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You Are What I Feel: A Test of the Affective Realism Hypothesis

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We present evidence for the affective realism hypothesis, that incidental affect is a key ingredient in an individual's experience of the world. In three studies, we used an interocular suppression technique (continuous flash suppression [CFS]) to present smiling, scowling, or neutral faces suppressed from conscious visual awareness while consciously perceived neutral faces were presented at three different timing intervals: 150 ms before, 150 ms after, and concurrent with the suppressed affective faces (Studies 1 and 3) or at timing intervals of 100 ms (Study 2). Results for all three studies revealed that consciously perceived neutral faces were experienced significantly more positively (e.g., as more trustworthy) when concurrently paired with suppressed smiling faces than when concurrently paired with suppressed scowling faces; there was no effect of suppressed affective faces on first impressions in the other timing conditions. This pattern of results is consistent with the affective realism hypothesis but inconsistent with both affective misattribution and affective priming interpretations. Incidental affect must be meaningfully contiguous in time with the target stimulus to be experienced as a property of the target.

Keywords: affective realism, affective misattribution, priming, continuous flash suppression, incidental affect

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Affective feelings exert a powerful influence on behavior and decision making, even when the source of those feelings is unrelated (or incidental) to the decision at hand (Clore, Gasper, & Garvin, 2001; Johnson & Tversky, 1983; Loewenstein & Lerner, 2003), and the influence of incidental affect persists even when the eliciting source is presented outside of conscious awareness (e.g., Niedenthal, 1990; Winkielman, Berridge, & Wilbarger, 2005; Winkielman, Zajonc, & Schwarz, 1997). In fact, research suggests that incidental affect influences phenomena as diverse as food choice and consumption (Garg, Inman, & Mittal, 2005; Garg, Wansink, & Inman, 2007; Oliver, Wardle, & Gibson, 2000), detection of threat in the environment (Baumann & DeSteno, 2010; Wormwood, Lynn, Feldman Barrett, & Quigley, 2016), prosocial behavior (Bartlett, Condon, Cruz, Baumann, & Desteno, 2012; Bartlett & DeSteno, 2006; DeSteno, Bartlett, Baumann, Williams, & Dickens, 2010; Isen & Levin, 1972), risk taking (Baumann & DeSteno, 2012; Keltner & Lerner, 2010; Lerner & Tiedens, 2006), and perceived life satisfaction (Schwarz & Clore, 1983). Incidental affect has also been shown to impact decisions with significant, real-life consequences like stock market investments (Hirshleifer & Shumway, 2003) and medical school admission decisions (Redelmeier & Baxter, 2009).

In the present paper, we propose a provocative explanation for some of these findings: affective feelings (incidental or not) naturally infuse our perceptions and give us a sense of confidence that they are valid windows onto the real world. We call this the *affective realism* hypothesis (Anderson, Siegel, White, & Barrett, 2012; Barrett & Bar, 2009; Kring, Siegel, & Barrett, 2014). The affective realism hypothesis builds on the writings of philosophers who, for centuries, have argued that feelings of pleasure and distress are intrinsic elements in perceptions of the world similar to

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the experiences of hue and brightness (for discussions, see Barrett & Bliss-Moreau, 2009; Lambie & Marcel, 2002). In much the same way that color is treated as a property of an object itself instead of one's own experience of the object—people experience the sky as blue rather than experiencing sky color as their own perception of light at 500 nm reflected off particles in the sky—objects and people in the world are said to be "positive" or "negative" by virtue of their coincidence with a person's affective feelings. If the perception of a snake evokes extreme, unpleasant feelings, then the snake is said to be extremely negative. If a defendant is viewed while a judge is unpleasantly hungry, the defendant is experienced as untrustworthy and unreliable (Danziger, Levav, & Avnaim-Pesso, 2011). Recent discoveries in neuroscience reveal that the human brain is creating a unified conscious experience, integrating all sources of sensation, both from inside the body and from without, with limbic circuitry as the driver (for a discussion, see Barrett, 2017; Barrett & Simmons, 2015; Chanes & Barrett, 2016). As a consequence, a person's affect can serve as a source of realism in perception, even when incidental to the target of perception and when the person is unaware of it (Anderson et al., 2012; Kring et al., 2014; Siegel, Wormwood, Quigley, & Barrett, 2018).

The affective realism hypothesis is consistent with a large literature demonstrating that affect shapes perception (for a review, see Zadra & Clore, 2011). Individuals experiencing negative affect perceive tones as louder (Siegel & Stefanucci, 2011), are more sensitive to visual contrast gradients (Phelps, Ling, & Carrasco, 2006), and exhibit more local than global perceptual processing of images (Gasper & Clore, 2002). Moreover, affectively negative stimuli tend to be perceived as larger compared with neutral or positive stimuli (Teachman, Stefanucci, Clerkin, Cody, & Proffitt, 2008; van Ulzen, Semin, Oudejans, & Beek, 2008), and perception is influenced by individual differences in motivation and past experience (Balcetis & Dunning, 2006): people perceive a glass of water as taller when they are feeling unpleasantly thirsty (Veltkamp, Aarts, & Custers, 2008), and spider-phobic individuals perceive spiders but not wasps as larger, despite reporting that both stimuli are unpleasant (Leibovich, Cohen, & Henik, 2016).

Traditionally, researchers have understood the effect of incidental affect on perception in the context of *affect-as-information* theory (e.g., Murphy & Zajonc, 1993; Payne, Cheng, Govorun, & Stewart, 2005; Winkielman et al., 2005). Typically, affect-as-information theory is explained as affective misattribution: incidental affect is thought to exert influence when people are unaware of the source of their feelings because, without an obvious source, individuals are more likely to attribute their affective feelings as related to or caused by the current situation (Clore et al., 2001; Clore & Huntsinger, 2007; Schwarz & Clore, 1983). The affective realism hypothesis is another variant of affect-as-information theory that shares with affective misattribution the idea that affective feelings drive changes in judgment and behavior. The affective misattribution hypothesis is relatively agnostic about whether feelings influence the experience of the object itself or just the judgment process, however. By comparison, the affective realism hypothesis explicitly hypothesizes that affect is a property of consciousness and, as a result, is an integral part of the construction of all conscious experiences (Barrett & Bar, 2009; Barrett & Bliss-Moreau, 2009; Barrett & Russell, 2015). Thus, affective realism may be thought of as a special case of affect-as-information theory.

