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Disentangling affect from self-esteem using subliminal conditioning

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ABSTRACT

Across three experiments, participants underwent conditioning sequences where the self-referential term *I AM* (Conditioned Stimulus, or CS+) or a scrambled counterpart *M IA* (CS-) was paired with either neutral (Unconditioned Stimulus, or US-) or positive attributes (US+). CS and US were presented under subliminal and/or visible conditions. A normalized indicator of affective shift and an explicit self-esteem measure were deployed as outcome measures. In Experiment 1 ($N = 60$), subliminal CS+ followed by visible US+ produced a significant affective shift only. Experiment 2 ($N = 59$) presented CS and US under subliminal conditions, which did not influence either outcome measure. In Experiment 3 ($N = 60$), visible CS appeared with visible US, which resulted in a significant effect on explicit self-esteem only. These findings highlight the central roles of CS and/or US visibility towards influencing reported affect and self-esteem. We theorize that configural components of subliminally presented stimuli can become perceptually encoded and influence self-related affect non-consciously.

1. Introduction

Learning theorists have long attempted to relate basic behavioral processes derived from laboratory observations to account for complex human behavior. One such attempt was advanced by Mowrer (1954), who suggested that naturally-occurring propositions relating a subject to a salient predicate can be effectively conceptualized as Pavlovian 'conditioning devices'. Using the proposition *Tom is a thief*, Mowrer noted that the predicate *thief* connotated a negative valence that could be likened to an affective, response-eliciting 'Unconditional Stimulus', or US. The subject *Tom*, being related to the negatively valenced *thief* by the contextual specifiers *is-a*, becomes 'conditioned' to elicit a negative evaluative response. The example by Mowrer functioned to illustrate how affective symbols may 'condition' their predicated subjects in selected contexts, similar to how US 'conditions' a formerly neutral stimulus into eliciting predictable responses as a 'Conditional Stimulus', or CS (Domjan and Delamater, 2023). The assumption that symbolic properties can be altered through Pavlovian processes has been largely retained, with qualifications, across contemporary investigations in evaluative conditioning, which describes a process where the 'liking' of a stimulus changes after being repeatedly paired with a valenced stimulus (Gawronski and Bodenhausen, 2018).

One qualification overlooked by Mowrer was the pervasive influence of prior beliefs on generated evaluations (Corneille and Lush, 2022). Take the self-evaluative proposition *I AM PLEASANT* for example. According to Mowrer (1960)'s 'conditioning device' heuristic, relating the self-referential *I* with positive attributes should enhance an individual's subjective evaluation of their self-worth, otherwise known as their 'self-esteem' (Niveau et al., 2021). In practice, consciously appraising an *I AM PLEASANT* proposition may not necessarily produce experiences of 'feeling pleasant' (Grumm et al., 2009; Niveau et al., 2021).

To see how, let us consider a subliminal conditioning study reported by Amd and Baillet (2019). In that study, participants who viewed consciously imperceptible ('subliminal') eating-related words with positive or neutral attributes reported no changes in consciously experienced hunger afterwards. Yet, the 'active' group who had viewed eating-related words with positive attributes generated significantly more salivation and conditioned affective responses relative to the 'control' group, who had viewed eating-related words with neutral attributes. Those findings led Amd and Baillet (2019) to speculate that the null effects across verbal reports may have been produced by the automatic elicitation of contra-valenced propositions following conscious identification of the evaluation target.

Applied to self-esteem, if some hypothetical participant holds a negative belief in relation to one's self (e.g., *I am an unpleasant person*), this might be 'automatically' elicited if said participant is required to consciously appraise the relational proposition *I AM PLEASANT*. In other words, prior self-related beliefs might mitigate the impact of external

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relational information designed to enhance an individual's 'self-esteem' (Niveau et al., 2021).

Recognizing these difficulties, various researchers (e.g., Baccus et al., 2004; Dijksterhuis, 2004; Grumm et al., 2009) have opted to use 'subliminal conditioning' procedures to enhance (implicit) self-esteem while simultaneously preventing conscious identification of self-referential terms (more on 'implicit' later). During subliminal conditioning, a masked term (the Conditioned Stimulus, or CS) is presented under consciously imperceptible ('subliminal') conditions. The CS is followed by a valenced attribute (the Unconditioned Stimulus, or US) typically presented under supraliminal/visible conditions. Repeated correlations between subliminal CS and a visible US can cause the valences of the latter to generalize towards the former without requiring conscious identification of the CS (Amd and Baillet, 2019; Amd and Passarelli, 2020; Passarelli et al., 2022). In the study by Dijksterhuis (2004), Dutch undergraduates viewed CS analogous to the English self-referential *I* (CS+) or the neutral letter *X* (termed CS-) for 17 millisecond (ms) durations. To further attenuate the likelihood of conscious detection, all CS were sandwiched between backward and forward masks. This brief masked presentation rendered the CS effectively invisible from conscious awareness (Amd and Passarelli, 2020). After the CS, Dijksterhuis (2004) 's participants viewed either positive or neutral attributes (US+ or US-), or random letter strings, across trials. US were presented under visible (Experiment 1) or subliminal (Experiment 3) visual conditions. Participants for whom CS+ were exclusively followed by US+ (CS+ → US+) produced performances indicative of increased 'implicit' self-esteem, independent of US visibility. The outcomes reported by Dijksterhuis (2004) and others (Baccus et al., 2004; Riketta and Dauenheimer, 2003) suggest the positive valences of US+ had generalized to an 'unconsciously' encoded self-referential CS+ , which subsequently led to enhanced (implicit) self-esteem.

Dijksterhuis (2004) 's interpretations become complicated by four considerations. First, note how the outcomes reported by Dijksterhuis (2004) reflected *post*-conditioning measures exclusively, which cannot distinguish whether outcome parameters had been influenced by conditioning or pre-experimental inter-individual differences (Amd and Roche, 2015). Second, correlating *I* with *PLEASANT* leaves open the question as to exactly which specifier participants had subjectively assigned. As outlined by De Houwer et al. (2021), viewing an *I* → *PLEASANT* (CS+ → US+) sequence during a conditioning task may occasion the derivation of an *I AM PLEASANT* proposition.

