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EXPLORING THE CONTEXTUAL RENEWAL OF CONDITIONED ATTITUDES AFTER COUNTERCONDITIONING

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Research on contextualized attitude change suggests that, even when counterattitudinal information effectively influences evaluations in the context in which this information was learned, previously formed attitudes sometimes continue to determine evaluations in any other context (*contextual renewal*). Expanding on evidence for contextual renewal in attitude change based on verbal information, five experiments tested the emergence of contextual renewal in evaluative conditioning, involving pairings of a conditioned stimulus with a valenced unconditioned stimulus. Counter to the notion of contextual renewal, counterconditioning changed initially conditioned attitudes to the same extent irrespective of the context. Verbal information presented with the same procedural parameters produced contextual renewal effects only when evaluations were not measured between the formation of initial attitudes and the learning of counterattitudinal information. The results suggest two previously unidentified boundary conditions of contextualized attitude change that need to be reconciled with extant theories of evaluative learning.

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A central question in attitude research concerns the factors that change people's evaluation of an object and whether such changes are stable over time (Petty & Cacioppo, 1986). For example, concerns have been raised that the effects of many interventions to reduce racially biased attitudes are rather short-lived (Lai et al., 2016). Another important, yet frequently ignored, question is whether intervention-related changes in the evaluation of an object generalize across contexts (Gawronski & Cesario, 2013). Research on contextualized attitude change suggests that, even when counterattitudinal information effectively determines evaluations in the context in which this information was learned, previously formed attitudes sometimes continue to determine evaluations in other contexts (for a review, see Gawronski et al., 2018).

Past research on contextualized attitude change has mainly focused on attitude change in response to verbal information. Another route to achieve attitude change is evaluative conditioning (EC), involving pairings of a conditioned stimulus (CS) with a valenced unconditioned stimulus (US; De Houwer, Thomas, & Baeyens, 2001). To date, the extent to which EC shows similar patterns of contextualized attitude change is still unclear. Extant theories suggest that contextualized attitude change is driven by enhanced attention to context during the encoding of expectancy-violating information (see Gawronski et al., 2018; Ogallar, Ramos-Álvarez, Alcalá, Moreno-Fernández, & Rosas, 2017). However, there is conflicting evidence regarding the role of conscious expectancies in EC. To the extent that EC effects can be independent of conscious expectancies, the patterns of contextualized attitude change produced by verbal information may not generalize to attitude change via EC. Yet, to the extent that conscious expectancies play a mediating role in EC, the patterns of contextualized attitude change produced by verbal information should also emerge in attitude change via EC. The main goal of the current work was to test these competing predictions.

CONTEXTUALIZED ATTITUDE CHANGE

Research on contextualized attitude change has been inspired by the notion of *contextual renewal*, which refers to the context-dependent recurrence of an initially learned response after successful learning of a new response (Bouton, 2004). Applied to research on attitude change, contextual renewal occurs when counterattitudinal information about an object effectively determines evaluations of that object in the context in which the counterattitudinal information was learned while initially formed attitudes continue to determine evaluations of the object in other contexts (Gawronski et al., 2018). *ABA renewal* refers to cases in which an initial attitude is formed in Context A, counterattitudinal information is successfully learned in a different Context B, and the initially formed attitude continues to determine evaluations when the target is encountered in the initial Context A. *ABC renewal* refers to cases in which an initial attitude is formed in Context A, counterattitudinal information is successfully learned in a different Context B, and the initial attitude continues to determine evaluations in a novel Context C in which the target has not been encountered before. Thus, in cases involving both ABA and ABC renewal, the counterattitudinal information determines evaluations only in Context B, but not in Context A and Context C, thereby reflecting a pattern of contextualized attitude change (see Figure 1).

In the first demonstration of contextualized changes in social attitudes, Rydell and Gawronski (2009) presented participants with verbal statements about a target person named Bob. In a first block of the learning task, participants were shown either positive or negative statements about Bob against a meaningless colored background (e.g., yellow). In a second block, participants were presented with new information about Bob that was evaluatively opposite to the information presented in the first block, and this information was presented against a different colored background (e.g., blue). After each of the two learning blocks, evaluative responses to Bob were measured against the background color of the first learning block (Context A), the background color of the second learning block (Context B), and a novel colored background (e.g., green) that was not part of the learning task (Context C). Results showed that evaluations of Bob changed in response to the counterattitudinal information only when Bob was presented against the background color of the second learning block. In contrast, evaluations reflected

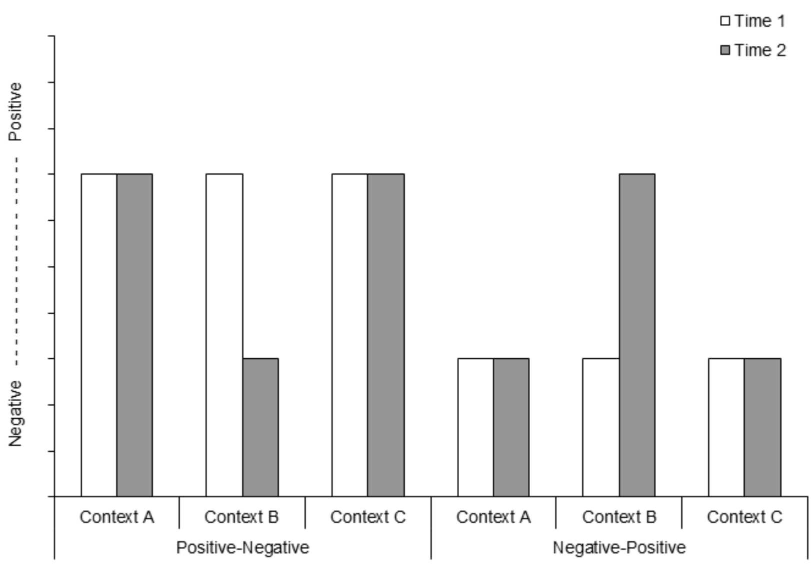

FIGURE 1. Hypothetical pattern of contextual renewal effects as a function of valence order (positive-negative vs. negative-positive), time (Time 1 vs. Time 2), and context (Context A vs. Context B vs. Context C). Context A refers to the context of initial attitudinal learning; Context B refers to the context of subsequent counterattitudinal learning; Context C refers to a novel context in which the target object has not been encountered before. Higher scores indicate more positive evaluations. ABA renewal is reflected in unchanged evaluations from Time 1 to Time 2 in Context A despite effective change in Context B. ABC renewal is reflected in unchanged evaluations in Context C from Time 1 to Time 2 despite effective change in Context B.

