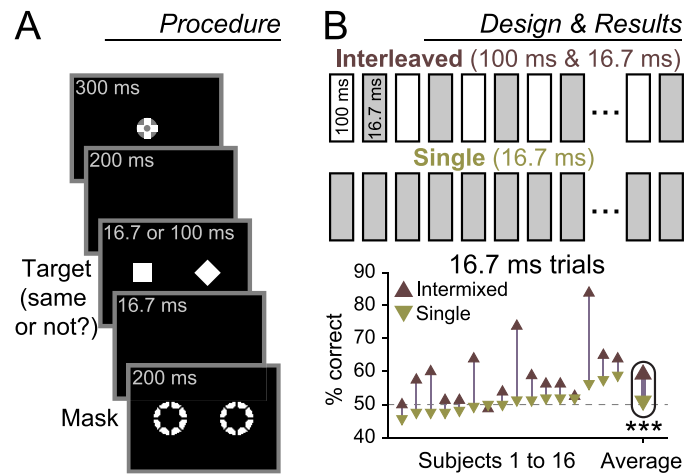


Figure 5. Experiment 3 (awareness test only). (A) Two shapes were presented and then masked by two annuli. They were presented either for 16.7 ms or for 100 ms. The task was to indicate whether the two shapes were the same (both squares or both diamonds) or different (one square and one diamond). (B) There were two different types of blocks, one in which 100-ms trials and 16.7-ms trials alternated (*interleaved*) and the other in which all were 16.7-ms trials (*single*). Forced-choice performance for the 16.7-ms targets was much better when they were interleaved with 100-ms targets than when presented alone. *** $p < 0.001$.

either 16.7 ms (invisible) or 100 ms (visible). The two objects could be both squares (size = 0.47°), both diamonds (size = 0.47°), or a square and a diamond. The task was to indicate whether the two objects were the same shape or different. The blank interval between the target and the mask was 16.7 ms.

There were 320 trials in four blocks (preceded by 16 practice trials). Crucially, two different types of blocks were used: interleaved and single. As Figure 5B illustrates, the interleaved blocks interleaved visible (100 ms) and invisible (16.7 ms) trials whereas the single blocks included only invisible trials. For half of the participants, the block order was interleaved–single–interleaved–single; for the other half, it was single–interleaved–single–interleaved.

Results and discussion

Assessing experiment participation by using catch trials

Based on the same criterion as in Experiment 1A (i.e., 65% correct in the weakly masked trials), two participants were excluded (one female, one male; average age = 22.0) as they were 53.8% and 55.0% correct; 16 participants (nine females, seven males; average age = 19.5) were included, and they ranged from 66.3% to 100% correct ($M = 86.2\%$, $SD = 12.3\%$).

Awareness elevation: Block level

From these participants, we tested whether target discrimination in strongly masked trials depended on whether they were presented alone or intermixed with weakly masked trials. Figure 5B shows the results: Discrimination performance of strongly masked trials was much better (by 8.1%) when interleaved with weakly masked trials than when presented alone, $t(15) = 4.20$, $p < 0.001$, $d = 1.05$.

This effect appeared to positively correlate with performance in the weakly masked trials, $r = 0.40$, $p = 0.120$. As in Experiment 1A, a repeated measures ANOVA on block type (interleaved vs. single) and block order (first two blocks vs. last two blocks) revealed only a significant main effect of block type, $F(1, 15) = 17.44$, $p < 0.001$, $\eta_p^2 = 0.54$, without a main effect of block order, $F(1, 15) = 1.65$, $p = 0.219$, $\eta_p^2 = 0.10$, or an interaction, $F(1, 15) = 1.73$, $p = 0.208$, $\eta_p^2 = 0.10$, indicating robust elevation of awareness in both the first two blocks (5.6%) and the last two blocks (10.6%). Additional analyses revealed a lack of learning-based, block-to-block carryover effect and found statistically similar effects for different types of trials (see Supplemental Online Materials: Experiment 3).