However, it is equally possible that (some) participants may derive propositions to the effect of *I WANT TO BE PLEASANT*, or *I CAN BE PLEASANT*, or even *I AM NOT PLEASANT* , all of which could be expected to elicit different connotations. Third, Dijksterhuis (2004) (Experiment 3) demonstrated that performance-based measures of self-esteem could be influenced by CS+ → US+ sequences when CS *and* US were subliminally presented (Experiment 3). This contradicts more recent studies that claim visibility of the unconditioned stimulus (US) to be a necessary condition for effective emotional appraisal (Amd and Baillet, 2019; Amd and Passarelli, 2020; Lahteenmaki et al., 2015).

Our final concern relates to Dijksterhuis (2004) 's decision to employ implicit association tests (IATs) for measuring *implicit* self-esteem. The latter concept can be differentiated from explicit reports of 'self-esteem', which reflect deliberated responses to a constructed concept of self relatively downstream in the stimulus processing chain (Greenwald and Banaji, 1995). Measures like the IAT, to compare, constrain deliberation through restricting response time and limiting response options, which supposedly captures "the product of automatic, intuitive processing of affective experiences" (Grumm et al., 2009, p. 6). However, it is uncertain exactly what self-esteem IAT performances are capturing, seeing how correlations between IAT performances and validated self-esteem questionnaires have remained close to zero for over a decade (Hofmann et al., 2010; Schimmack, 2019). These considerations may explain why, in a recent meta-analysis of psychological interventions on self-esteem, Niveau et al. (2021) reported that although most "studies of evaluative conditioning's impact on self-esteem (have focused) on implicit self-esteem", these have largely yielded "mixed results" (p. 3). Furthermore, IAT performances can be influenced by top-down deliberations, similar to survey responses (Corneille and Hutter, 2020). This is not unexpected when considering IATs capture *evaluative* responses, which necessitate the conscious (thereby modifiable) specification of relational propositions (De Houwer et al., 2021).

The above concerns were addressed across three experiments currently. Building upon the work of Dijksterhuis (2004) and related findings (Baccus et al., 2004; Grumm et al., 2009), cohorts of Fijian undergraduates were exposed to three variants of a subliminal CS → US conditioning procedure. In each experiment, participants were randomly assigned to active or control groups. Participants in active groups viewed the term *#I#AM#* (CS+) followed by positive attributes (US+), or the term *#M#IA#* (CS-) followed by neutral attributes (US-), so CS+ → US+ and CS- → US- sequences. CS-US mappings were reversed for control groups, who viewed CS- → US+ and CS+ → US- sequences. Similar to Dijksterhuis (2004), all CS presentations were sandwiched by backward and forward ###### masks to attenuate likelihood of conscious CS identification, which is standard practice across subliminal conditioning preparations (Amd and Baillet, 2019; Amd and Passarelli, 2020; Passarelli et al., 2022). The single procedural parameter manipulated between experiments was stimulus duration. Specifically, across Experiment 1, 60 participants viewed subliminal CS followed by visible US; across Experiment 2, 59 participants viewed subliminal CS followed by subliminal US; finally, across Experiment 3, 60 participants viewed visible CS followed by visible US.

Two outcome measures, corresponding to affective state shifts and explicit self-esteem, were estimated for individual participants across experiments (detailed under Results). The former identified whether subjectively experienced affect had shifted as a function of conditioning by estimating normalized differences between two positive (happiness, optimism) and two negative (frustration, anger) moods collected before and after conditioning (detailed under Method). The second outcome measure constituted of a single item deployed immediately after conditioning that was designed to capture explicit self-esteem. During analyses, we explored whether observed parameter distributions produced evidence for/against a one-sided null hypothesis (active - control ≤ 0) using Welch's contrasts and bias-corrected Hedge's g estimates with bootstrapped confidence intervals (Delacre et al., 2017).

2. Method

2.1. Data availability and ethical statement

Data and the analysis script is available in an online OSF repository (https://osf.io/vxk24/?view_only=1b4cb3800b9f4555ad2bb749fd3a13be). All procedures reported had been approved by the University IRB and adhere to the Declaration of Helsinki guidelines. The current study was not pre-registered.

2.2. Participants

217 undergraduate students the University of the South Pacific Laucala had volunteered for the present investigation in exchange for course credit. We had aimed on recruiting 60 participants per experiment for balanced two-sample contrasts between active and control groups. A sensitivity analysis for a two-sample t-test with 80 % power and a 5 % Type-1 error rates had indicated a sample of 60 participants could detect moderate effects ($d = 0.52$). Following inspection of the data, 38 participants had to be removed due to missing responses, leaving a final sample of 179 participants (M = 26.1; SD = 5.6 years) consisting of 70 male, 106 female and 3 non-binary participants. Participants were randomly assigned to active or control conditions across experiments, which were balanced for the most part (most N 's = 30) with the exception of the control group from Experiment 2, which

consisted of $N = 29$.

2.3. Materials

The terms *#I##AM#* and *#M##IA#* were classified as CS+ and CS- respectively.

All CS presentations were sandwiched between ###### masks during conditioning to minimize the likelihood of their conscious detection (Amd and Baillet, 2019). '#' fillers were embedded across CS to minimize morphological variation relative to masks, further attenuating CS detection likelihood (Dijksterhuis, 2004). Positively valenced terms (US+) constituted of *DELIGHTFUL, GOOD-NATURED, CHARMING, AGREEABLE, WARMHEARTED, ENJOYABLE, PLEASING* and *DEPENDABLE*. Neutral terms (US-) constituted of *UNBIASED, IMPARTIAL, DISPASSIONATE, BALANCED, NONALIGNED, DETACHED, INDIFFERENT* and *NONCHALANT*.

US selection was informed by two considerations: first, US+ (or US-) categories had to consist of words that reliably connotated a positive (or neutral) valence. Second, both US categories had to be morphologically consistent to control for bottom-up artefacts. In relation to the former, a pilot study with an unrelated sample had evaluated series of words using a 5-point visual analog scale anchored by sad and happy faces (adapted from Amd, Machado, Oliveira, Passarelli, & De Rose, 2019). The mean ± SD valences for US+ and US- words were recorded to be 3.57 ± .73 and 2.83 ± 1.02 respectively. Those ratings correspond with the mean ± SD valences for six of our US+ words (7.3 ± .3), and four of our US- words (4.7 ± 1.02), in an affective word database containing nearly 14,000 English lemmas (Warriner et al., 2013), partly vindicating our classification strategy. We say 'partly' since remaining US+ (*GOOD-NATURED, WARMHEARTED*) and US- (*UNBIASED, DISPASSIONATE, BALANCED, NONALIGNED*) words were not reported in Warriner et al. (2013) 's dataset. We had not exclusively sampled from Warriner et al. (2013) 's dataset to accord with our second goal behind US selection, which was to control for morphological variability. For instance, the mean ± SD number of elements across US+ (9.86 ± 1.64) and US- (9.75 ± 1.67) were statistically indistinguishable to control for differences in 'word length' influencing outcome variance (Yu et al., 2021).