the valence of the initial attitudinal information when Bob was presented against the background color of the first learning block (ABA renewal) and a novel background color that was not part of the learning task (ABC renewal). Although average effect sizes of ABA and ABC renewal tend to be relatively small (Gawronski, Hu, Rydell, Vervliet, & De Houwer, 2015), contextualized attitude change has been found to be a reliable phenomenon that occurs on measures of spontaneous and deliberate evaluations (Gawronski, Hu et al., 2015; Hutchings et al., 2020), replicates in samples from Western and Eastern cultures (Ye, Tong, Chiu, & Gawronski, 2017), and is unaffected by individual differences in responses to belief-incongruent information, including individual differences in the preference for consistency, need for structure, and lay theories of personality (Brannon & Gawronski, 2018a).

To account for patterns of contextualized attitude change, Gawronski, Rydell, Vervliet, and De Houwer (2010) argued that attention to context is typically low during the encoding of initial attitudinal information about a target object (see Gilbert & Malone, 1995). Conversely, exposure to expectancy-violating information about a target object enhances attention to the context, which leads to an integration of the context into the representation of the expectancy-violating information (see Roese & Sherman, 2007). According to Gawronski and colleagues (2010), this difference in attention to context leads to a dual representation of the target object, including (1) a context-free representation of initially acquired attitudinal information and (2) a contextualized representation of expectancy-violating counterattitudinal information (see also Ogallar et al., 2017). As a result, activation of the counterattitudinal information is limited to the context in which the counterattitudinal information has been learned, allowing initial attitudinal information to shape evaluative responses in any other context. These assumptions are consistent with research showing that (1) attention to incidental contexts is higher during the encoding of attitude-incongruent than attitude-congruent information (Brannon & Gawronski, 2018b; Brannon, Sacchi, & Gawronski, 2017; Gawronski, Ye, Rydell, & De Houwer, 2014) and (2) experimental manipulations of attention to context moderate ABA and ABC renewal in a manner that is consistent with the predictions of the proposed account (Gawronski et al., 2010).

CONTEXTUAL RENEWAL IN EC?

Although several studies have demonstrated contextual renewal effects in attitude change based on verbal information (e.g., Brannon & Gawronski, 2018a; Gawronski et al., 2010, 2014; Rydell & Gawronski, 2009; Hutchings et al., in press; Ye et al., 2017; for a notable exception, see Brannon & Gawronski, 2017), it is still unclear whether similar effects occur for other instances of evaluative learning. An interesting candidate in this regard is EC, which is defined as the change in the evaluation of a CS due to its pairing with a valenced US (De Houwer, 2007). The mechanisms underlying EC are still under debate (see Bar-Anan & Balas, 2018; Hofmann, De Houwer, Perugini, Baeyens, & Crombez, 2010), and there is mixed evidence regarding whether EC-related changes in liking depend on conscious expectancies, which appear to be critical for contextualized attitude change (see Gawronski et al., 2018).

Some evidence suggests that EC-related changes in liking can be independent of conscious expectancies (for reviews, see De Houwer et al., 2001; Walther, Nagengast, & Trasselli, 2005). Although pairings of a neutral CS (e.g., bell) and a valenced US (e.g., electric shock, palatable food) may give rise to the expectancy that the CS will be followed by the US (*expectancy learning*), the same CS-US pairings may influence evaluations of the CS independent of conscious expectancies (*evaluative learning*). For example, repeated pairings of Nescafe with George Clooney in commercial advertisements may elicit a positive response to packages of Nescafe in a grocery store, but the pairings may not lead to an expectation of seeing George Clooney in the coffee aisle of the grocery store. Although the theoretical implications of differences between conditioned expectancies and conditioned liking are still under debate (Aust, Haaf, & Stahl, 2019; De Houwer, Mattavelli, & Van Desssel, 2019), there is sufficient evidence to treat the two as functionally distinct phenomena. For example, whereas expectancy learning based on CS-US pairings occurs only when the CS precedes the US (i.e., forward conditioning), evaluative learning based on CS-US pairings also occurs when the CS follows the US (i.e., backward conditioning; see Kim, Sweldens, & Hütter, 2016). Thus, to the extent that (1) CS-US pairings can influence evaluative responses independent of conscious expectancies and (2) contextual renewal effects are driven by enhanced attention to context during the encoding of expectancy-violating information, EC may not show patterns of contextual renewal (see Gawronski et al., 2018). Instead, evaluative counterconditioning may effectively change initially conditioned attitudes regardless of the context (for related evidence, see Baeyens, Crombez, De Houwer, & Eelen, 1996; Baeyens, Hendricks, Crombez, & Hermans, 1998).