In the present studies, we sought to empirically demonstrate that affective realism is a special case of affective misattribution by manipulating the timing offset between the presentation of an affective stimulus and a to-be-judged (target) stimulus, allowing us to carefully tease apart whether incidental affect can influence the experience of the target stimulus itself and not just the perceivers' post hoc judgment of the target. We employed a paradigm used to study visual consciousness called continuous flash suppression (CFS; Tsuchiya & Koch, 2005) in which flashing images are presented to one eye (and seen) while static, low contrast images are presented to the other eye (and suppressed from awareness). In our studies, we utilized CFS to suppress affective faces from visual consciousness and asked participants to provide their first impressions of neutral target faces that remained in conscious awareness using a series of personality judgments (e.g., trustworthiness). Critically, we also directly manipulated the timing of the presentation of the seen target face across trials, presenting it either before, concurrent with, or after the suppressed affective face, while keeping the time between the suppressed affective face and the person perception judgments of the target stimulus the same across all conditions. Consistent with previous research (Anderson et al., 2012; Kring et al., 2014; Siegel et al., 2018), we predicted that neutral faces would be judged as more positive (e.g., more trustworthy) when paired with suppressed smiling faces than with suppressed scowling faces, and we explored whether this effect held across timing conditions.

This approach provides a direct test of whether affective realism is distinct from affective misattribution because the two hypotheses make different predictions about whether the relative timing of the affective and target stimuli will influence the strength of the effect. The affective misattribution hypothesis would not predict an effect of timing. The ordering of the stimuli should not alter the use of one's feelings as information for evaluating the target face, particularly when the time between the presentation of the affective information (i.e., the suppressed affective face) and the evaluation (i.e., the personality rating) is constant across all trials. If the effect of the suppressed affective faces on personality ratings is consistent across timing conditions, this would suggest that affective realism and affective misattribution are not distinguishable in the present experiments. However, evidence consistent with affective realism (i.e., that affective feelings are integrated into the experience of the target face) would be found if the suppressed affective faces influence personality ratings most strongly when affective stimuli are concurrent with neutral target stimuli. This pattern of results would provide evidence that affective realism represents a unique class of affective misattribution effects.

Studies 1 and 2

In Study 1, we utilized an offset of 150 ms: neutral target faces were presented 150 ms before, concurrent with, and 150 ms after the suppressed affective faces, which were presented for 200 ms. In Study 2, we shortened the offset to 100 ms. These offsets were chosen in light of previous literature demonstrating that visual stimuli presented for longer than 130 ms (as in the present studies) persist at multiple levels of the visual system (from activity in the peripheral retinal receptors to cortical activity) for a constant of

100 ms beyond their physical presentation (Bowling & Lovegrove, 1980, 1981; Efron, 1970). Thus, we selected offsets that would put us outside the possible visual persistence window for our face stimuli, limiting the possibility that the affective and target stimuli might be perceived or experienced as simultaneous in our offset timing conditions. Across both studies, we predicted that seen neutral faces would be evaluated more positively when paired with suppressed smiling faces than with suppressed scowling faces, and that this effect would be strongest in the concurrent timing condition.

Method

For conciseness and because methods and analyses were identical, we describe the samples, methods, and results jointly for Studies 1 and 2 rather than separately by study. All procedures and materials were approved by the Northeastern University Institutional Review Board.

Participants. Participants in Studies 1 and 2 were students and community members recruited from the greater Boston area through fliers on college campuses and advertisements on Craigslist.org and in the Boston-Metro newspaper. All participants reported normal or corrected-to-normal visual acuity and were naïve to the experimental hypotheses. Individuals wearing glasses were excluded because glasses interfere with the proper function of the visual apparatus (mirror stereoscope). Target sample size for Study 1 was determined by conducting a power analysis in G*Power (Faul, Erdfelder, Lang, & Buchner, 2007) using the average effect size from previous research in our lab that utilized a similar experimental task (Anderson et al., 2012; Study 4; Siegel et al., 2018; Studies 1 and 2). This power analysis revealed that in order for an effect size of $\eta^2 = 0.163$ for affect condition to be detected within any of the timing conditions (80% chance) with significance at the $\alpha = .05$ level, a sample of at least 28 participants would be required. Thirty-three participants (18 males, 15 females; Age 18–60; $M_{age} = 28.58$, $SD_{age} = 14.23$) completed Study 1. Target sample size for Study 2 was determined by conducting a power analysis using the effect size of the critical Affect Condition × Timing Condition interaction from Study 1. This power analysis revealed that in order for an interaction with an effect size of $\eta^2 = 0.115$ to be detected (80% chance) with significance at the $\alpha = .05$ level, a sample of at least 54 participants would be required. Sixty-one participants (18 males, 43 females; Age 18–53, $M_{age} = 23.82$, $SD_{age} = 10.41$) completed Study 2.[1] Prior to data analysis, seven participants were removed because they either did not understand or follow the experimental instructions ($n = 3$, Study 2), they experienced difficulty using the stereoscope ($n = 2$, Study 2), or there was a computer error during data collection ($n = 2$, Study 1).

Experimental tasks. Participants viewed stimuli through a mirror stereoscope, a device that uses mirrors to simultaneously present a different image to each eye while the participant rests their chin and forehead on the rests of the device. All stimuli were presented in grayscale on a 25-in. monitor (Study 1) or a 19-in. monitor (Study 2).

Contrast adjustment task. We first established eye dominance for each participant using the hole-in-the-card test (Dolman method; Dolman, 1919; Fink, 1938) since suppression of images under CFS is more easily achieved when images are presented to the nondominant eye. Participants then completed a contrast adjustment task during which the contrast level of images presented to the nondominant eye under CFS was adjusted to improve suppression on an individual basis. Each trial of the contrast adjustment task began with a 500 ms fixation cross and ended with a 500 ms backward mask. Following the fixation cross, high-contrast, Mondrian-type images (similar to Tsuchiya & Koch, 2005) were "flashed" in the participant's dominant eye at 20 Hz for 1,200 ms. During this 1,200 ms, a photograph of a house (either upside down or right-side up) was presented for 200 ms to the participant's nondominant eye, from 500–700 ms after the fixation cross. Participants reported the orientation of the suppressed house on each trial by clicking one of two keys on their keyboard. They also rated their subjective awareness of the suppressed house using the 4-point Perceptual Awareness Scale (PAS; Ramsøy & Overgaard, 2004), from *no experience* to *absolutely clear experience*. Images of houses were presented at four discrete contrast levels, created by reducing the contrast and luminance levels of the original photographs to 75, 50, 25, and 12.5%. For the first 20 trials of this task, all house images were presented at 75% contrast with half of the trials containing right-side up images and half containing upside-down images. If participants correctly guessed the orientation of the suppressed house on 70% of the trials or reported *no experience* on less than 75% of trials, the contrast level was reduced and the participant completed another 20 trials of this task at the next highest contrast level. This procedure was repeated until participants correctly guessed the orientation on 13 or fewer trials and reported *no experience* on at least 15 trials, or until the 12.5% contrast level was reached. The contrast adjustment task determined the individualized contrast level at which all suppressed images would be presented for the remainder of the experimental tasks.