We also equalized the ratio of vowels-to-consonants across $US + \frac{32}{55} = .582$ and $US - \frac{32}{56} = .571$ categories to control for potential 'phonaesthetic' artifacts, which describes the perceived pleasantness or unpleasantness inherent to particular vowel-consonant combinations (Crystal, 1995; Firth, 1935). All stimuli appeared in a white 18 Arial font in the center of the screen against a black background. All tasks were developed and administered using *Psytoolkit* (Stoet, 2010, 2016). A working sample of the conditioning procedure is available as an html file in the online materials. Data analyses were run on Posit (formerly RStudio - R Core Team, 2021) using the *tidyverse*(Wickham et al., 2019), *rstatix*(Kassambara, 2023), *ggplot2* (Wickham, 2016), *reshape2* (Wickham, 2007), *apa*(Gromer, 2020), *pwr* (Champely, 2020), and *effectsize* (Ben-Shachar et al., 2020) packages. The manuscript was typeset in R Markdown (Xie et al., 2020) using a *papaja* template (Aust and Barth, 2022). All data, analysis scripts and the raw markdown file are available in the online OSF file (*LINK*).

2.4. Procedure

Undergraduate students from the University of the South Pacific received links to access the current study in exchange for optional course credit, which included information and consent forms. Only participants who consented were entered into the study, which commenced with collecting demographic information (e.g., age and sex). Next, participants viewed one of four questions *How HAPPY* (or *FRUSTRATED/ANGRY/OPTIMISTIC*) *do you feel right now?* alongside 10-point slider scales anchored by the labels *Not at all* (1) and *Very Much* (10) on the left and right sides respectively. After four mood ratings were collected, participants viewed instructions about an upcoming 'word detection' task.

Specifically, they were instructed to use the 'z′ or 'm′ letters to indicate whether the last word they could consciously detect had been positive or neutral. The task commenced following a spacebar press, which produced a white '+ ' fixation cross appeared on screen center. If no response was detected within 3000 ms (ms), a message stating *Press the spacebar to begin* appeared directly underneath the cross. A spacebar press produced a ###### forward mask for 170 ms, followed by either *#I##AM#* (CS+) or *#M##IA#* (CS-) for 17 ms (Experiments 1 and 2) or 500 ms (Experiment 3) followed by another ###### backward mask for 170 ms (see Fig. 1). The screen then cleared for 500 ms, after which a neutral (US-) or positive (US+) word appeared on screen for 500 ms (Experiments 1 and 3) or 17 ms (Experiment 2). Next, two enclosed 'boxes' with the labels *POSITIVE* and *NEUTRAL* appeared near the left and right sides of the screen respectively (left-right positioning of the labeled boxes were randomized across trials). If no response was detected within 5000 ms, the phrases *press 'z′* and *press 'm′* respectively appeared beneath the boxes near the left and right sides of the display, along with the question *Did you see a POSITIVE or NEUTRAL word?*. All items remained on screen until the participant pressed the letters 'z′ or 'm′ on a physical keyboard. This cleared the screen for 500 ms, followed

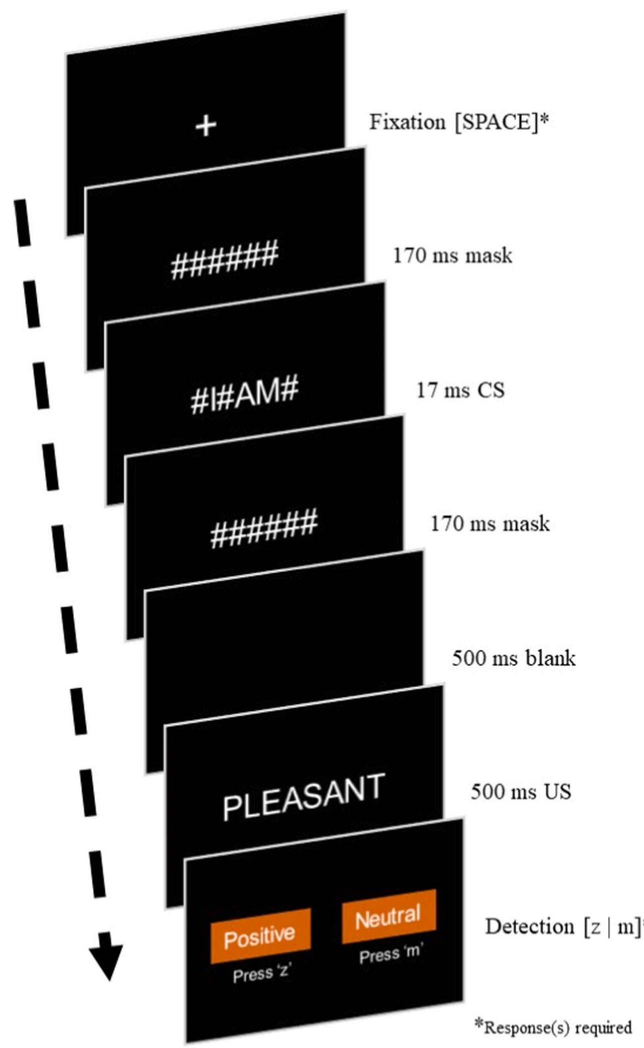

Fig. 1. Conditioning task sequence from Experiment 1. Participants had to press the spacebar to commence the trial following fixation. They next had to identify the valence of the previously displayed US using the letters 'z' or 'm' on the keyboard. In Experiment 2, both CS and US appeared for 17 ms. In Experiment 3, both CS and US appeared for 500 ms.

by a '+ ' fixation signaling onset of the subsequent trial. All participants underwent 16 conditioning trials. A pilot study confirmed that none of the CS were consciously perceptible, corroborating earlier reports (Amd and Baillet, 2019; Amd and Passarelli, 2020; Greenwald and De Houwer, 2017). Completion of 16 conditioning trials was immediately followed by the question *How favorably do you feel about yourself?* alongside a 10-point slider scale. Finally, participants were asked to (again) report their current levels of happiness, optimism, frustration and anger using 10-point scales, after which the experiment terminated.