In contrast to research suggesting that EC effects are independent of conscious expectancies, other research suggests that conscious expectancies might play a mediating role in EC-related changes in (dis)liking. For example, some studies found attenuated EC effects when participants had been verbally informed that a CS would no longer be followed by the US it had been paired with in a prior conditioning task (e.g., Gast & De Houwer, 2013; but see Lipp, Mallan, Libera, & Tan, 2010). Thus, to the extent that (1) CS-US pairings influence evaluative responses via learned expectancies and (2) contextual renewal effects are driven by enhanced attention to context during the encoding of expectancy-violating information, EC may show the same pattern of contextual renewal obtained for verbal information. That is, evaluative counterconditioning may change initially conditioned attitudes only in the context in which counterconditioning occurred, and initially conditioned attitudes may continue to determine evaluations in any other context (for related evidence, see Hardwick & Lipp, 2000).

Expanding on the presumed contribution of expectancy violations to contextual renewal effects and the conflicting assumptions about the role of conscious expectancies in EC, the main goal of the current research was to investigate whether contextual renewal effects occur for counterconditioning of initially conditioned attitudes. Experiment 1 provided an initial test of contextual renewal effects in EC. Experiment 2 investigated whether contextual renewal effects in EC depend on the duration of CS-US pairings, assuming that longer CS-US pairings may facilitate the generation of conscious expectancies. Experiments 3–5 directly compared contextual renewal effects in attitude change based on verbal information to the effects obtained in an EC paradigm using the same procedural parameters. For all studies reported here, the data were collected in one shot without prior statistical analyses. We report all data exclusions, all measures, and all manipulations. All materials, raw data, and analysis files are available at https://osf.io/a6sez/.

EXPERIMENT 1

Experiment 1 provided an initial test of contextual renewal effects in EC. Toward this end, participants were presented with CS-US pairings against a meaningless colored background (e.g., yellow). After completion of the initial EC task, participants completed a counterconditioning task in which the CSs of the initial EC task were paired with new USs that were evaluatively opposite to the USs in the first block. The CS-US pairings of the counterconditioning task were presented against a colored background that was different from the one of the initial EC task (e.g., blue). After each of the two conditioning tasks, evaluative responses to the CSs were measured against the background color of the initial EC task (Context A), the background color of the counterconditioning task (Context B), and a novel background that was not part of either of the conditioning tasks (Context C). The main question was whether initially conditioned attitudes continue to determine CS evaluations in Context A and Context C even when counterconditioning effectively changes CS evaluations in Context B.

METHODS

Participants and Design. A total of 106 undergraduate students at the University of Texas at Austin were recruited for a one-hour battery that included the present experiment and one additional study that was unrelated to this experiment.[1] Participants received research credit for an introductory psychology course. Due to computer malfunctions, data from three participants were lost, leaving us with a final sample of 103 participants (78 women, 25 men, M_{age} = 19.23, SD_{age} = 1.46). The study included a 2 (Valence-Order: positive-negative vs. negative-positive) × 2 (Measurement Time: time 1 vs. time 2) × 3 (Context: Context A vs. Context B vs. Context C) within-subjects design.

1. The sample size was determined prior to data collection to include 100 participants. Because G*Power (Faul, Erdfelder, Lang, & Buchner, 2007) does not permit power estimates for the multi-factorial within-subjects design of Experiment 1 and we were unable to find alternative tools to obtain power estimates, the sample size was determined in a heuristic fashion based on hypotheses-relevant post-hoc tests. A sample of 100 participants provides a power of 80% in detecting a small EC effect of dz = 0.28 at Time 1 (two-tailed), a power of 80% in detecting a small change of dz = 0.28 in the size of EC effects from Time 1 to Time 2 (two-tailed), and a power of 80% in detecting a small difference of f = 0.134 in evaluations across contexts at Time 2 (two-tailed). Based on meta-analytic data by Gawronski, Hu, and colleagues (2015), the power analysis for context effects at Time 2 assumed a correlation between measures of r = .45.

Procedure. The study included four components in the following order: (1) an initial conditioning task, (2) a Time-1 measure of speeded evaluations, (3) a counterconditioning task, and (4) a Time-2 measure of speeded evaluations. In the initial conditioning task, participants were presented with repeated pairings of neutral CSs with positive or negative USs against a particular background color (e.g., yellow). In the counterconditioning task, participants were presented with CS-US pairings of the opposite valence against a background color that was different from the one in the initial conditioning task (e.g., blue). After each conditioning task, participants completed a speeded evaluation task designed to measure rapid evaluative responses to the CSs against the background color of the initial conditioning task (Context A), the background color of the counterconditioning task (Context B), and a novel background color that was not presented during either of the two conditioning tasks (Context C).

Materials. As CSs, we used eight computer-generated images of shapes with different color patterns (300 × 215 pixels) from Gawronski, Mitchell, and Balas (2015). As USs, we used 16 positive and 16 negative images (600 × 450 pixels) from the International Affective Picture System (Lang, Bradley, & Cuthbert, 2008). Half of the US images were used in the initial conditioning task (8 positive, 8 negative); the other half were used in the counterconditioning task (8 positive, 8 negative). The assignment of the US images to the initial conditioning task and the counterconditioning task was counterbalanced across participants.

Evaluative Conditioning. Participants were told that they would be presented with different kinds of images, including computer-generated drawings and real-world photographs. Participants were further told that we would ask them a number of questions about the pictures later in the study and that they should pay close attention throughout the task. In the initial conditioning task, 4 CS images were paired with positive USs and 4 CS images were paired with negative USs. In the counterconditioning task, the valence of the CS-US pairings was reversed. The use of a given CS for pairings with a positive versus negative US in the initial conditioning task was counterbalanced across participants. The procedural parameters were identical in the two conditioning tasks. On each trial, a CS-US pair was presented simultaneously on the screen for 1000 ms. The USs were presented slightly above and the CSs slightly below the center of the screen. The inter-trial interval was 2000 ms. For half of the participants, the background color was blue during the initial conditioning task and yellow during the counterconditioning task. For the remaining half, the color mapping was reversed. Each CS-US pair was presented 10 times in each of the two conditioning tasks, summing up to a total of 80 trials per task.