These results therefore demonstrate a robust awareness-elevation effect that operated at the block level, which is consistent with Experiments 1 and 2. Next, we sought to uncover a potential intertrial effect based on the priming account.

Awareness elevation: Intertrial level

We tested intertrial effects by examining the types of trial sequences that participants discriminated better than others in the interleaved blocks (but not in the single blocks, assuming that performance would be the same for all types of trial sequences in the single blocks). We took advantage of a unique feature in our design. As Figure 6 illustrates, for trial pairs of “the preceding trial → the current 16.7-ms trial,” when they were same → same, the targets in the preceding and current trials could have either the same exemplar (square–square → square–square or diamond–diamond → diamond–diamond) or different exemplars (square–square → diamond–diamond or diamond–diamond → square–square). Thus, in the interleaved blocks, if priming was conceptual or only at the block level, then one would expect no difference between same-exemplar trials and different-exemplar trials; however, if priming was at the visual, shape-specific level, then one would expect better performance in the same-exemplar trials than in the different-exemplar trials. The results showed that priming was indeed shape-specific with a 12.5% advantage for same-exemplar trials, $t(15) = 4.20$, $p = 0.010$, $d = 0.74$. No such effect was found for single

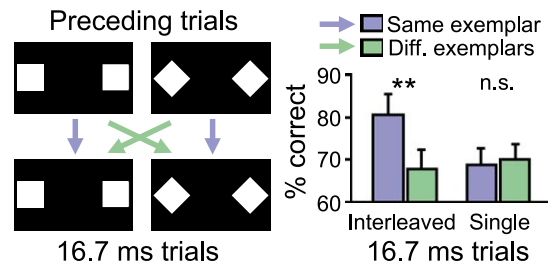


Figure 6. Awareness elevation through shape-specific template enhancement (Experiment 3). In the interleaved condition when the targets in the preceding 100-ms trial and the current 16.7-ms trial were both same-shape targets, awareness elevation was much stronger if they had the same exemplar (e.g., both consisted of two squares) than different exemplars (e.g., one consisted of two squares, the other of two diamonds). No such effect was observed in the single condition, revealing the contribution of a shape-specific template enhancement in awareness elevation. $**p < 0.01$. Error bars are SEM.

blocks, -1.6% , $t(15) = -0.55$, $p = 0.590$, $d = -0.14$, resulting in a significant interaction of block type and exemplar relation, $F(1, 15) = 8.39$, $p = 0.011$, $\eta_p^2 = 0.36$.

Experiment 4: Task difficulty is not sufficient for awareness elevation

In Experiments 1, 2, and 3, given that strongly masked trials were difficult and weakly masked trials were easy, there was an inherent difference in task difficulty between mixed blocks and single blocks, raising the possibility that awareness elevation could be due to task difficulty rather than priming. For instance, easier blocks might boost motivation (as suggested in unconscious priming, Pratte & Rouder, 2009) or free up more attentional resources, which conceivably could facilitate target mental imagery and temporal attention to the target.

To test whether task difficulty is *sufficient* for awareness elevation, Experiment 4 examined whether a similar awareness-elevation effect would appear when the contribution from priming is removed. Specifically, the target shapes in the 16.7-ms strongly masked trials were exactly the same as in Experiment 3 (i.e., square and diamond), but to eliminate shape-specific priming, the target shapes in the 200-ms weakly masked trials were drawn from a novel set of shapes (Figure 7A). Strongly masked trials could be presented alone (single blocks), interleaved with weakly masked trials (interleaved blocks), or randomly mixed with weakly masked trials (intermixed blocks). Thus, if task difficulty by itself was sufficient for awareness elevation, performance should be worse in the single blocks than in the other two conditions.