3. Results

3.1. Affect scores

All participants reported their current levels of happiness, optimism, anger and frustration before (*T1*) and after (*T2*) conditioning. For each mood, normalized scores across time were estimated, where $\Delta_{mood} = \frac{T2_{mood} - T1_{mood}}{T2_{mood} + T1_{mood}}$. Affect scores for each participant constituted the difference between the sum of normalized positive moods from the sum of normalized negative moods, so $\Delta_{happy} + \Delta_{optimism} - (\Delta_{anger} + \Delta_{frustration})$.

Score normalization emphasized mood *shifts,* controlled for inter-subject variance and scaled observations to a standardized range (Amd, 2022; Amd and Passarelli, 2020; Amd and Roche, 2015). Positive (or negative) affect scores respectively indicated mood had shifted positively (or negatively) after conditioning. Mean affect scores alongside 95% confidence intervals (CIs) for active and control groups are summarized in the top panel of Table 1. Bonferroni-corrected Welch's contrasts confirmed mood had reliably and positively shifted for the active (relative to control) group across Experiment 1 only, $t(48.3) = 2.46$; $p = 0.017$, g [95 %] = 0.63 [0.11, 1.14]. Contrasts between active and control groups across.

Experiment 2, $t(48.3) = 0.09$; $p = 0.932$, g [95%] = 0.02 [− 0.48, 0.53], and across Experiment 3, $t(40.2) = 0.59$; $p = 0.558$, g [95 %] = 0.15 [− 0.35, 0.65], were non-significant. Affect score summaries for active and control groups are illustrated in the top row of Fig. 2 After planned contrasts, additional *t*-tests explored whether pre-conditioning moods varied between active and control groups, as this could influence sensitivity to the acquisition of valenced information (Wright and Bower, 1992). None of the contrasts reached significance across any experiment (all p's > .1; all g's < .42), implying mean levels of happiness, optimism, frustration and anger did not vary between active and control groups before conditioning across any experiment.

Table 1
Descriptive summaries for active and control groups across individual experiments and outcome parameters.

Experiment: Condition	Mean [95 % CI]
Normalized affect	
Exp1: Active	0.59 [0.54–0.64]
Exp1: Control	*0.12 [0.04–0.21]*
Exp2: Active	0.07 [− 0.04–0.18]
Exp2: Control	*0.06 [− 0.01–0.12]*
Exp3: Active	0.04 [0.01–0.07]
Exp3: Control	*-0.04 [− 0.1–0.02]*
Self-esteem	
Exp1: Active	6.37 [6.2–6.53]
Exp1: Control	*5.73 [5.57–5.9]*
Exp2: Active	6.13 [5.97–6.29]
Exp2: Control	*5.57 [5.4–5.73]*
Exp3: Active	6.53 [6.39–6.67]
Exp3: Control	*5.5 [5.41–5.59]*
US visibility	
Exp1: Active	0.82 [0.81–0.84]
Exp1: Control	*0.76 [0.74–0.77]*
Exp2: Active	0.57 [0.55–0.58]
Exp2: Control	*0.63 [0.62–0.65]*
Exp3: Active	0.82 [0.81–0.83]
Exp3: Control	*0.83 [0.82–0.83]*

Note: Underlined values were significantly different following experiment-specific Active vs Control contrasts

3.2. Self-esteem

Immediately after conditioning, participants responded to the statement *How favorably do you feel about yourself?* using a 10-point scale, which functioned as a single-item proxy of self-esteem. Mean self-esteem with 95 % CIs for all active and control groups are summarized in the middle section of Table 1. Welch's tests indicated that self-esteem ratings did not statistically vary between active and control groups across Experiment 1, $t(58.0) = 1.36$; $p = 0.179$, g [95 %] = 0.35 [− 0.16, 0.85], nor Experiment 2, $t(56.8) = 1.12$; $p = 0.268$, g [95%] = 0.29 [− 0.22, 0.79]. Only across Experiment 3 were active participants observed to report significantly higher self-esteem relative to control participants, $t(50.3) = 3.10$; $p = 0.003$, g [95 %] = 0.79 [0.27, 1.31]. Self-esteem summaries and effect sizes are indicated in the bottom row of Fig. 2. A Pearson correlation indicated that scaled self-esteem and affective shift scores were significantly and positively associated, $r(177) = .28$, $p < .001$.

3.3. US visibility

Recall that all participants had been instructed to identify US valence (as positive or neutral) immediately after US offset. Across Experiments 1 and 3, US appeared for 500 ms, which was sufficiently long to have been consciously detected. Across Experiment 2, US appeared for 17 ms, matching the onset displays of subliminal CS but without any masks. Responses which accurately reflected valence of the prior US (e.g., identifying the word *PLEASANT* as Positive) implied US had been consciously detected, and were scored as '1'. All other responses were scored as '0'. Mean detection accuracies did not significantly vary between active and control groups across Experiment 1, $t(55.3) = 1.69$; $p = 0.096$, g [95%] = 0.43 [− 0.08, 0.94]; Experiment 2, $t(56.3) = -1.71$; $p = 0.093$, g [95%] = . -0.44 [− 0.95, 0.07]; nor Experiment 3, $t(55.8) = -0.49$; $p = 0.626$, g [95%] = −0.12 [− 0.62, 0.38] (middle row, Fig. 2). Between experiments, the mean detection accuracy for US which appeared for 500 ms varied between 74 % and 83 %. When US appeared for 17 ms, detection accuracies ranged between 56 % and 64 % (bottom panel, Table 1).

4. Discussion

We conducted three experiments which involved exposing Fijian undergraduates to different variants of subliminal CS-US conditioning sequences. Across each experiment, we investigated whether affective state shifts and explicit self-esteem varied between participants in active and control groups. Participants in the active group were exposed to a masked self-referential CS+ (*I AM*) paired with a positive US+ (*PLEASANT*), while the control group saw the scrambled counterpart CS- (*M IA*) paired with a neutral US- (*INDIFFERENT*). The single parameter varied between experiments was CS and US display durations. Specifically, CS appeared for 17 ms across Experiments 1 and 2, and for 500 ms across Experiment 3. Similarly, US appeared for 500 ms across Experiments 1 and 3, and for 17 ms across Experiment 2. Planned contrasts highlighted a reliable (>80% power) and positive shift in affective state across active relative to control participants from Experiment 1 only, who had viewed subliminal CS followed by visible US. Additional contrasts showed a significant increase in self-esteem across active relative to control participants across Experiment 3, who viewed both CS and US for supraliminal durations.