Speeded Evaluation Task. The speeded evaluation task was adopted from earlier research on contextual renewal effects by Gawronski and colleagues (2014; see Ranganath, Smith, & Nosek, 2008). The task included brief presentations of the CS images against the background color of the initial conditioning task (Context

A), the background color of the counterconditioning task (Context B), and a new background color (i.e., green) that was not presented during either conditioning task (Context C). Participants were instructed to press a right-hand key (*Numpad 5*) if their immediate gut response was positive and a left-hand key (*A*) if their immediate gut response was negative. The key assignment was identical for all participants. Participants were told that they have only one second to provide their response. Each trial started with a fixation cross which was displayed for 500 ms against a black background in the center of the screen. The fixation cross was followed by the presentation of one of the CS images against one of the three backgrounds for 100 ms, which was followed by a black screen for 100 ms. Participants were then prompted by a question mark in the center of a black screen to indicate whether their immediate "gut" response to the presented stimulus was positive or negative. If participants did not respond within 1000 ms after the onset of the target image, the message *Please try to respond faster!* was presented for 2000 ms on the screen. The speeded evaluation task included 4 trials for each of the 8 CSs against each of the 3 colored backgrounds, summing up to a total of 96 trials. Participants were asked to complete the speeded evaluation task after the initial conditioning task (Time 1) and again after the counterconditioning task (Time 2).

RESULTS

Responses on the speeded evaluation task were aggregated by calculating the mean proportion of *positive* responses for the six types of target stimuli implied by the manipulation of US valence in the initial conditioning task (i.e., CSs initially paired with a positive US vs. CSs initially paired with a positive US) and background color in the speeded evaluation task (i.e., background color of initial conditioning task vs. background color of counterconditioning task vs. novel background color) for each of the two measurement points (i.e., after initial conditioning vs. after counterconditioning). Higher scores on this index reflect more favorable responses to a given CS in the respective context. Submitted to a 2 (Valence Order) × 3 (Context) × 2 (Measurement Time) ANOVA for repeated measures, evaluation scores revealed a significant main effect of Valence Order, $F(1, 102) = 5.15$, $p = .025$, $\eta_p^2 = .048$, and a significant main effect of Measurement Time, $F(1, 102) = 6.33$, $p = .013$, $\eta_p^2 = .058$, which were qualified by a significant two-way interaction between Valence Order and Measurement Time, $F(1, 102) = 205.85$, $p < .001$, $\eta_p^2 = .669$ (see Figure 2). This interaction indicated that, at Time 1, CSs paired with positive USs in the initial conditioning task (and negative USs in the counterconditioning task) were evaluated more favorably compared to CSs paired with negative USs in the initial conditioning task (and negative USs in the counterconditioning task), $F(1, 102) = 120.08$, $p < .001$, $\eta_p^2 = .541$. Conversely, at Time 2, CSs paired with positive USs in the initial conditioning task (and negative USs in the counterconditioning task) were evaluated less favorably compared to CSs paired with negative USs in the initial conditioning task (and negative USs in the counterconditioning task), $F(1, 102) = 144.12$, $p < .001$, $\eta_p^2 = .586$. Moreover, CSs paired with positive USs in the initial conditioning task (and negative USs in the counterconditioning task) were evaluated more

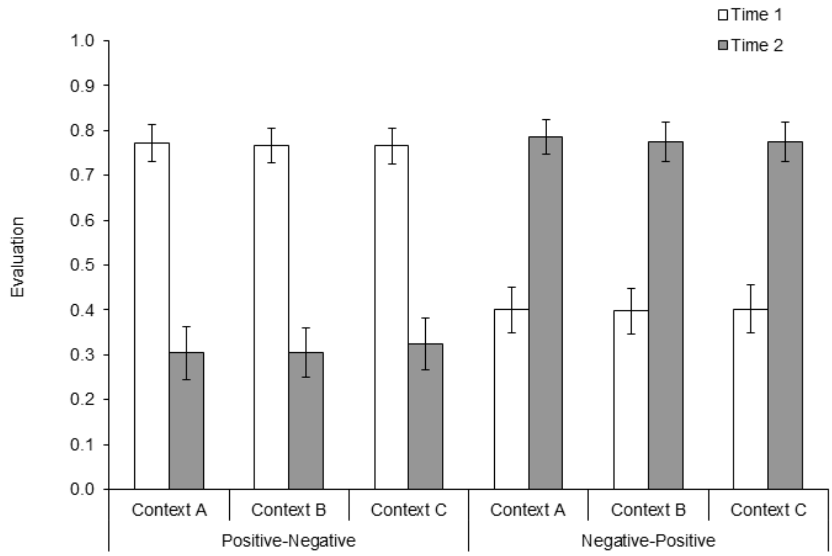

FIGURE 2. Evaluative responses to target stimuli as a function of valence order (positive-negative vs. negative-positive), time (Time 1 vs. Time 2), and context (Context A vs. Context B vs. Context C), Experiment 1. Higher scores indicate more positive evaluations. Error bars depict 95% confidence intervals.

favorably at Time 1 compared to Time 2, $F(1, 102) = 183.36$, $p < .001$, $\eta_p^2 = .643$. Conversely, CSs paired with negative USs in the initial conditioning task (and positive USs in the counterconditioning task) were evaluated less favorably at Time 1 compared to Time 2, $F(1, 102) = 137.79$, $p < .001$, $\eta_p^2 = .575$.