Person perception task. On each trial of the person perception task, participants were presented with flashing Mondrian-type images interleaved with an image of an individual with a neutral face in their dominant eye (neutral target face) while an image of an individual with an affective facial pose was presented to their nondominant eye and was suppressed from awareness (suppressed affective face). Participants were asked to rate how likable, trustworthy, and reliable they found each seen, neutral target face on a scale from 1 to 5. All face stimuli were pulled from a normed facial stimulus set, the Interdisciplinary Affective Science Laboratory Face Set, which was developed by our laboratory with support from the National Institutes of Health Director's Pioneer Award (DP1OD003312) to LFB. Emotion expressions in this stimulus set were posed. Models were given instructions concerning the facial muscles to move for each expression, and were shown several sample photos for each expression. For example, for neutral expressions, models were told "Let your face hang in a relaxed, natural position. Look straight into the camera." The specific images utilized in the current studies were selected at random from this larger facial stimulus set. To control for potential differences that might result from certain neutral faces being perceived as more attractive or trustworthy than other neutral faces, all seen

[1] Although participants vary widely in age, research suggests age-related thinning of tissue in the visual cortices is not prominent until after age 60 (McGinnis, Brickhouse, Pascual, & Dickerson, 2011).

neutral faces were presented in the various suppressed affective conditions randomly across participants (i.e., Identity 1 was always paired with a smiling face for Participant 1, but always paired with a scowling face for Participant 2, etc.). Consistent with the facial stimuli in Anderson et al. (2012), Gruber et al. (2016), and Kring et al. (2014), we used faces with no visible teeth that were cropped to 150 (width) × 169 (height) pixels at 100 dpi.

All trials in the person perception task began with a fixation cross that was presented to both eyes for 500 ms and ended with a backward mask that was presented to both eyes for 500 ms. The stimuli presented to each eye differed for the 1,200 ms between fixation and the backward mask. Figure 1 displays visual representation of the trial structure. In the nondominant eye, a low-contrast, low-luminance face was displayed for 200 ms (from 500–700 ms after fixation). The suppressed face displayed one of three affective poses (smile, scowl, or neutral) and the contrast level of the suppressed faces was individually determined for each participant based on performance in the preceding contrast adjustment task. The presentation of the suppressed affective face was the same in all three timing conditions. In the dominant eye on each trial, a series of flashing, high-contrast Mondrian-type images were presented and were consciously seen by participants. These were interleaved with a 200-ms display of a full-contrast image of a neutral target face, which was also consciously seen. The timing of the presentation of the neutral target face in the dominant eye varied across three within-subject experimental conditions: the affect before target condition, the concurrent condition, and the affect after target condition (see Figure 1). In the concurrent condition of Studies 1 and 2, the neutral target face was displayed from 500–700 ms following fixation, concurrent with the presentation of the suppressed affective face. In the affect before target condition, the neutral target face was displayed with 150 ms between the offset of the suppressed affective face and the onset of the neutral target face in Study 1; this offset was reduced to 100 ms in Study 2. In the affect after target condition, the neutral target face was displayed with 150 ms between the offset of the neutral target face and the onset of the suppressed affective face in Study 1; this offset was reduced to 100 ms in Study 2. In all three timing conditions in both studies, Mondrian-type images were flashed at 20 Hz in the dominant eye before and after the presentation of the neutral target face.

At the conclusion of each trial, participants completed four ratings on a standard keyboard. First, they indicated the gender of the face they saw by choosing "male," "female," or "don't know." They were instructed to choose don't know if they had trouble determining the gender, saw more than one gender, or saw a blend of two genders/faces. Because the suppressed face was a different gender than the seen neutral target face, this gender question was used as a trial-by-trial measure of conscious visual awareness of

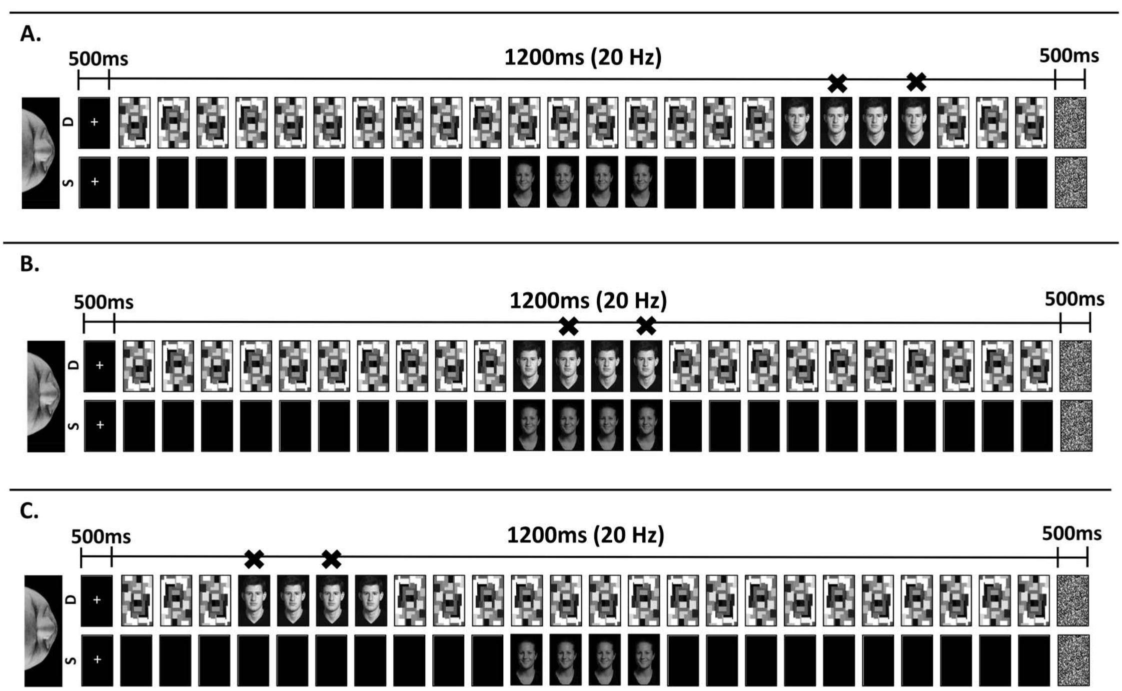

Figure 1. Diagram of the trial structure for each of the timing conditions in Study 1. Trials each began with a 500 ms fixation cross and ended with a 500 ms backward mask. All other images were presented in 50 ms bins. (A) Shows the trial structure for the affect before target condition, (B) shows the trial structure for the concurrent condition, and (C) shows the trial structure for the affect after target condition. The top row in each panel (labeled D) represents the images shown to the dominant eye, which are consciously perceived, including the neutral target face. Neutral target faces that were replaced with Mondrian images in Study 3 are marked with an X. The bottom row in each panel (labeled S) represents the images shown to the nondominant eye, including the affective faces; these images are typically suppressed from conscious awareness.