In Experiment 2, where both CS and US were subliminal, no shifts in affect or self-esteem were observed, suggesting that US visibility was crucial for conditioning-induced changes. These differential outcomes warrant a closer examination of our target constructs of 'affect' and 'self-esteem'.

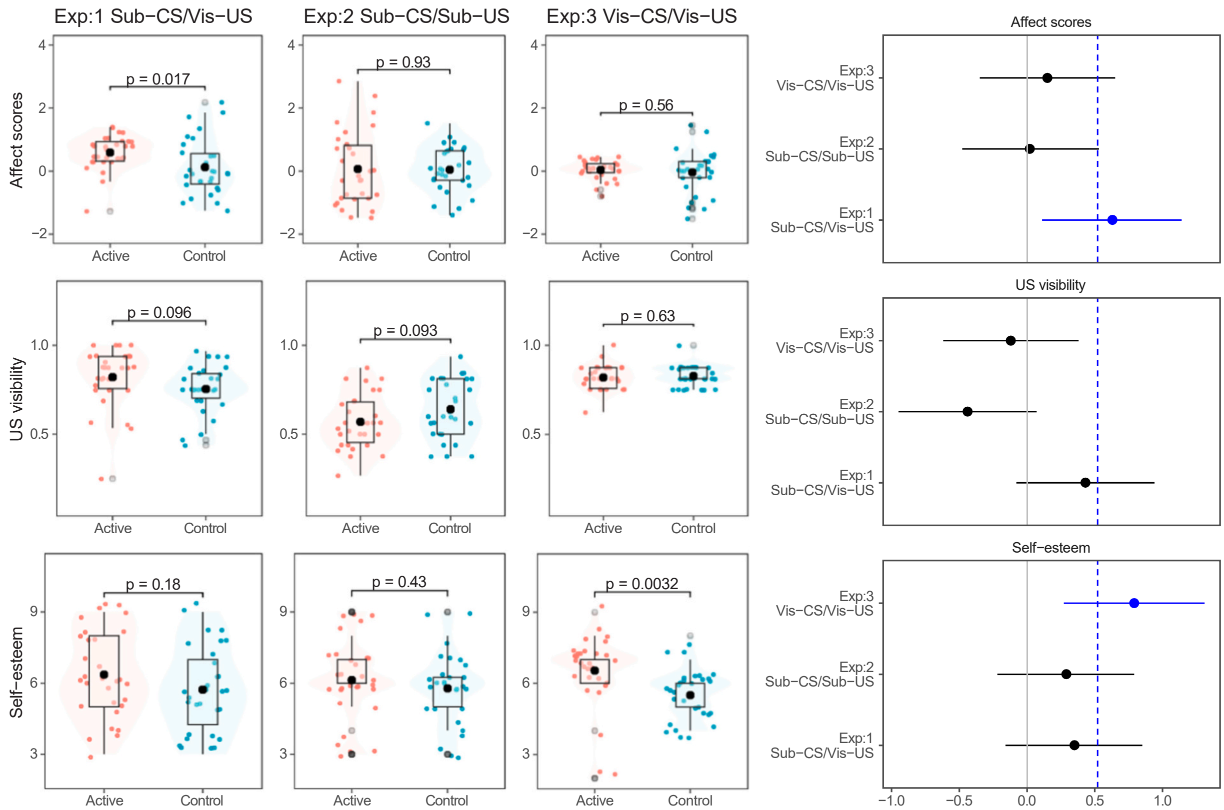

Fig. 2. Violin-boxplot summaries of outcome measures (rows) across experiments (columns) are illustrated in Panel A. Effect size estimates with 95 % CIs following *Active- Control* contrasts for each outcome measure and experiment are illustrated in Panel B. Across the latter, the solid vertical intercept indicates the null. The dashed intercept indicates the smallest effect that can be detected with 80 % power following sensitivity tests ($d = 0.52$).

First, by *affect*, we imply the initial, centrally-generated response in the stimulus-elicited processing chain that can be differentiated with respect to perceived valence (Amd, 2014; Staats, 1996). Affective responses can become differentiated as early as 150–200 ms of stimulus perception, whereas lexical processing (involved in proposition generation) takes at least 250 ms of preparatory activity (Amd and Baillet, 2019; Amd et al., 2013; Kulke et al., 2019). Importantly, stimuli can trigger varying affective responses even when presented under conditions that preclude conscious appraisal (Amd and Baillet, 2019). The notion of a rapid, potentially non-conscious, inclination to respond to affective stimuli appears evolutionarily sensible when considering that conscious symbol manipulation likely evolved later in human history (Passarelli et al., 2022). These observations led us to hypothesize that subjective reports of one's immediate affective state might better capture conditioning outcomes, especially if the self-referent *I AM* (CS+) had been encoded 'outside' conscious awareness and was inaccessible to the latter. Additionally, since we measured four different mood states both before and after the conditioning, any systematic changes in affect could be directly linked to our experimental intervention.

Our *self-esteem* measure, to compare, required a 'self-as-subject' be consciously available before any evaluation could be made. Only participants allocated to the active condition in Experiment 3 could be expected to positively evaluate a consciously realized self, which they did, seeing as they were the only group for whom both *I AM* (CS+) and *PLEASANT* (US+) appeared for consciously perceptible durations. It's plausible that consciously evaluating the subject-relational term cued corrective or contraindicative propositions among some participants, which attenuated their mood reports (Corneille and Lush (2022)). These considerations help clarify why the conscious evaluation of *I AM* → *PLEASANT* sequences in Experiment 3 impacted explicit self-esteem, but did not produce similar effects on affective shifts. These results align with Grumm et al. (2009), who also explored the effects of subliminal conditioning on both implicit and explicit self-esteem.

Those authors found that subliminal conditioning produced no effect on explicit self-esteem, similar to our Experiment 1. However, Grumm et al. (2009)'s third experiment did find a significant impact on explicit self-esteem, but only after participants had been explicitly instructed to "think about their momentary feelings" in relation to themselves before conditioning. The sequential presentation of *I AM* and *PLEASANT* for consciously perceptible durations appears functionally analogous to Grumm et al. (2009)'s instructions, in that both approaches explicitly orient participants towards self-evaluations, explaining Experiment 3's outcomes.