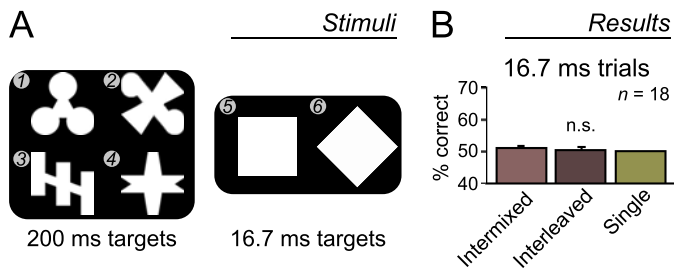


Figure 7. Experiment 4 (awareness test only). (A) The 200-ms targets and the 16.7-ms targets were two different shape sets. The task was to indicate whether the two targets in each trial were the same or different. (B) Without shape-specific priming of awareness, forced-choice performance for the 16.7-ms trials was comparable whether they were randomly intermixed with 200-ms trials, interleaved with 200-ms trials, or presented alone.

Method

The method was the same as in Experiment 3 except for two main differences. First, while the invisible 16.7-ms targets were selected from square and diamond shapes, the visible 200-ms targets were selected from four novel shapes (as opposed to the square and diamond shapes in Experiment 3). Second, there were 480 trials in six blocks with three different types of blocks: single, interleaved, and intermixed (as opposed to only the single and interleaved blocks in Experiment 3).

Counterbalancing the order of block types requires multiples of six participants (six possible orders); based on the number of participants used in Experiments 1, 2, and 3 (16 each), the number of participants in Experiment 4 was predetermined to be 18. In the course of the experiment, three participants were replaced (one participant developed a strategy of blinking his eyes during stimulus presentation to counteract masking; two participants did not perform above chance in the weakly masked trials as detailed in the following section), making the total number of participants 21 (11 females, 10 males; average age = 20.1; all were new and naïve participants except the author ZL).

Note that, because Experiment 1A and B suggested a positive correlation between awareness elevation and performance in the weakly masked trials, to maximize a potential elevation effect, the number of trials in the practice was increased to 160 trials, which included 16.7-ms, 33.3-ms, and 200-ms targets (50, 50, and 100 trials each, randomly mixed), and the target duration was increased to 200 ms (as opposed to 100 ms in Experiment 3). As before, participants were not explicitly told about the target shapes (i.e., square and diamond) but were asked to indicate whether the two shapes were the same or not.

Results and discussion

Assessing experiment participation by using catch trials

Based on the same criterion as in Experiment 1A (i.e., 65% correct in the weakly masked trials), two participants were excluded (one female, one male; average age = 19.5) as they were both 57.5% correct; 18 participants (10 females, eight males; average age = 19.9) were included, and they ranged from 78.8% to 100% correct ($M = 93.2\%$, $SD = 5.3\%$).

Task difficulty is not sufficient for awareness elevation

As Figure 7B shows, performance was comparable across the intermixed blocks (51.0%), the interleaved blocks (50.2%), and the single blocks (49.8%), $F(2, 34) = 0.63$, $p = 0.540$, $\eta_p^2 = 0.04$. Therefore, task difficulty by itself was not sufficient for awareness elevation.

In general, when the target input is too impoverished (due to low contrast, small size, etc.), it could conceivably thwart any awareness elevation. But, given that the stimuli in the strongly masked trials and the task were exactly the same as in Experiment 3, the lack of awareness elevation here could not be explained by the target visibility being too low and must be due to the change in weakly masked trials—a lack of shape-specific priming of awareness.

Experiment 5: Task difficulty and priming in awareness elevation are dissociable

Contrary to previous suggestions (Pratte & Rouder, 2009), Experiment 4 demonstrates that task difficulty is not sufficient for awareness elevation—being embedded within easy blocks does not necessarily lead to an elevation in awareness. This observation provides strong evidence supporting template enhancement as a mechanism for awareness elevation.