the suppressed face. All trials in which perceivers selected the gender of the suppressed face or the "don't know" option were excluded from analyses (17.9% of all trials in Study 1; 17.0% of all trials in Study 2). This trial-by-trial measure of conscious visual awareness, or a very similar one, has been used in five published experiments examining the impact of suppressed affective faces on judgments about seen neutral faces (Anderson et al., 2012; Siegel et al., 2018). This approach utilizes a fairly conservative check for visual conscious awareness because it successfully captures trials on which perceptual blending or breakthrough of the suppressed image may have occurred, but also likely captures a large number of trials on which the gender of the seen neutral face was not observed for reasons unrelated to suppression of the image in the nondominant eye (i.e., distraction, blinking, fatigue). Participants then rated how likable, trustworthy, and reliable they found each neutral target face on three 5-point scales: *unlikeable* to *likable*, *untrustworthy* to *trustworthy*, and *unreliable* to *reliable*.

Participants completed a total of 540 trials of the person perception task: 10 neutral target faces (5 male, 5 female) × 3 suppressed facial pose conditions × 3 timing conditions × 6 repetitions. While a given neutral target face was always paired with the same suppressed affective face throughout the experiment for a given participant, these pairings were counterbalanced across participants, such that approximately one third of participants saw any given neutral target face matched with each of the possible suppressed affective poses (i.e., smile, scowl, neutral). The task was broken into 6 blocks of 90 trials each, and participants were given a 2-min break to rest their eyes after each block.

Objective awareness task. The final task served as a measure of a participants' objective awareness of the suppressed images. These trials were nearly identical to the experimental trials in the concurrent condition of the person perception task except that (a) suppressed affective faces were presented upside down on half of the trials, and (b) a scrambled image of a face (designed to be unidentifiable as a face) was presented to the dominant eye (instead of a neutral target face). Participants completed 60 trials of this task: each of the 30 unique suppressed affective faces they were shown during the person perception task were presented once right-side up and once upside down (rotated 180°) at the same contrast level as used in the person perception task for each participant. At the conclusion of each trial, participants were asked to guess the orientation of the face, and then to rate the quality of their visual experience on the same 4-point PAS used during the contrast adjustment task. If images presented to the nondominant eye were successfully suppressed throughout the experiment, participants should have no conscious awareness of the faces and should report the correct orientation of the suppressed affective faces at chance level during this task. Performance on the objective awareness task is useful for describing group-level performance, demonstrating whether the majority of participants exhibit evidence of objective awareness in a task with nearly identical parameters to the main experimental task (the person perception task). We use exploratory analyses to confirm that any pattern of results does not depend on the inclusion/exclusion of individuals who showed better-than-chance performance on this separate task.

Procedure. A research assistant greeted participants and provided a verbal description of the experiment. Participants then provided informed consent followed by demographic information, including gender, race, age, and handedness. The researcher assessed the participant's eye dominance and led the participant into an individual testing room with a computer and a mirror stereoscope. The researcher calibrated each participant to the stereoscope, adjusting the mirrors and rests so that the stimuli being presented were aligned properly. Before each of the experimental tasks, the researcher read instructions and watched while the participant completed five practice trials. Participants completed each task alone in the testing room with the lights off. At the end of the experimental session, researchers administered a debriefing questionnaire assessing participants' awareness of the suppressed stimuli and the purpose of the experiment, as well as their comprehension of task instructions. They were then debriefed about the nature of the study and compensated for their participation.

Results

Each participant's three personality ratings of the neutral target face (likable, trustworthy, reliable) were averaged together to create a single personality rating measure for each trial of the person perception task. This was done to simplify the reporting and interpretation of results and because predictions did not differ for the dependent variables. We found very strong and consistent internal reliability across the three ratings within each timing condition in Study 1 (affect after target condition: $\alpha = .98$; concurrent condition: $\alpha = .98$; affect before target condition: $\alpha = .98$) and in Study 2 (affect after target condition: $\alpha = .96$; concurrent condition: $\alpha = .96$; affect before target condition: $\alpha = .95$). Separate analyses on the individual personality ratings revealed the same general pattern of results and can be found in the online supplemental materials (Tables S1–S4).

Study 1 results

Objective awareness. Two participants in Study 1 did not complete the objective awareness task because the experimental session ran over the allotted time (and participants opted not to stay to complete it). Of the participants who did complete the objective awareness task, five participants were able to correctly guess the orientation of the suppressed face on more than 61.67% of the trials (better than chance, $p < .05$, two-tailed), demonstrating that the majority of participants did not demonstrate evidence of objective awareness of the suppressed affective faces. These participants were included in all analyses, but the general pattern of results for Study 1 did not change whether we included (or excluded) these participants.

Conscious visual awareness. All trials of the person perception task in which perceivers selected the gender of the suppressed face or the "don't know" option were excluded from analyses (17.9% of all trials). To examine whether conscious visual awareness varied across affect and timing conditions, we conducted a 3 (affect condition: suppressed smile, suppressed neutral, suppressed scowl) × 3 (timing condition: affect after target, concurrent, affect before target) repeated-measures analysis of variance (ANOVA) with number of included trials as the dependent variable. This analysis revealed that the number of included/excluded trials did not differ significantly across affect conditions, $F(2, 60) = 0.50$, $p = .61$, $\eta_p^2 = .016$, and there was no significant interactions between timing and affect condition on the number of included trials, $F(4, 120) = 0.93$, $p = .45$, $\eta_p^2 = .030$. There was, however, a significant difference in the number of included trials across timing conditions, $F(2, 60) = 5.19$, $p = .008$,

$\eta_p^2 = .147$. A post hoc Fischer's least significant difference (LSD) test revealed that there were significantly more included trials, on average, within the affect before target conditions ($M = 50.24$, $SE = 2.44$) compared with both the affect after target conditions ($M = 45.80$, $SE = 2.70$), $p = .03$, and the concurrent conditions ($M = 44.95$, $SE = 2.72$), $p = .03$, which did not differ, $p = .16$.