Our results partly corroborate earlier claims of (implicit) self-esteem becoming enhanced through subliminal conditioning. We say 'partly' for two reasons: first, contrary to Dijksterhuis (2004), none of our outcome parameters were statistically influenced when US appeared for 17 ms durations, implying conscious US identification had been necessary for eliciting valence (Lahteenmaki et al., 2015). Second, we did not deploy any IATs or letter-preference tasks, which are conventionally used for measuring automatically-generated ('implicit') self-esteem (Baccus et al., 2004; Dijksterhuis, 2004). IATs were not included given their low construct validity with respect to self-esteem (Schimmack, 2019), alongside conceptual concerns with the ill-specified notion of 'implicitness', as noted previously (Corneille and Hutter, 2020). As for why letter-preference tests were not included, note that at least two assumptions have to be satisfied before any outcomes across such tests can be meaningfully interpreted: first, one must assume that appraisal of an *I* → *PLEASANT* sequence leads to the positive valence from *PLEASANT* generalizing to a consciously perceived *I*. One must additionally assume the initials of one's name are already 'linked' to the self-referential term [*I*], in which case the valences which generalize to *I* (from *PLEASANT*) can be expected to influence evaluations of one's initials (Dijksterhuis, 2004). In contrast, our affective shift score necessitates only the former assumption be met. If positive valences do generalize to the self, an introspective report of one's immediate affective state could arguably serve as a more accurate measure of subliminally influenced self-esteem, given that affective responses are early in the stimulus-response chain, compared to letter-preference tests or IATs (Amd and Baillet, 2019).

The assumption that letter-preference tasks may reflect subliminally conditioned self-esteem seems reasonable in light of the well-documented phenomenon of 'sensory preconditioning', which commences with the pairing of two neutral stimuli. Then, if one of those stimuli is paired with an unconditional stimulus (US), the other (formerly neutral) stimulus can elicit a conditioned response when presented alone, even though it had never been directly paired with any US (Rescorla, 1980). In the subliminal conditioning protocol deployed presently however, one of the associated stimuli across 50 % of all trials was always a positive US (e.g., *PLEASANT*). Additionally, there was no pre-conditioning phase in which either term (*I AM* or *PLEASANT*) was associated with another affective attribute. Without having associated one's initials with a self-referential pronoun beforehand, the findings described in Dijksterhuis (2004) and related works (Baccus et al., 2004; Grumm et al., 2009) cannot be easily attributed to sensory pre-conditioning processes. Relatedly, the latter cannot predict why the systematic manipulation of conscious access to CS and/or US presentations would differentially impact variances across outcome parameters. Our own interpretation of these outcomes are elaborated in the subsequent section.

4.1. Theoretical considerations

Our account of the present findings hinges on Holt (1914)'s observation that propositions (like *I AM PLEASANT*) cannot be consciously formed if the terms involved (*I* and *PLEASANT*) are unavailable to awareness. Given that participants allocated to Experiment 3 were explicitly aware of the target proposition (*I AM PLEASANT*), they might have been predicted to evaluate themselves more favorably in accordance with Mowrer (1954)'s assertion that propositions may operate as symbolic 'conditioning devices'. From this framework, valenced terms (US) can modify the response-eliciting properties of a previously neutral subject, transforming it into a CS. The interpretation derived from Mowrer (1954) aligns with contemporary learning theorists who argue that all evaluations are fundamentally based on "propositional representations" which involves the specification of relational information (De Houwer et al., 2021). From this view, the presentation of *I AM* before *PLEASANT* for perceptible durations constitutes an 'optimal' condition that is both necessary and sufficient for explaining Experiment 3′s outcomes. However, a term that is encoded without being consciously detected, as in Experiment 1, does not fulfill the criteria for forming a proposition, since no relations are specified (De Houwer et al., 2021).

This raises the psychologically significant question of *what* is being learned at a non-conscious level (Amd, 2022).

One possibility is that the characters I, A, and M may have been perceptually encoded and 'mentally represented', sharing basic, unspecified links ('associations') between them (Harris, 2006). Once encoded, these elemental representations might have triggered other representations through 'spreading activation'. This describes how the elicitation of a self-as-subject representation, following exposure to the character *I*, might weakly activate ('spread to') associated self-related concepts (Dehaene et al., 2006). Since 'associations' don't specify any particular relationship between characters, they circumvent any need for conscious identification of the latter (Gawronski and Bodenhausen, 2018; Jurchis et al., 2020). Accepting these premises, one can posit that conditioning sequences generated *I AM -PLEASANT* associations, irrespective of whether the associated terms were consciously identified. In this respect, our findings more closely align with viewpoints positing the operation of associative and propositional processes (Gawronski and Bodenhausen, 2018; McLaren et al., 2014) versus those positing a single, propositional process exclusively (De Houwer et al., 2021).

On balance, an associative interpretation faces at least two challenges. First, if associations are to be understood as 'unspecified links' between 'mentally represented' symbols, one must confront the issue that any symbol identified consciously is inherently propositional (Burks, 1949). Even individual elements, like *I* or *M*, assert specific relationships upon conscious appraisal (Rantzen, 1993). On this matter, we concur with De Houwer et al. (2021) that a 'mental association' (between terms like *I AM* and *PLEASANT*) becomes functionally indistinguishable from two "propositional representations" perceived to be related. However, if symbols are not consciously appraised, they cannot specify any relational information, which includes relationships of identity (Holt, 1914). This renders the notion of a 'non-conscious representation' self-contradictory, as it lacks the very specificity presupposed by the concept of a representation (Fodor, 1975). So, either 'associations' form between consciously represented symbols, in which case they are indistinguishable from propositions (De Houwer et al., 2021). Or, associations can form between self-contradictory 'non-conscious representations' that generate (McLaren et al., 2014) and/or interact with (Gawronski and Bodenhausen, 2018) propositional representations in some unknown manner (De Houwer et al., 2021). Another challenge faced by an associative account arises when considering that, in contrast to previous studies (Dijksterhuis, 2004; Grumm et al., 2009), our experiments kept elemental composition consistent across CS. Specifically, the control term *M IA* (CS-) was a reconfiguration of *I AM* (CS+), guaranteeing that all participants encountered the letters *I*, *M*, and *A* an equal number of times with positive US throughout the conditioning process. Despite this uniform exposure, our results unambiguously revealed differences between active and control groups, implying how elements were spatially *organized* relative to each other might have been central to the non-conscious differentiation of CS structures (Amd, 2022; Jurchis et al., 2020).