Importantly, the two-way interaction between Valence Order and Measurement Time was not qualified by a higher-order interaction with Context, $F(2, 204) = 1.16$, $p = .316$, $\eta_p^2 = .011$. Moreover, the two-way interaction between Valence Order and Measurement Time was statistically significant regardless of whether CS evaluations were measured against the background of the initial conditioning task (Context A), $F(1, 102) = 211.49$, $p < .001$, $\eta_p^2 = .675$, the background of the counterconditioning task (Context B), $F(1, 102) = 198.21$, $p < .001$, $\eta_p^2 = .660$, or a novel background (Context C), $F(1, 102) = 172.90$, $p < .001$, $\eta_p^2 = .629$. Further analyses revealed that counterconditioning with negative USs significantly reduced favorable CS evaluations from Time 1 to Time 2 regardless of whether CS evaluations were measured against the background of the initial conditioning task (Context A), $t(102) = 12.95$, $p < .001$, $d = 1.297$, the background of the counterconditioning task (Context B), $t(102) = 13.69$, $p < .001$, $d = 1.373$, or a novel background (Context C), $t(102) = 12.16$, $p < .001$, $d = 1.217$. Conversely, counterconditioning with positive USs significantly increased favorable CS evaluations from Time 1 to Time 2 regardless of whether CS evaluations were measured against the background of the initial conditioning task (Context A), $t(102) = 11.83$, $p < .001$, $d = 1.175$, the background of the counterconditioning task (Context B), $t(102) = 10.92$, $p < .001$, $d = 1.079$, or a novel background (Context C), $t(102) = 11.08$, $p < .001$, $d = 1.099$.

Together, these findings suggest that, counter to the notion of contextual renewal, counterconditioning effectively reversed initially conditioned attitudes regardless of the context.

DISCUSSION

Different from earlier research showing contextual renewal effects in attitude change based on verbal information (for a meta-analysis, see Gawronski, Hu et al., 2015), Experiment 1 found no evidence for contextual renewal effects in EC. In the current study, counterconditioning reversed initially conditioned attitudes to the same extent irrespective of the context. Although conclusions from the obtained null effects of context should be treated with caution in the absence of further evidence, these findings are consistent with the argument that CS-US pairings can influence evaluative responses independent of conscious expectancies (see De Houwer et al., 2001; Walther et al., 2005). Thus, to the extent that contextual renewal effects are driven by enhanced attention to context during the encoding of expectancy-violating information (Gawronski et al., 2018), counterconditioning should be effective in changing initially conditioned attitudes regardless of the context.

EXPERIMENT 2

Although the available evidence suggests that EC-related changes in the (dis)liking of a CS can be independent of conscious expectancies (e.g., De Houwer et al., 2019; Lipp et al., 2010), it seems reasonable to assume that conscious expectancies may contribute to EC effects under certain conditions (e.g., Gast & De Houwer, 2013). From this perspective, a central question concerns the conditions under which conscious expectancies may lead to contextual renewal effects in EC. One such condition might be the available time to process CS-US pairings.[2] Assuming that the generation of conscious expectancies requires sufficient cognitive resources (Gawronski & Bodenhausen, 2014, 2018), contextual renewal effects may be more likely to occur when the available time to process CS-US pairings is long than when it is short. Based on these considerations, Experiment 2 investigated the emergence of contextual renewal effects in EC under conditions of short versus long CS-US presentations.

METHODS

Participants and Design. A total of 196 undergraduate students at the University of Texas at Austin were recruited for a one-hour battery that included the present

2. There are presumably multiple other factors that influence the contribution of conscious expectancies to EC effects. In the current study, we focused on processing time as one such factor, because it is easy to manipulate during the encoding of CS-US pairings, which avoids ambiguities in the interpretation of correlations with recollective memory measures administered after encoding (see Gawronski & Walther, 2012).

experiment and one additional study that was unrelated to this experiment.[3] Participants received research credit for an introductory psychology course. Due to a computer malfunction, data from one participant were lost, leaving us with a final sample of 195 participants (119 women, 75 men, 1 missing, M_{age} = 19.19, SD_{age} = 2.46). The study included a 2 (Valence-Order: positive-negative vs. negative-positive) × 2 (Measurement Time: time 1 vs. time 2) × 3 (Context: Context A vs. Context B. vs. Context C) × 2 (Presentation Time: 1000 ms vs. 3000 ms) mixed design with the last variable as a between-subjects factor and the other three as within-subjects factors.

Procedure. The procedure of Experiment 2 was identical to Experiment 1, the only difference being that we additionally manipulated the presentation times for the CS-US pairings in the two conditioning tasks. For half of the participants, the CS-US pairings were presented for 1000 ms, as in Experiment 1. For the remaining half, the CS-US pairings were presented for 3000 ms. Although there is no theoretical basis for a priori classifications of CS-US pairings as short vs. long, we assumed that a three-fold increase of the presentation times in Experiment 1 would provide a much greater opportunity to generate expectancies about CS-US pairings.

RESULTS

Responses on the speeded evaluation task were aggregated in line with the procedures in Experiment 1. Submitted to a 2 (Valence Order) × 3 (Context) × 2 (Measurement Time) × 2 (Presentation Time) mixed ANOVA, evaluation scores revealed a significant main effect of Measurement Time, $F(1, 193) = 18.56$, $p < .001$, $\eta_p^2 = .088$, and a significant two-way interaction effect of Valence Order and Presentation Time, $F(1, 193) = 13.60$, $p < .001$, $\eta_p^2 = .066$. More important for the current investigation, there was a significant two-way interaction between Valence Order and Measurement Time, $F(1, 193) = 377.67$, $p < .001$, $\eta_p^2 = .662$, which replicated the main finding of Experiment 1 (see Figures 3a and 3b). Specifically, at Time 1, CSs paired with positive USs in the initial conditioning task (and negative USs in the counterconditioning task) were evaluated more favorably compared to CSs paired with negative USs in the initial conditioning task (and negative USs in the counterconditioning task), $F(1, 193) = 340.72$, $p < .001$, $\eta_p^2 = .638$. Conversely, at Time 2, CSs paired with positive USs in the initial conditioning task (and negative USs in the counterconditioning task) were evaluated less favorably compared to CSs paired with negative USs in the initial conditioning task (and negative USs in the counterconditioning task), $F(1, 193) = 222.35$, $p < .001$, $\eta_p^2 = .535$. Moreover, CSs paired with positive USs in the initial conditioning task (and negative USs in the counterconditioning task) were evaluated more favorably at Time 1 compared to Time

3. The sample size was determined prior to data collection to include approximately 200 participants. Because G*Power (Faul et al., 2007) does not permit power estimates for the mixed-factorial design of Experiment 2 and we were unable to find alternative tools to obtain power estimates, the sample size was determined in a heuristic fashion by doubling the sample size of Experiment 1 to compensate for the additional between-subjects manipulation of encoding time.