But, in principle, motivation and attention accounts could still coexist with the priming account, raising two intriguing questions: Might there be conditions under which task difficulty can lead to awareness elevation? If so, can awareness elevation induced by task difficulty be dissociated from awareness elevation induced by priming? We reasoned that easy blocks could enhance awareness by facilitating target mental imagery and temporal attention to the target. So, in Experiment 5, we increased the duration of the strongly masked target from 16.7 ms to 33.3 ms, and we explicitly informed the participants (with visual illustrations) that the strongly masked target could only be one of four possibilities:

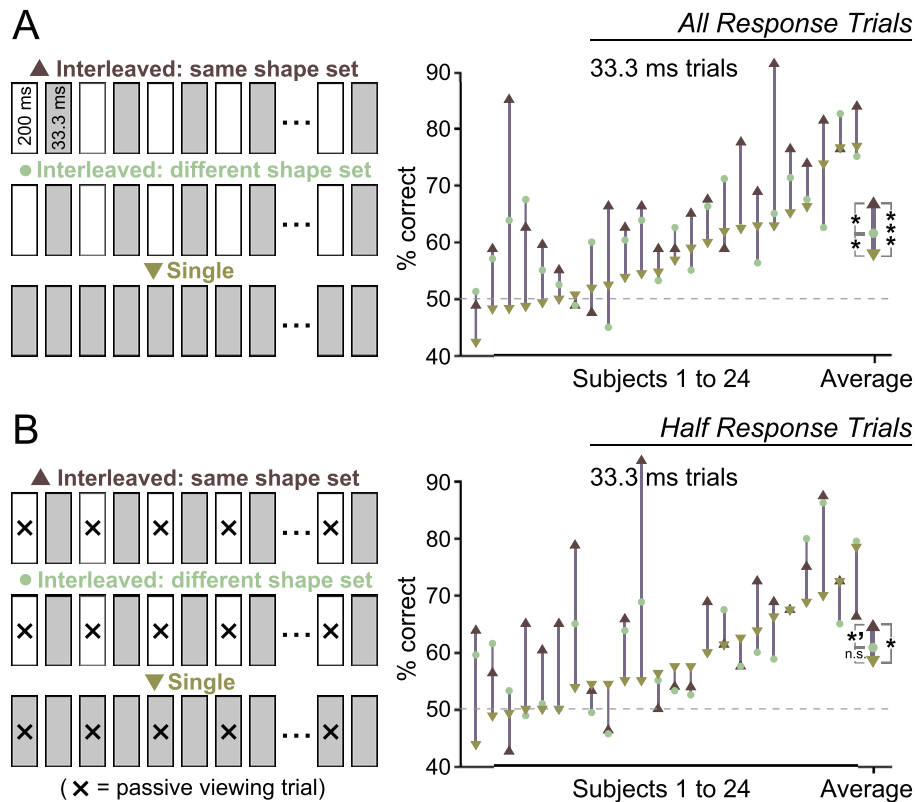


Figure 8. Experiment 5 (awareness test only). (A) In the “all were response trials” session, participants responded to all the trials as before. The strongly masked trials could be interleaved with weakly masked trials of the same shape set (with priming; *interleaved–same*), interleaved with weakly masked trials of a different shape set (without priming; *interleaved–different*), or presented alone (*single*). (B) In the “half were response trials” session, participants responded to the even, strongly masked trials only, and they passively viewed the odd trials. In both sessions, performance was significantly better in the interleaved–same blocks than in the interleaved–different blocks or in the single blocks. * $p < 0.05$; *** $p < 0.001$; *marginally significant ($p = 0.06$); n.s.: not significant.

square–square, square–diamond, diamond–diamond, or diamond–square.

The goal of Experiment 5 was threefold. The first goal was to dissociate and delineate the respective contributions from priming and task difficulty. To do so, we combined the designs of Experiments 3 and 4; the weakly masked targets in interleaved blocks could be drawn (a) from the same set of shapes (i.e., square and diamond) as the strongly masked targets (*with priming* as in Experiment 3), or (b) from a different set (*without priming* as in Experiment 4).

The second goal was to test whether task difficulty is *necessary* and whether priming is *sufficient* for awareness elevation by asking whether a similar awareness-elevation effect would manifest when the contribution from task difficulty is removed but priming is preserved. To do so, we had participants passively view the weakly masked trials without responding to them in interleaved blocks.