Person perception task. A 3 (affect condition: suppressed smile, suppressed neutral, suppressed scowl) × 3 (timing condition: affect after target, concurrent, affect before target) repeated-measures ANOVA with personality ratings as the dependent variable revealed that the critical interaction between affect condition and timing condition was significant, $F(4, 120) = 3.91$, $p = .005$, $\eta_p^2 = .115$ (Figure 2). To examine this interaction, we conducted separate repeated-measures ANOVAs assessing the impact of affect condition on personality ratings within each of the timing conditions separately. These analyses revealed a significant effect of affect condition on personality ratings in the concurrent condition, $F(2, 60) = 5.49$, $p = .006$, $\eta_p^2 = .155$, but no significant effect in the affect after target condition, $F(2, 60) = 2.16$, $p = .12$, $\eta_p^2 = .067$, or in the affect before target condition, $F(2, 60) = 0.14$, $p = .87$, $\eta_p^2 = .005$. As predicted, a post hoc Fischer's LSD test revealed that, within the concurrent condition, seen neutral faces were rated significantly more positively when paired with suppressed smiling faces ($M = 3.08$, $SE = .12$) than with suppressed neutral faces ($M = 2.97$, $SE = .12$), $p = .009$, or with suppressed scowling faces ($M = 2.89$, $SE = .12$), $p = .009$. Differences in personality ratings of neutral faces paired with suppressed scowling faces and suppressed neutral faces were in the predicted direction, but did not reach conventional levels of significance, $p = .20$.

Our omnibus repeated-measures ANOVA also revealed a significant main effect for affect condition on personality ratings, $F(2, 60) = 3.61$, $p = .03$, $\eta_p^2 = .107$. As predicted, a post hoc Fischer's LSD test revealed that, across all timing conditions, seen neutral faces were rated significantly more positively when paired with suppressed smiling faces ($M = 2.98$, $SE = .11$) than with suppressed scowling faces ($M = 2.89$, $SE = .12$), $p = .02$. Also as predicted, personality ratings of seen neutral faces paired with suppressed neutral faces ($M = 2.93$, $SE = .12$) were intermediate; however, comparisons to personality ratings of seen neutral faces paired with suppressed smiling ($p = .10$) or suppressed scowling faces ($p = .24$) did not reach conventional levels of significance.

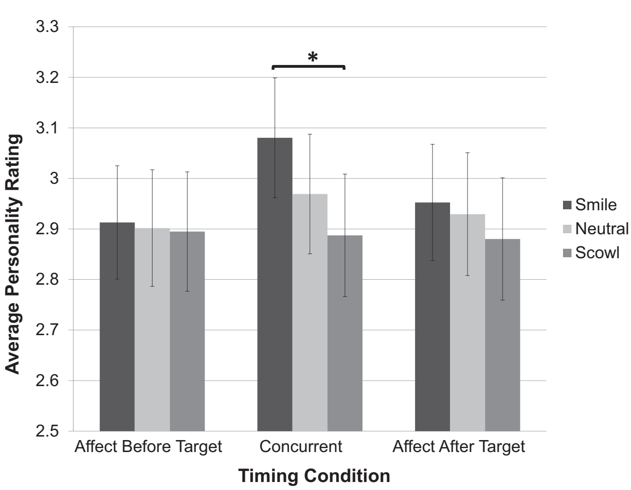

Figure 2. Average personality rating by affect and timing conditions for Study 1. Error bars represent ±*SE* (between-subjects). $^*p < .05$.

The omnibus repeated-measures ANOVA also revealed a main effect for timing condition on personality ratings, $F(2, 60) = 5.50$, $p = .006$, $\eta_p^2 = .155$. A post hoc Fischer's LSD test revealed that, across all affect conditions, seen neutral faces were rated more positively when shown concurrently with the suppressed face ($M = 2.98$, $SE = .12$) than when shown before the suppressed face ($M = 2.92$, $SE = .12$), $p = .03$, or after the suppressed face ($M = 2.90$, $SE = .11$), $p = .01$. Personality ratings did not differ significantly between the affect after target and the affect before target conditions, $p = .31$.

Study 2 results

Objective awareness task. Twelve participants in Study 2 did not complete the objective awareness task because the experimental session ran over the allotted time (and participants opted not to stay to complete it). Of the participants who did complete the objective awareness task, nine participants from Study 2 were able to correctly guess the orientation of the suppressed face on more than 61.67% of the trials (better than chance, $p < .05$, two-tailed), demonstrating that the majority of participants did not demonstrate evidence of objective awareness of the suppressed affective faces. These participants were included in all analyses, but the general pattern of results for Study 2 did not change whether we included (or excluded) these participants.

Conscious visual awareness. All trials of the person perception task in which perceivers selected the gender of the suppressed face or the "don't know" option were excluded from analyses (17.0% of all trials). To examine whether conscious visual awareness varied across affect and timing conditions, we conducted a 3 (affect condition: suppressed smile, suppressed neutral, suppressed scowl) × 3 (timing condition: affect after target, concurrent, affect before target) repeated-measures ANOVA with number of included trials as the dependent variable. This analysis revealed that the number of included/excluded trials did not differ significantly across timing conditions, $F(2, 110) = 2.20$, $p = .12$, $\eta_p^2 = .038$, or across affect conditions, $F(2, 110) = 0.40$, $p = .67$, $\eta_p^2 = .007$, and there was no significant interaction between timing and affect condition on the number of included trials, $F(4, 220) = 1.14$, $p = .34$, $\eta_p^2 = .020$.

Person perception task. In Study 2, the influence of the suppressed affective faces on personality ratings once again differed significantly across timing conditions, $F(4, 220) = 4.17$, $p = .003$; $\eta_p^2 = .070$ (Figure 3). As in Study 1, the effect of affect condition on personality ratings was only significant within the concurrent condition, $F(2, 110) = 7.42$, $p = .001$, $\eta_p^2 = .119$; we found no significant effect in the affect after target condition, $F(2, 110) = 1.48$, $p = .23$, $\eta_p^2 = .026$, or the affect before target condition, $F(2, 110) = 2.21$, $p = .11$, $\eta_p^2 = .039$. Within the concurrent condition, as predicted, a post hoc Fischer's LSD test revealed that seen neutral faces were rated significantly less positively when paired with suppressed scowling faces ($M = 2.90$, $SE = .06$) than with suppressed smiling faces ($M = 3.02$, $SE = .06$), $p = .002$, or with suppressed neutral faces ($M = 3.00$, $SE = .06$), $p < .001$. Differences in the personality ratings of neutral faces paired with suppressed smiling faces and suppressed neutral faces were in the predicted direction, but did not reach significance, $p = .74$.