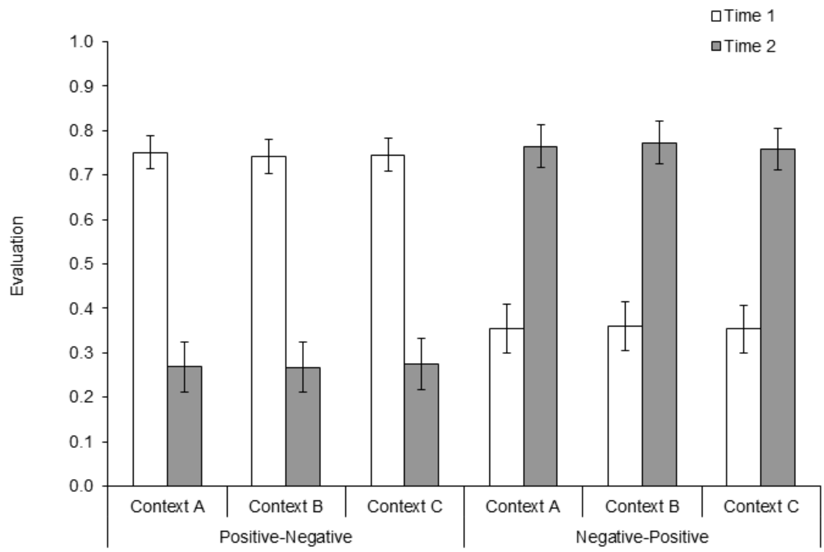

FIGURE 3a. Evaluative responses to target stimuli as a function of valence order (positive-negative vs. negative-positive), time (Time 1 vs. Time 2), and context (Context A vs. Context B vs. Context C) for short CS-US pairings (1000 ms), Experiment 2. Higher scores indicate more positive evaluations. Error bars depict 95% confidence intervals.

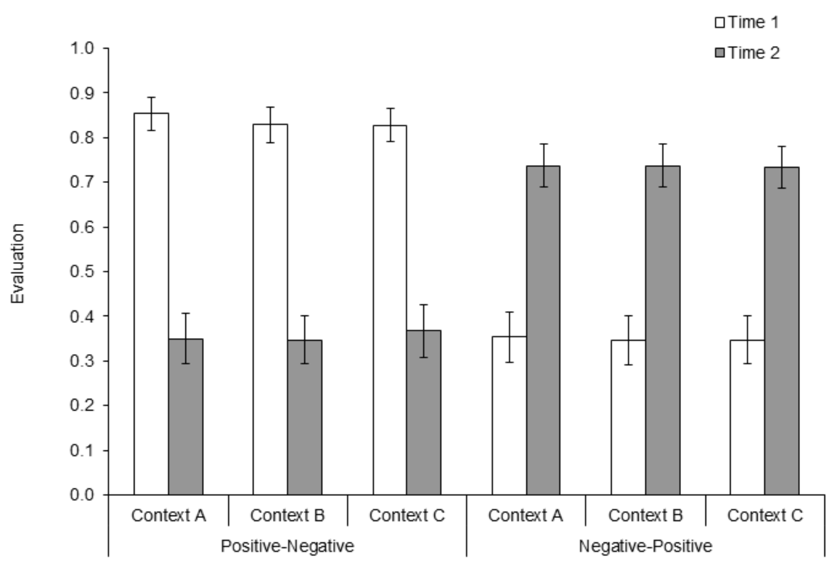

FIGURE 3b. Evaluative responses to target stimuli as a function of valence order (positive-negative vs. negative-positive), time (Time 1 vs. Time 2), and context (Context A vs. Context B vs. Context C) for long CS-US pairings (3000 ms), Experiment 2. Higher scores indicate more positive evaluations. Error bars depict 95% confidence intervals.

2, $F(1, 193) = 389.36$, $p < .001$, $\eta_p^2 = .669$. Conversely, CSs paired with negative USs in the initial conditioning task (and positive USs in the counterconditioning task) were evaluated less favorably at Time 1 compared to Time 2, $F(1, 193) = 262.26$, $p < .001$, $\eta_p^2 = .576$.

As in Experiment 1, the two-way interaction between Valence Order and Measurement Time was not qualified by a higher-order interaction with Context, $F(2, 386) = 1.51$, $p = .223$, $\eta_p^2 = .008$. There was also no significant four-way interaction between Valence Order, Measurement Time, Context, and Presentation Time, $F(2, 386) = 0.26$, $p = .769$, $\eta_p^2 = .001$. The two-way interaction between Valence Order and Measurement Time was statistically significant regardless of whether CS-US pairings were presented for 1000 ms, $F(1, 97) = 188.43$, $p < .001$, $\eta_p^2 = .660$, or 3000 ms, $F(1, 97) = 189.39$, $p < .001$, $\eta_p^2 = .664$. Moreover, the three-way interaction between Valence Order, Measurement Time, and Context was not statistically significant regardless of whether CS-US pairings were presented for 1000 ms, $F(2, 194) = 0.26$, $p = .775$, $\eta_p^2 = .003$, or 3000 ms, $F(2, 192) = 1.57$, $p = .210$, $\eta_p^2 = .016$.