The final goal was to test whether awareness elevation might simply reflect a difference in anticipatory responses between single blocks and interleaved blocks—responses that do not reflect target awareness

(i.e., RTs < 100 ms). As is typical in studies using nonspeeded responses (e.g., in objective awareness tests), RTs were not recorded in Experiments 1, 2, 3, and 4, rendering it impossible to test this possibility. Indeed, although well recognized in the RT literature, this issue has often been neglected in tests of objective unawareness. But because this issue could potentially invalidate ostensible chance performance, in Experiment 5 we recorded both RTs and accuracy.

Method

The method was the same as in Experiment 3 except as noted here. First, the strongly masked target was increased from 16.7 ms to 33.3 ms and the weakly masked target from 100 ms to 200 ms. Second, as Figure 8 shows, two types of sessions were used: one in which participants responded to all trials as before (*all response trials*), the other in which participants did not respond to the odd trials and only responded to the even trials (*half response trials*). To facilitate this manipulation, trials that required a response were

marked with a green fixation; trials that did not were marked with a red fixation. Both *all response trial* and *half response trial* sessions involved single blocks as well as two types of interleaved blocks: one in which the weakly masked targets were the same as the strongly masked targets (i.e., square and diamond; referred to as *interleaved–same* blocks) and the other in which the weakly masked targets were from a different shape set (i.e., items 2 and 3 in Figure 7A; referred to as *interleaved–different* blocks).

In total, there were 480 trials (in six blocks, preceded by 16 practice trials) each for the two types of sessions. Participants were directly informed (with visual illustrations) that each strongly masked target could only be square–square, square–diamond, diamond–diamond, or diamond–square.

Counterbalancing the order of session types and block types requires multiples of 12 participants (12 possible orders); based on the number of participants used in Experiments 1, 2, and 3 (16 each), the number of participants in Experiment 5 was predetermined to be 24. In the course of the experiment, two participants were replaced (explained in the following section), making the total number of participants 26 (20 females, six males; average age = 19.5; all were new to this study).

Results and discussion

Assessing experiment participation by using catch trials and RTs

Based on the same criterion as in Experiment 1A (i.e., 65% correct in the weakly masked trials), one participant was excluded (female; age = 19.0) as she was 48.8% correct in the interleaved–same blocks. In addition, experiment participation was assessed based on RTs in the strongly masked trials to ensure that chance performance was not due to excessive anticipatory responses that could not possibly reflect target awareness (i.e., RTs < 100 ms). One participant was accordingly excluded (female; age = 22.0) as her mean RT was 296.2 ms with 33.9% of trials reflecting anticipatory responses.

This left us with 24 participants (16 females, eight males; average age = 19.9), whose accuracies in the weakly masked trials ranged from 78.8% to 100% in the interleaved–same blocks ($M = 94.1$, $SD = 5.8\%$) and from 71.3% to 100% in the interleaved–different blocks ($M = 95.0\%$, $SD = 7.4\%$) and whose RTs in the strongly masked trials ranged from 460 ms to 2047 ms ($M = 952$ ms, $SD = 349$ ms). For these participants, strongly masked trials with anticipatory responses (i.e., RTs < 100 ms) were rare ($M = 1.3\%$, $SD = 2.8\%$) and excluded in the following analyses. False alarm to trials that did not require a response in the half response trial session was rare ($M = 2.1\%$, $SD = 2.8\%$).

Dissociable effects from priming and task difficulty

A repeated measure ANOVA on session type (all response trials vs. half-response trials) and block type (interleaved–same, interleaved–different vs. single) revealed only a main effect of block type, $F(2, 46) = 9.81$, $p < 0.001$, $\eta_p^2 = 0.30$. Performance in the interleaved–same blocks (with priming) was significantly better than in the interleaved–different blocks (without priming), $F(1, 23) = 7.08$, $p = 0.014$, $\eta_p^2 = 0.24$, or single blocks, $F(1, 23) = 13.87$, $p < 0.001$, $\eta_p^2 = 0.38$; performance in the interleaved–different blocks was also better than single blocks, $F(1, 23) = 4.99$, $p = 0.036$, $\eta_p^2 = 0.18$. These differences could not be attributed to RTs (see Supplemental Online Materials: Experiment 5). These results suggest that, although task difficulty could contribute to awareness elevation, the effect from priming goes above and beyond task difficulty.