We emphasize that our notion of an 'organizing relation' does not presuppose any direct correspondences with 'mental associations' and/or 'propositional representations' (Neuber, 2022; Spaulding, 1912). Both humans and many non-humans can reliably discriminate between configurations constituting of previously un-encountered elements, but only humans can specify propositional relations between socially defined symbols (Penn et al., 2008; Swinton et al., 2019). Thus, similar to our arguments for pre-lexical and affect-sensitive response systems (Amd and Baillet, 2019; Amd et al., 2013), we posit that behavioral systems dedicated to organizational pattern recognition likely evolved prior to our symbolic manipulation capabilities. From this perspective, affective and organizing relations can be theorized to influence symbols "without being coupled to propositional knowledge" (p. 119, Amd, 2022).

This view readily accounts for the current findings. If CS configurations can be encoded "without knowledge of their constituent elements", then a non-conscious 'self-as-subject' would still remain susceptible to valences elicited by a consciously appraised US (p. 119, Amd, 2022). Thereby, any positive valences transferred to a consciously non-realized self would manifest in the latter becoming positively valenced, explaining the outcomes of our first experiment. Alternatively, when CS configurations are consciously identified, the conditions for propositional specification become met, anticipating the top-down moderation of evaluations based on appraised information (Corneille and Lush, 2022). Because participants 'knew' where to attribute any experienced variations in their affective state by virtue of consciously appraising *I-AM* → *PLEASANT* sequences (Waroquier et al., 2020), they might be expected to positively evaluate a consciously realized self-as-subject, explaining the outcomes of our third experiment. Lastly, because our second experiment impeded conscious US detection, the conditions for eliciting valence responses and subsequent valence generalization had not been met (Amd and Passarelli, 2020; Lahteenmaki et al., 2015).

Our theoretical position derives from a behavioristic epistemology grounded in a direct realist ontology (Amd, 2022; Holt, 1914; Spaulding, 1912; Tonneau, 2013). A common and well-defined ontology is necessary for meaningful scientific progress since, without it, even the concept of 'progress' becomes undefinable (Ladyman et al., 2007). A realism which acknowledges the "plurality of psychological relations" (p. 119, Amd, 2022) that can directly influence an organism without presupposing additional hypotheses about mediating representational structures is a viable candidate for constructing a theoretical framework on metaphysically certain grounds (Marsh and Boag, 2014). Metaphysical certainty differentiates a coherent system of thought from a collection of disconnected facts and theories (Guénon, 2009), the latter being a growing issue in psychology since the advent of the 'cognitive revolution' (Amsel, 1992; Tonneau, 2013; Zagaria et al., 2020). Our framework provides an 'a-representational' alternative capable of generating diverse and innovative hypotheses in evaluative learning and related areas (Amd, 2022). The term *a-representational* is used because, as realists, we remain neutral as to whether the 'mental representations' mediating evaluative responses constitute of associative and/or propositional processes. Consequently, we are largely impartial in the ongoing debate between proponents of 'dual-process' (Gawronski and Bodenhausen, 2018) and 'single-process' (De Houwer et al., 2021) approaches to evaluative learning which, echoing the famous 'contiguity versus contingency' debates between Hull and Guthrie, anticipate no clear resolutions (Kimble, 1961; Staddon, 2014). Our own view is that all evaluations, by virtue of being consciously specified, are necessarily propositional, in agreement with De Houwer et al. (2021). Our position diverges from the latter through the assertion of "non-propositional" relations, including (but not limited to) affective and organizing relations, that may not necessarily be associative (Amd, 2022). We conclude with a summary of our findings after addressing some limitations of our design.

4.2. Limitations

First, the use of a single-item measure for explicit self-esteem, administered only once post-conditioning, warrants justification. One might question why we opted not to use a more established and comprehensive instrument, like the Rosenberg Self-Esteem Scale (RSES) (Gnambs et al., 2018), which would be less prone to random measurement errors and specificity issues compared to a single-item scale. Additionally, using a single post-conditioning measure limited our ability to attribute self-esteem changes directly to our conditioning protocol as opposed to pre-existing individual differences, a limitation we previously noted in Dijksterhuis (2004)'s work.

Acknowledging this limitation, we still reasoned that a single-item measure would sufficiently capture 'self-esteem' based on Gnambs et al. (2018)'s meta-analysis of the RSES' factorial structure, which revealed a single general-level factor accounted for approximately 85 % of the scale's common variance. Additionally, we aimed to assess self-esteem effects as rapidly as possible to minimize (any) extinction effects modulating affect reports. We did not implement self-esteem measures after affect measurements in order to avoid priming effects. For instance, imagine one participant self-evaluated after responding to the question "how *happy* do you feel right now?" while another did so after viewing "how *angry* do you feel right now?", the affective connotations of *happy* versus *angry* may have influenced subsequent self-esteem ratings, which we sought to avoid.

Relatedly, one might question why we didn't capture pre- and post-conditioning self-esteem to normalize scores, akin to our affect metric. We avoided collecting self-esteem ratings before conditioning to avoid inadvertently prompting participants to generate task (un)related hypotheses, which could have influenced subsequent evaluations (Corneille and Lush, 2022). Furthermore, given that the only group expected to show increased self-esteem did so (after appraising visible *I AM* → *PLEASANT* sequences) indicates our outcome measure was sufficiently sensitive to the effects of conditioning. Nevertheless, we acknowledge the inherent limitations of a single-item post-conditioning outcome measure, and recommend future works to validate our single response scale with an established measure such as the RSES (Gnambs et al., 2018).

Another concern may be raised in relation to the presumed subliminality of Conditioned Stimuli (CS) terms. By 'subliminality', we imply visual conditions under which presented terms are unlikely to be consciously identified (Amd and Baillet, 2019). Because current participants were not explicitly asked to report whether they had consciously detected any of the CS, we cannot claim with certainty that every CS presentation across Experiments 1 and 2 went undetected. On balance, it's worth restating that the display parameters employed presently have been demonstrated to effectively render stimuli subliminal across earlier studies (Amd and Passarelli, 2020; Di Domenico et al., 2016; Grumm et al., 2009; Passarelli et al., 2022). We also noted earlier how participants in an unpublished pilot could not verbally report detection of 'any word flashing' after each mask-CS-mask sequence, which validated our presentation parameters. Similarly, no visibility checks were deployed to avoid priming (potentially) interfering evaluations (Amd and Baillet, 2019). Readers interested in assessing our claims of CS subliminality can access our conditioning task in the online materials and attempt to discern between the various CS.