Further analyses revealed that the two-way interaction between Valence Order and Measurement Time was statistically significant regardless of whether CS evaluations were measured against the background of the initial conditioning task (Context A), $F(1, 194) = 375.90$, $p < .001$, $\eta_p^2 = .660$, the background of the counterconditioning task (Context B), $F(1, 194) = 359.83$, $p < .001$, $\eta_p^2 = .650$, or a novel background (Context C), $F(1, 194) = 351.98$, $p < .001$, $\eta_p^2 = .645$. Moreover, counterconditioning with negative USs significantly reduced favorable CS evaluations from Time 1 to Time 2 regardless of whether CS evaluations were measured against the background of the initial conditioning task (Context A), $t(194) = 19.63$, $p < .001$, $d = 1.430$, the background of the counterconditioning task (Context B), $t(194) = 19.05$, $p < .001$, $d = 1.378$, or a novel background (Context C), $t(194) = 18.17$, $p < .001$, $d = 1.329$. Conversely, counterconditioning with positive USs significantly increased favorable CS evaluations from Time 1 to Time 2 regardless of whether CS evaluations were measured against the background of the initial conditioning task (Context A), $t(194) = 15.06$, $p < .001$, $d = 1.080$, the background of the counterconditioning task (Context B), $t(194) = 15.56$, $p < .001$, $d = 1.117$, or a novel background (Context C), $t(194) = 15.88$, $p < .001$, $d = 1.140$. Together, these findings suggest that (1) counterconditioning effectively reversed initially conditioned attitudes regardless of the context, and (2) this context-independent reversal replicated regardless of whether CS-US pairings were presented for short or long intervals.

DISCUSSION

Counter to the hypothesis that contextual renewal effects in EC may depend on the time to process CS-US pairings, Experiment 2 found no evidence for contextual renewal effects in EC regardless of whether CS-US pairings were presented for short or long durations. Although these findings raise important questions about the conditions under which conscious expectancies contribute to EC effects, they provide further evidence for the hypothesis that counterconditioning may change initially conditioned attitudes to the same extent irrespective of the context.

EXPERIMENT 3

Together with earlier evidence for contextual renewal effects in attitude change based on verbal information (for a meta-analysis, see Gawronski, Hu et al., 2015), Experiments 1 and 2 suggest that contextual renewal effects do not occur in attitude change via EC. However, this conclusion rests on the assumption that verbal information would produce contextual renewal effects in a learning task with the same procedural parameters. The main goal of Experiment 3 was to test this assumption, allowing for direct comparisons between patterns of attitude change produced by EC and verbal information. Toward this end, half of the participants were presented with pairings of unknown faces (CSs) and affect-eliciting images (USs), equivalent to the conditioning paradigms in Experiments 1 and 2 (evaluative-conditioning group). The remaining half were presented with the same unknown faces and verbal information about positive or negative behaviors performed by the depicted individuals, using the same procedural parameters (verbal-information group). Based on earlier research on contextual renewal effects and the results of Experiments 1 and 2, we expected contextual renewal effects to emerge in the verbal-information group, but not in the evaluative-conditioning group.

METHODS

Participants and Design. A total of 481 undergraduate students at the University of Texas at Austin were recruited for a one-hour battery that included the present experiment and one additional study that was unrelated to this experiment.[4] Participants received research credit for an introductory psychology course. Due to a computer malfunction, data from one participant were lost, leaving us with a final sample of 480 participants (347 women, 133 men, $M_{age} = 18.80$, $SD_{age} = 1.21$). The study included a 2 (Valence-Order: positive-negative vs. negative-positive) × 2 (Measurement Time: time 1 vs. time 2) × 3 (Context: Context A vs. Context B. vs. Context C) × 2 (Task: evaluative conditioning vs. verbal information) mixed design with the last variable as a between-subjects factor and the other three as within-subjects factors.

Procedure. The procedure of Experiment 3 was similar to the 3000 ms condition in Experiment 2 with a few important differences. First, the current study used images of two Caucasian male faces in their mid 20s as CSs instead of computer-generated images. The two face stimuli were adopted from earlier research on contextual renewal in attitude change based on verbal information (Brannon & Gawronski, 2018a; Gawronski et al., 2014). Second, the current study additionally

4. The sample size was determined prior to data collection to include 480 participants. Because G*Power (Faul et al., 2007) does not permit power estimates for the mixed-factorial design of Experiment 4 and we were unable to find alternative tools to obtain power estimates, the sample size was determined in a heuristic fashion. To provide a stronger basis for the interpretation of null effects of context, we aimed for a larger sample of 480 participants in Experiment 3.

manipulated whether the two target faces were repeatedly paired with (1) positive or negative images (evaluative-conditioning group) or (2) statements describing positive or negative behaviors (verbal-information group). Third, to reduce the length of the evaluative learning parts, the inter-trial interval was reduced from 2000 ms to 1000 ms. The evaluative learning task in the evaluative-conditioning group included 25 presentations of each target face with the same positive or negative US. The evaluative learning task in the verbal-information group included presentations of each target face with either 25 positive or 25 negative statements adopted from Rydell and Gawronski (2009). Thus, both tasks included a total of 50 trials. The target faces were presented slightly above the center of the screen; the US images and evaluative statements were presented slightly below the center of the screen. The mapping between the target faces and valence in the evaluative learning tasks was counterbalanced across participants. The instructions for participants in the evaluative-conditioning group were identical to Experiment 1. Participants in the verbal-information group were told that the study investigates how people form first impressions of other individuals, and that they should form an impression of two individuals based on the presented information. The procedure of the speeded evaluation task was identical to Experiment 1, the only difference being that each of the two target faces was presented 10 times against each of the three backgrounds, summing up to a total of 60 trials.