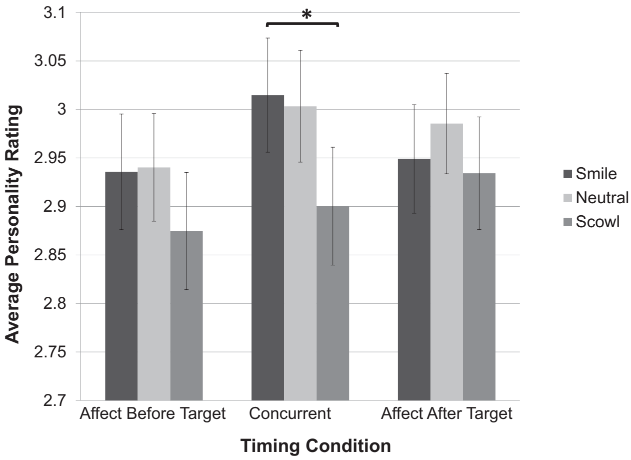

Figure 3. Average personality rating by affect and timing conditions for Study 2. Error bars represent $\pm SE$ (between-subjects). $^{*}p < .05$.

In addition, the omnibus repeated-measures ANOVA again revealed a significant main effect of affect condition on personality ratings, $F(2, 110) = 3.62$, $p = .03$, $\eta_p^2 = .062$. As predicted, a post hoc Fischer's LSD test revealed that, across all timing conditions, seen neutral faces were rated significantly less positively when paired with suppressed scowling faces ($M = 2.90$, $SE = .06$) than with suppressed smiling faces ($M = 2.97$, $SE = .06$), $p = .01$, or suppressed neutral faces ($M = 2.98$, $SE = .05$), $p = .05$. Differences in the personality ratings of neutral faces paired with suppressed smiling faces and suppressed neutral faces were in the predicted direction, but did not reach significance, $p = .73$.

Finally, the omnibus repeated-measures ANOVA also revealed a main effect for timing condition on personality ratings, $F(2, 110) = 11.27$, $p < .001$, $\eta_p^2 = .170$. A post hoc Fischer's LSD test revealed that, for all affect conditions, seen neutral faces were rated less positively in the affect before target condition ($M = 2.92$, $SE = .05$) than the concurrent condition ($M = 2.97$, $SE = .06$), $p < .001$, or the affect after target condition ($M = 2.96$, $SE = .06$), $p = .003$. Personality ratings did not differ significantly between the concurrent and affect after target conditions, $p = .17$.

Discussion

Across two studies, we see evidence that seen neutral target faces are evaluated more positively when paired with suppressed positive affective information than when paired with suppressed negative affective information. Critically, this effect is only significant when the suppressed affective information is presented concurrent with the seen neutral target face and not when the neutral target face is presented before or after the affective information. This pattern of results demonstrates that affect can be integrated into the experience of the seen neutral target face in real time and not just influence evaluations of it post hoc.

A major strength of these studies is the inclusion of the trial-by-trial measure of conscious visual awareness, which allows us to directly assess the possibility of interocular fusion on all trials across all conditions. Specifically, the seen neutral face and the suppressed affective face were always of opposite gender, and we asked participants to report the gender of the face they saw or to select "don't know" if they saw a blend of faces/genders or were unsure of the gender of the face they saw for any reason. We then excluded from all analyses any trials on which they reported seeing the gender of the suppressed face or a perceptual blend of faces (i.e., trials on which they selected "don't know"). Importantly, across both studies, we saw no evidence of higher subjective awareness in the concurrent condition relative to the other two timing conditions. Thus, it seems highly unlikely that differences in conscious visual awareness or the amount of perceptual blending across conditions could account for any of the reported findings. Nevertheless, in Studies 1 and 2, there were no flashing images in the dominant eye during the presentation of the suppressed affective stimulus within the concurrent timing condition. We designed Study 3 to explicitly address this concern and rule out alternative explanations for our findings based on differences in awareness across timing conditions.

Study 3

In Study 3, we adjusted the task structure such that dynamically changing, high contrast images were always presented to the dominant eye while low contrast, unchanging images were presented to the nondominant eye across all trials. Specifically, the neutral target face presented to the dominant eye was flashed continuously across all conditions (vs. being presented statically for 200 ms in Studies 1 and 2; See Figure 1). Critically, this allowed us to rule out potential alternative explanations concerning whether interocular suppression was being achieved via different mechanisms for trials within the concurrent condition relative to the affect before target and affect after target conditions in Studies 1 and 2 (e.g., binocular rivalry vs. CFS; Tsuchiya, Koch, Gilroy, & Blake, 2006).

Method

All procedures and materials were approved by the Northeastern University Institutional Review Board.

Participants. Target sample size for Study 3 was determined by conducting a power analysis in G*Power (Faul et al., 2007) using the effect size of the critical Affect Condition × Timing Condition interaction from Study 2. This power analysis revealed that in order for an interaction with an effect size of $\eta^2 = 0.070$ to be detected (80% chance) with significance at the $\alpha = .05$ level, a sample of at least 87 participants would be required. We recruited 100 undergraduate students from Northeastern University (40 males, 60 females; Age 18–40, $M_{age} = 19.43$, $SD_{age} = 2.54$) who participated for course credit. All participants reported normal or corrected-to-normal visual acuity and were naïve to the experimental hypotheses. Individuals wearing glasses were excluded because glasses interfere with the proper function of the visual apparatus (mirror stereoscope). Prior to data analysis, two participants were removed because they experienced difficulty using the stereoscope. One additional subject was removed during analyses for Study 3 because he or she had so few valid trials (13/540) that not all necessary cells for the analyses had valid responses.

Experimental tasks. The procedure and experimental tasks for Study 3 were nearly identical to that of Study 1, with the exception that, within the primary task (the person perception task), the neutral target face presented to the dominant eye flashed instead of being presented for a steady 200 ms on all trials (i.e., it was presented twice for 50 ms separated by a Mondrian-type image for 50 ms; See Figure 1).

Results

As in Studies 1 and 2, each participant's three personality ratings of the neutral target face (likable, trustworthy, reliable) were averaged together to create a single personality rating measure for each trial of the person perception task. We again found very strong and consistent internal reliability across the three ratings within each timing condition in Study 3 (affect after target condition: $\alpha = .96$; concurrent condition: $\alpha = .95$; affect before target condition: $\alpha = .95$). Separate analyses on the individual personality ratings revealed the same general pattern of results and can be found in the online supplemental materials (Tables S5 and S6).