Next, our choice to label affective terms like *PLEASANT* and self-referential terms such as *I AM* as US and CS, respectively, could be justified. Given that both terms are meaningful sequences of letters learned in relation to one's perceived self, an argument could be made that both *I AM* and *PLEASANT* would more appropriately be classified as CS, with one's self-perception serving as the US. We acknowledge that any symbol, including sequences of meaningful letters, is inherently learned in the context of one's self-perception (Burks, 1949). However, the overarching claim that 'all' symbols are learned in relation to one's self fails to account for how specific symbols come to be psychologically differentiated. Similar considerations led Mowrer (1954) to propose that certain propositions function as 'conditioning devices', where a salient attribute (US) could influence selective properties a predicated subject (CS), regardless of the fact that both the subject (*I AM*) and the attribute (*PLEASANT*) were learned in the context of one's self-perception.

The validity of Mowrer (1954)'s heuristic hinges on two key assumptions: first, that symbols chosen to function as US can unconditionally elicit affective responses without being experimentally prepared to do so. Second, a previously neutral symbol can be shown to elicit affective responses after being systematically related with the US, thereby transforming into a Conditional Stimulus, or CS (Das, 2014). Supporting these points, research has consistently shown that affective words, such as *PLEASANT*, unconditionally elicit emotional responses, irrespective of the idiosyncratic circumstances under which the original meanings of these terms were acquired (Bosshard et al., 2019; Kensinger and Schacter, 2006; Staats, 1996). Additionally, overexposure to affective words can diminish their emotional impact but only temporarily, akin to how overexposure to the US can temporarily reduce US effectiveness (Black et al., 2013; Rescorla, 1973). Given that affective words unconditionally elicit emotional responses and persist in their ability to do so, words such as *PLEASANT* function more like US than CS. The notion that affective terms maintain their salience is also supported by their usage in contexts unrelated to self-perception. For instance, hearing phrases like *the weather is PLEASANT* or *that's a NICE car* still conveys a positive sentiment, regardless of whether the listener currently perceives themselves as 'pleasant' or 'unpleasant'.

Some readers may remain unconvinced, pointing out that the designation of *I AM* as a CS would necessarily elicit any prior affective responses associated with one' self. If one's self-assessment is negative (e.g., "I am an unpleasant person"), the efficacy of an externally presented *I AM PLEASANT* proposition in univocally generating positive self-evaluations could be called into question. In response, we point out that this criticism is applicable to Experiment 3 exclusively, which produced the necessary conditions for participants to moderate their responses based on self-perceptions. Specifically, only Experiment 3′s 'active' group viewed CS (*I AM*) followed by the positive US (*PLEASANT*) for durations long enough to be consciously identified, a minimum requirement for appraising an *I AM*.

PLEASANT proposition (Amd and Passarelli, 2020; Tonneau, 2004). The same group also produced significantly higher self-esteem ratings relative to control participants (as well as all other participant groups), implying that, collectively, the evaluative information provided by *I AM* → *PLEASANT* had been consciously appraised.

If one insists that the observed outcomes were driven by direct correlations with current self-esteem levels, it must also be granted that the majority of participants in the active group of Experiment 3 held more positive self-views compared to all remaining participant groups. This is statistically less probable than the assumption that individuals with both positive and negative self-views were similarly distributed among the groups. In support for the latter, recall that affective shift scores were shown to be significantly and positively correlated with explicit self-esteem, paralleling earlier claims (Grumm et al., 2009). We also noted that affective states did not statistically vary between participant groups prior to conditioning. This indirectly implies that collective self-esteem levels may have also been fairly similar between groups. So, even if some participants entered the study with low self-esteem levels that impacted their performance, we see no evidence to suggest that these individuals were disproportionately represented in any specific group.

Future replications could consider screening participants based on self-esteem levels and other relevant traits to further control for dispositional differences on subliminally conditioned effects.

Lastly, one may point out that our earlier claims of 'contraindicative' propositions being elicited by the conscious appraisal was not currently measured. Without having checked for strategies participants deployed when reporting their moods, any claims about top-down interference moderating observed outcomes are speculative. Our response comprises of two parts: first, had we asked participants to explicate their mood evaluation strategies, this could have prompted propositions that might otherwise have remained unspecified, potentially introducing a confound (Strohmetz, 2008). Relatedly, asking participants to describe their strategy 'after' providing mood evaluations would generate.

global justifications that would not differentiate between strategies motivating individual mood evaluations. In summary, we did not implement in vivo strategy checks to avoid cueing demand characteristics, and we excluded post-task checks as these would reflect aggregated strategies (Amd, 2022).

5. Conclusion

Across the first experiment of the current investigation, we found reliable evidence for positive affective shifts when subliminally presented self-referential terms were followed by visible and positively valenced attributes [*I AM* → *PLEASANT*]. When both terms and attributes were visible, reliable shifts were observed across explicit self-esteem across our third experiment. These outcomes add nuance to earlier claims of subliminally conditioned self-esteem by Dijksterhuis (2004) and others (Baccus et al., 2004; Grumm et al., 2009) by highlighting the differential impact of stimulus valence on self-referents which are consciously versus non-consciously encoded. We emphasize that the 'self-referent' was *non-consciously* perceived, at least across Experiments 1 and 2, where the CS was presented under visual conditions known to impede conscious identification (Amd and Passarelli, 2020). The acquisition and operation of 'non-conscious' (non-propositional) relations was rendered concordant with a direct realist ontology (Amd, 2022).

A compelling extension of our work could explore the extent to which subliminally conditioned affect remains salient over time using, say, experience-sampling methods similar to those described in Amd et al. (2019). There is some evidence which suggests that subliminal conditioning of action-related concepts can lead to lasting changes in decision-making (Ruch et al., 2016), smoking (Arzi et al., 2014; Palmatier, 1980), and even dishwashing (Di Domenico et al., 2016) behaviors, independent of participants' conscious awareness of the motivations underlying their behavioral shifts (Elgendi et al., 2018). If future iterations of our procedure can be observed to demonstrate a lasting positive impact on affective states, this could potentially produce beneficial downstream effects, such as reduced cortisol levels and improved cardiovascular health (Dockray and Steptoe, 2010). Future studies may benefit from exploring these questions by adapting the conditioning parameters we've outlined here. Considering the relative simplicity of deploying these conditioning procedures (i.e., as freely available HTML files accessible to anyone with an internet browser), a conditioning intervention that can measurably and positively influence markers of physiological health would have considerable societal benefits, especially for marginalized population clusters with limited access to mental health resources (McKenzie et al., 2004).

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Declaration of Competing Interest

There are no conflict of interests to declare.

Data availability

I have provided the OSF link to access the data and analysis script.

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