For the half response trial session, because participants responded only to strongly masked trials, task difficulty was the same across the different blocks, yet awareness elevation still occurred, suggesting that task difficulty per se is not necessary for awareness elevation and priming can be sufficient for awareness elevation (Figure 8B).

General discussion

The current study documents a novel effect of visible stimuli on visual awareness: They prime perceptual representations to boost otherwise invisible objects into awareness. This phenomenon, *priming of awareness*, has rich methodological implications for measuring awareness as well as practical and conceptual implications for models of conscious awareness.

Methodological implications: Two recommendations

Accurately characterizing unconscious processing relies on a correct measurement of awareness.¹ Chance performance in an awareness test is usually accepted as the gold standard with little subsequent interpretation. Yet we demonstrate here that due diligence is crucially required to ensure that the awareness test is a genuine assessment of awareness. Two immediate implications follow from the findings reported here.

First, verify that participants are truly performing the awareness task: This principle, “trust but verify,” is not limited to studies of unconscious cognitive control but is general for all studies in which objective unawareness is critical (for example, studies using unconscious priming and unconscious adaptation). For studies that do not involve mixing visible trials and invisible trials in

the main experiment, we recommend appending visible trials at the end of the awareness test so as to ascertain whether participants are truly participating without introducing carryover effects from these visible trials. The performance check afforded by visible trials also helps to motivate experimenters to clearly explain the task to the participants and to ensure that participants are sufficiently motivated to perform the task at hand. Similarly, it is also valuable to have RTs recorded in order to evaluate the contribution from anticipatory responses as these could not possibly reflect target awareness.

Second, use all types of trials in the awareness test as in the main experiment: This principle is violated in a collection of studies on unconscious cognitive control: In a typical study, the main experiment would involve both strongly masked and weakly masked trials, yet the awareness test would only use strongly masked trials. As the current study shows, this violation leads to an underestimation of awareness and an overestimation of unconscious processing. More generally, although such trial-to-trial carryover effects are usually underappreciated, our data provide direct evidence that these effects are serious enough that one needs to explicitly consider trial sequences (see also Reingold & Merikle, 1988).

These two simple methodological recommendations are easy to implement and should prove indispensable for establishing true unawareness and consequently the true extent of unconscious processing.

Why priming of awareness matters: Two cases in point

That visible trials might elevate the awareness of invisible trials through priming has been overlooked in the literature, leading to ostensible conclusions that are confounded by priming of awareness. Besides underestimation of awareness (and overestimation of unconscious processing) as discussed above, here we provide two specific examples to illustrate its wider implications for understanding visual awareness.

The first case concerns the role of task difficulty in visual awareness. In a typical study demonstrating unconscious processing, performance in the indirect task is above chance whereas performance in the direct task is around chance. Could chance performance invalidate the direct task itself, perhaps (as one could argue) because chance performance might reflect a reduction in motivation? In a study of subliminal priming, Pratte and Rouder (2009) compared performance in blocks that mixed short- and long-duration primes (“easy blocks”) and blocks that included only short-duration primes (“difficult blocks”). Performance of the short-duration primes was found to be better in the easy blocks than in the difficult blocks—an effect

they attributed to motivation and attention. Hence, they argued that subliminal priming was due to a task-difficulty artifact. But our results imply that the task-difficulty effect in Pratte and Rouder might be partly attributed to priming from long-duration primes. Given that the indirect test in a typical subliminal priming study uses only short-duration primes, the awareness test should not mix short- and long-duration primes; mixing short- and long-duration primes in the awareness test yields a biased measure of awareness due to priming of awareness.