Objective awareness task. One participant in Study 3 did not complete the objective awareness task because the experimental session ran over the allotted time (and the participant opted not to stay to complete it). Of the participants who did complete the objective awareness task, 13 participants were able to correctly guess the orientation of the suppressed face on more than 61.67% of the trials (better than chance, $p < .05$, two-tailed), demonstrating that the majority of participants did not demonstrate evidence of objective awareness of the suppressed affective faces. These participants were included in all analyses, but the general pattern of results for Study 3 did not change whether we included (or excluded) these participants.

Conscious visual awareness. All trials of the person perception task in which perceivers selected the gender of the suppressed face or the "don't know" option were excluded from analyses (31.7% of trials).[2] To examine whether conscious visual awareness varied across affect and timing conditions, we conducted a 3 (affect condition: suppressed smile, suppressed neutral, suppressed scowl) × 3 (timing condition: affect after target, concurrent, affect before target) repeated-measures ANOVA with number of included trials as the dependent variable. This analysis revealed that the number of included/excluded trials did not differ significantly across affect conditions, $F(2, 192) = 2.37$, $p = .10$, $\eta_p^2 = .024$, and there was no significant interaction between timing and affect condition on the number of included trials, $F(4, 384) = 0.54$, $p = .71$, $\eta_p^2 = .006$. There was, however, a significant difference in the number of included trials across timing conditions, $F(2, 192) = 19.60$, $p < .001$, $\eta_p^2 = .170$. A post hoc Fischer's LSD test revealed that there were significantly more included trials, on average, within the affect before target conditions ($M = 43.35$, $SE = 1.60$) compared with both the affect after target conditions ($M = 39.94$, $SE = 1.71$), $p < .001$, and the concurrent conditions ($M = 39.67$, $SE = 1.73$), $p < .001$, which did not differ, $p = .61$.

Person perception task. In Study 3, the influence of the suppressed affective faces on personality ratings once again differed significantly across timing conditions, $F(4, 384) = 3.87$, $p = .004$; $\eta_p^2 = .039$ (see Figure 4). As in Studies 1 and 2, the effect of affect condition on personality ratings was only significant within the concurrent condition, $F(2, 192) = 8.09$, $p < .001$, $\eta_p^2 = .078$; we found no significant effect in the affect after target condition, $F(2, 192) = 1.43$, $p = .24$, $\eta_p^2 = .015$, or the affect before target condition, $F(2, 192) = 2.41$, $p = .09$, $\eta_p^2 = .025$. Within the concurrent condition, as predicted, a post hoc Fischer's LSD test revealed that seen neutral faces were rated significantly less positively when paired with suppressed scowling faces ($M = 2.81$, $SE = .05$) than with suppressed smiling faces ($M = 2.97$, $SE = .05$), $p = .001$, or with suppressed neutral faces ($M = 2.92$, $SE = .05$), $p = .007$. Differences in the personality ratings of neutral faces paired with suppressed smiling faces and suppressed neutral faces were in the predicted direction, but did not reach significance, $p = .21$.

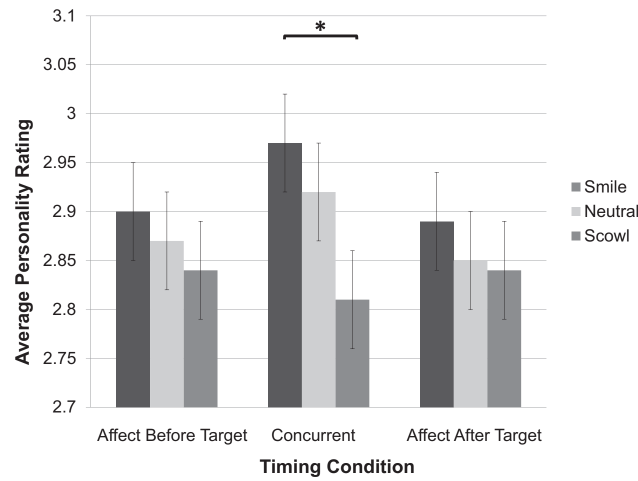

Figure 4. Average personality rating by affect and timing conditions for Study 3. Error bars represent $\pm SE$ (between-subjects). $^{*}p < .05$.

In addition, the omnibus repeated-measures ANOVA again revealed a significant main effect of affect condition on personality ratings, $F(2, 192) = 5.51$, $p = .005$, $\eta_p^2 = .054$. As predicted, a post hoc Fischer's LSD test revealed that, across all timing conditions, seen neutral faces were rated significantly less positively when paired with suppressed scowling faces ($M = 2.83$, $SE = .05$) than with suppressed smiling faces ($M = 2.92$, $SE = .05$), $p = .009$, or suppressed neutral faces ($M = 2.88$, $SE = .05$), $p = .04$. Differences in the personality ratings of neutral faces paired with suppressed smiling faces and suppressed neutral faces were in the predicted direction, but did not reach significance, $p = .09$.

Finally, the omnibus repeated-measures ANOVA also revealed a main effect for timing condition on personality ratings, $F(2, 192) = 3.57$, $p = .03$, $\eta_p^2 = .036$. A post hoc Fischer's LSD test revealed that, for all affect conditions, seen neutral faces were rated significantly more positively in the concurrent condition ($M = 2.90$, $SE = .04$) than the affect after target condition ($M = 2.86$, $SE = .05$), $p = .02$. Personality ratings within the affect before target condition ($M = 2.87$, $SE = .04$) did not differ significantly from those in either the concurrent condition, $p = .07$, or the affect after target condition, $p = .36$.

[2] We believe the larger rate of trial-by-trial exclusions in Study 3 is due to the more rapid, flashing presentation of the neutral target face across all trial types. Participants may have selected the "don't know" option much more frequently because they were not able to identify the gender of the rapidly presented neutral target face.

Discussion

Replicating Studies 1 and 2, we found that seen neutral faces were evaluated more positively when presented concurrent with suppressed affectively positive stimuli than when presented concurrent with suppressed affectively negative stimuli and that evaluations of neutral target faces did not differ on trials where the suppressed affective faces were not presented concurrent with the neutral target faces. Moreover, we again saw no evidence of greater subjective awareness of the suppressed affective stimuli within the concurrent timing conditions compared with the offset timing conditions. Thus, even with the neutral target faces flashing dynamically on all trials, we find consistent support for the affective realism hypothesis: affect can influence the experience of a neutral target stimulus in real time, not just the evaluation of that target stimulus post hoc.