The second case concerns the role of executive setting in unconscious response inhibition. Experiments of unconscious response inhibition typically mix go trial with no-go trials. Chiu and Aron (in press) asked whether no-go trials were necessary for the effect. They suggested that no-go trials were necessary as these trials induced in participants a state of preparing to inhibit responses (i.e., a response-inhibition executive setting)—a notion consistent with the modulation of unconscious processing by task sets (e.g., Kiefer & Martens, 2010; Kunde, Kiesel, & Hoffmann, 2003). To test this notion, they compared two conditions: (a) a mixed condition as before that included both go and no-go trials (i.e., mixing both strongly masked and weakly masked trials) and (b) a new condition that included only go trials (i.e., with only strongly masked trials), which they considered as not involving executive setting. The critical finding was an unconscious inhibition effect in the mixed condition but not in the go-only condition, a difference they attributed to executive setting. As our results demonstrate, however, this conclusion is unwarranted: Because the mixed condition comingled visible and invisible trials, awareness of the invisible trials was higher in the mixed condition than in the go-only condition. In other words, the “unconscious” inhibition effect in the mixed condition could well be due to conscious perception of the strongly masked stimuli. Thus, whether executive setting is necessary for unconscious response inhibition remains an open question (Lin & Murray, under review).

What determines visual awareness? An expanded model of top-down attention

Although well documented, priming is typically studied within trials and on a visible target by showing a faster response to the target when it is preceded by a congruent stimulus within the same trial (Wiggs & Martin, 1998). The current study goes further by showing a cross-trial priming effect that effectively renders an invisible stimulus (chance performance) visible (above-chance performance). Priming, in other words, fundamentally alters visual awareness, boosting otherwise invisible objects into awareness.

Priming of awareness sheds new light on a fundamental issue in consciousness, namely what determines conscious awareness. In the traditional model of conscious awareness, awareness is thought to depend on bottom-up stimulus strength and top-down attentional amplification (Dehaene et al., 2006). In this model, top-down attention draws on voluntary, goal-driven attention as derived from task demands. However, our results suggest that past experience from the preceding trials can also exert a powerful influence on current awareness, representing a new source of modulation that is not goal-relevant. Priming of awareness thus reveals a hitherto unrecognized contributor to conscious awareness.

To reconcile influences of awareness based on traditional goal-driven attention with those based on past experience, we propose a parsimonious account building on an expanded model of top-down attention. In this expanded model, the sources of top-down control are defined as extraretinal and thus include not only goals, but also expectation, working memory, knowledge, and experience (see also Baluch & Itti, 2011). Based on how intentionally an observer uses them, extraretinal factors can be organized along the continuum dimension of automaticity. For example, to the extent that goals and working memory rely heavily on one's intention, they lie at the voluntary, effortful end; to the extent that knowledge and experience can control attention without one trying to (or even despite one trying not to), they lie at the involuntary, effortless end.

This expanded model of top-down attention has two merits. First, in visual attention itself, it accommodates an emerging line of findings in which factors that are neither perceptually salient nor goal-relevant nevertheless can automatically drive the deployment of attention (Lin, under review). These otherwise inconvenient findings for the narrow dichotomic view of stimulus-driven and goal-driven attention can then be reintegrated within the broad framework of bottom-up and top-down attention (which are intuitively defined as attention based on retinal and extraretinal factors, respectively). Second, in visual awareness, this expanded top-down model suggests that awareness depends on retinal stimulus strength and extraretinal modulation (as opposed to being limited to goal-driven, voluntary amplification). This expanded model captures priming of awareness as observed here and also raises new questions regarding how awareness might be controlled by such extraretinal factors as working memory, rewards, and learning experience.

Future directions

Priming of awareness illustrates a close tie between methodological (how to correctly measure visual

awareness) and theoretical (what determines visual awareness) issues in the studies of conscious awareness. Given the methodological issues identified and their solutions, it is imperative to use these solutions to properly investigate unconscious processing (including reexamining known unconscious effects to understand their true extent). In addition, the proposed expanded model of top-down attention provides a new perspective regarding what determines awareness. The critical question remains as how various sources of active and passive top-down signals, including their interaction with stimulus strength, modulate awareness.

Keywords: visual awareness, consciousness, cognitive control, unconscious processing, priming of awareness, top-down attention

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Footnote

¹But see Schmidt & Vorberg (2006) for an alternative approach without relying on chance performance in the awareness test.

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