General Discussion

Across all three studies, the patterns of results provide consistent evidence that affective realism can be distinguished from more general affective misattribution. Whereas the affective misattribution hypothesis would predict that the effect of the suppressed affective faces on personality ratings of seen neutral faces would be present across all timing conditions, results instead reveal that the effect was only significant when affective information was presented *at the same time* as the target stimulus. That is, neutral faces were experienced more positively (i.e., as more trustworthy, likable, and reliable) when presented at the same time as suppressed smiling faces, and were experienced less positively when presented at the same time as suppressed scowling faces. These findings suggest that affective feelings are being integrated into the experience of the target face. Thus, our findings support the philosophical conjecture that affect is a property of conscious experience. There are, in fact, many examples where affect is perceived as a property of objects and people in the world, similar to the way we typically perceive color as a property of objects in the world (e.g., Danziger et al., 2011; Leibovich et al., 2016; Veltkamp et al., 2008). In the studies we report here, participants did not appear to use negative/positive feelings to guide post hoc judgments of another person; they experienced the person as more negative/positive.

Furthermore, our findings demonstrate that affective realism and affective misattribution are empirically distinguishable. To be clear, not all instances of affective misattribution are necessarily affective realism. Our studies show, however, that some subset of instances previously understood as affective misattribution may, in fact, be better explained by this more specific causal relationship. Interestingly, our results also cannot be explained as another common phenomenon, affective priming, which would occur when the unseen affective stimuli were presented *prior* to the seen target stimuli. The timing offsets used in these studies (100–150 ms) are well within the normal range of timings used in typical priming manipulations. For example, it is common to have a subliminal affective prime precede a target stimulus by 125 ms (e.g., Gawronski & Ye, 2014; Payne et al., 2005) and subliminal primes have been shown to influence judgments of target stimuli even when they precede it by as much as 1,500 ms (Payne et al., 2005). If the present effects were merely an example of subliminal priming, we should have seen a significant effect of the suppressed affective faces on personality ratings of seen neutral faces within both the affect before target and concurrent conditions. Instead, results are more consistent with the affective realism hypothesis.

The present findings are consistent with recent empirical work demonstrating that one's affective state may influence how positive or negative a neutral target face *looks* to the perceiver in a very literal way (Siegel et al., 2018): neutral faces were perceived as looking more smiling when presented concurrent with suppressed affectively positive stimuli and as looking more scowling when presented concurrent with suppressed affectively negative stimuli. Thus, affective realism may involve changes in actual perception. What is unclear is whether the mechanism is a type of sensory integration (similar to multimodal processing) or some other process. We believe our data lend support to the idea that affect, at least in the case of affective realism effects, may be acting like a "sixth sense" that is integrated with other sensory processing (e.g., vision or hearing), similar to the multimodal processing that has been repeatedly demonstrated among the "traditional" senses. For example, visual stimuli can shape sound perception (e.g., the McGurk effect; McGurk & MacDonald, 1976). Moreover, past research has shown that sensory events are more likely to be perceived as coming from the same source when they occur in close temporal proximity, irrespective of physical location (Calvert, Brammer, & Iversen, 1998; Spence, 2007). In a similar way, affective feelings may bind with the visual properties of a neutral face, causing the person to literally be seen as more or less trustworthy, reliable, and likeable when the feelings and the face occur in close temporal proximity.

Although our results demonstrate that affective realism is sensitive to very small time differences between the presentation of a target stimulus and incidental affective information (i.e., 100 ms), they do not show that this close temporal coupling is necessary to produce affective realism. That is, we must be careful about drawing conclusions concerning the null results in our offset timing conditions (when the affective face was presented before the target or after the target). It is not clear whether nonsignificant differences in these timing conditions reflect a true null effect or simply a weaker effect of the affective information that the current studies are not sufficiently powered to observe. Significant but weaker effects in the nonconcurrent timing conditions could provide evidence that affective realism weakens as a function of timing offset, or it could suggest that, within a single task, affect may influence person perception via multiple separable mechanisms. Nevertheless, the significant interaction demonstrates support for the affective realism hypothesis by showing that the relative timing is important for this particular social judgment effect; a tight temporal coupling ($<$100 ms) between target and affective stimuli significantly *enhances* the influence of affect on person perception, suggesting that affect can indeed influence the experience of target stimuli in real-time. However, it would be interesting to directly explore the nuances of these offset timing conditions in future work. For example, a future study could employ a greater number of timing conditions where the timing off-sets are manipulated in smaller increments (e.g., 5 ms). Such a design would allow us to plot the strength of the effect as a continuous function of the timing off-set in both the affect before and affect after target conditions. Results would reveal, not only the boundary conditions of affective realism, but to some extent would also inductively reveal the timing boundaries of a single conscious event (as opposed to two distinct events).

Our findings highlight several interesting avenues for future research. The suppressed facial configurations utilized in the present study (i.e., smiling, scowling) have been shown in prior work to influence judgments of the personality traits we assessed (see, e.g., Campellone & Kring, 2013, on trust-related evaluations). Future work could examine whether affective realism is robust to less directly relevant affective stimuli (e.g., when using suppressed faces with different positive and negative facial configurations (e.g., fear, pride) or suppressed affective stimuli that are not faces). In addition, future research could develop and test suppression paradigms to allow for the simultaneous presentation of a consciously perceived visual stimulus (e.g., a neutral target face) and a suppressed affective stimulus to spatially distinct portions of the visual field. Although we excluded trials in the present analyses where participants reported consciously perceiving the suppressed affective face or a blending of the neutral target face and suppressed affective face, a paradigm that involved separating the images spatially could more completely rule out any possibility that interocular fusion contributed to the strength of the present findings.

Conclusion

Reality is not a direct perception of what is out there in the world (for discussions, see Barrett, 2017; Barrett & Bliss-Moreau, 2009; Lambie & Marcel, 2002). As perceivers, people's experience of the world is meaningfully shaped by the affect they feel in real time. The fact that we perceive the world differently, depending on how we feel, has wide-reaching implications for real-world situations of great import. For example, the affective realism hypothesis sheds new light on cases of military and police violence in which intense emotions appear to actively shape the way military and law enforcement personnel perceive threats around them, and may lead to deadly errors in judgment when experience is amplified or otherwise distorted by affect (for a discussion, see Barrett & Wormwood, 2015). Research on affective realism stands to fundamentally alter the way in which scientists understand perception and its impact on decision making, with important implications for how we train and evaluate individuals who must act rapidly under pressure in dangerous situations.

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