RESULTS

Responses on the speeded evaluation task were aggregated in line with the procedures in Experiment 1. Submitted to a 2 (Valence Order) × 3 (Context) × 2 (Measurement Time) × 2 (Task) mixed ANOVA, evaluation scores revealed a significant main effect of Context, $F(2, 956) = 6.92$, $p = .001$, $\eta_p^2 = .014$, a significant main effect of Measurement Time, $F(1, 478) = 8.24$, $p = .004$, $\eta_p^2 = .017$, and a significant two-way interaction of Context and Task, $F(2, 956) = 7.34$, $p = .001$, $\eta_p^2 = .015$. More important for the current investigation, there was a significant two-way interaction between Valence Order and Measurement Time, $F(1, 478) = 1336.50$, $p < .001$, $\eta_p^2 = .737$, which was qualified by a significant three-way interaction between Valence Order, Measurement Time, and Task, $F(1, 478) = 248.93$, $p < .001$, $\eta_p^2 = .342$. To decompose this interaction, we conducted separate 2 (Valence Order) × 3 (Context) × 2 (Measurement Time) repeated-measures ANOVAs for the two Task conditions.

Evaluative Conditioning. For participants in the evaluative-conditioning group, the ANOVA revealed a significant main effect of Context, $F(2, 474) = 8.13$, $p < .001$, $\eta_p^2 = .033$, and a significant main effect of Measurement Time, $F(1, 237) = 5.96$, $p = .015$, $\eta_p^2 = .025$. More important for the current investigation, there was a significant two-way interaction of Valence Order and Measurement Time, $F(1, 237) = 172.79$, $p < .001$, $\eta_p^2 = .422$, which replicated the findings of Experiments 1 and 2 (see Figure 4a). Specifically, at Time 1, CSs paired with positive USs in the initial conditioning task (and negative USs in the counterconditioning task) were evaluated more favorably compared to CSs paired with negative USs in the

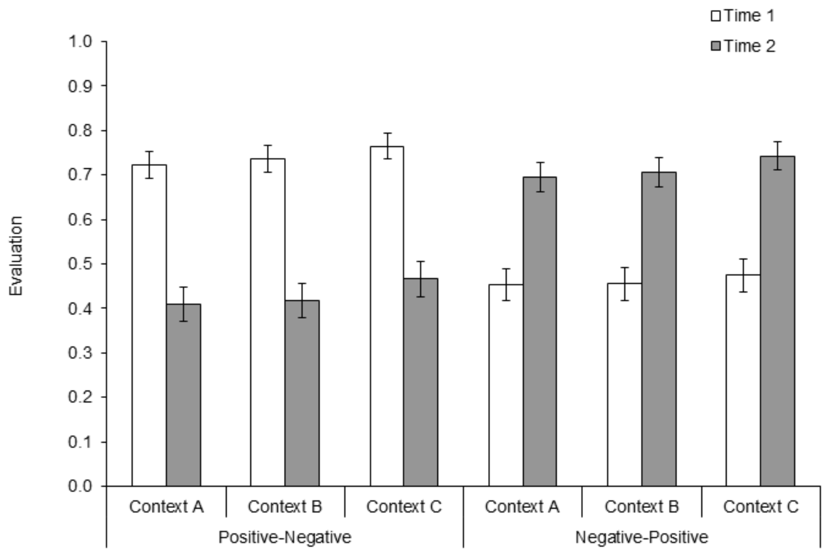

FIGURE 4a. Evaluative responses to target stimuli as a function of valence order (positive-negative vs. negative-positive), time (Time 1 vs. Time 2), and context (Context A vs. Context B vs. Context C) for evaluative conditioning, Experiment 3. Higher scores indicate more positive evaluations. Error bars depict 95% confidence intervals.

initial conditioning task (and negative USs in the counterconditioning task), $F(1, 237) = 94.50$, $p < .001$, $\eta_p^2 = .285$. Conversely, at Time 2, CSs paired with positive USs in the initial conditioning task (and negative USs in the counterconditioning task) were evaluated less favorably compared to CSs paired with negative USs in the initial conditioning task (and negative USs in the counterconditioning task), $F(1, 237) = 73.07$, $p < .001$, $\eta_p^2 = .236$. Moreover, CSs paired with positive USs in the initial conditioning task (and negative USs in the counterconditioning task) were evaluated more favorably at Time 1 compared to Time 2, $F(1, 237) = 158.72$, $p < .001$, $\eta_p^2 = .401$. Conversely, CSs paired with negative USs in the initial conditioning task (and positive USs in the counterconditioning task) were evaluated less favorably at Time 1 compared to Time 2, $F(1, 237) = 111.93$, $p < .001$, $\eta_p^2 = .321$.

Importantly, the two-way interaction between Valence Order and Measurement Time was not qualified by a higher-order interaction with Context, $F(2, 474) = 0.25$, $p = .775$, $\eta_p^2 = .001$. The two-way interaction between Valence Order and Measurement Time was statistically significant regardless of whether CS evaluations were measured against the background of the initial conditioning task (Context A), $F(1, 237) = 155.16$, $p < .001$, $\eta_p^2 = .396$, the background of the counterconditioning task (Context B), $F(1, 237) = 160.76$, $p < .001$, $\eta_p^2 = .404$, or a novel background (Context C), $F(1, 237) = 163.45$, $p < .001$, $\eta_p^2 = .408$. Further analyses revealed that counterconditioning with negative USs significantly reduced favorable CS evaluations from Time 1 to Time 2 regardless of whether CS evaluations were measured against the background of the initial conditioning task (Context A), $t(237) = 11.75$, $p < .001$, $d = 0.771$, the background of the counterconditioning task (Context B),

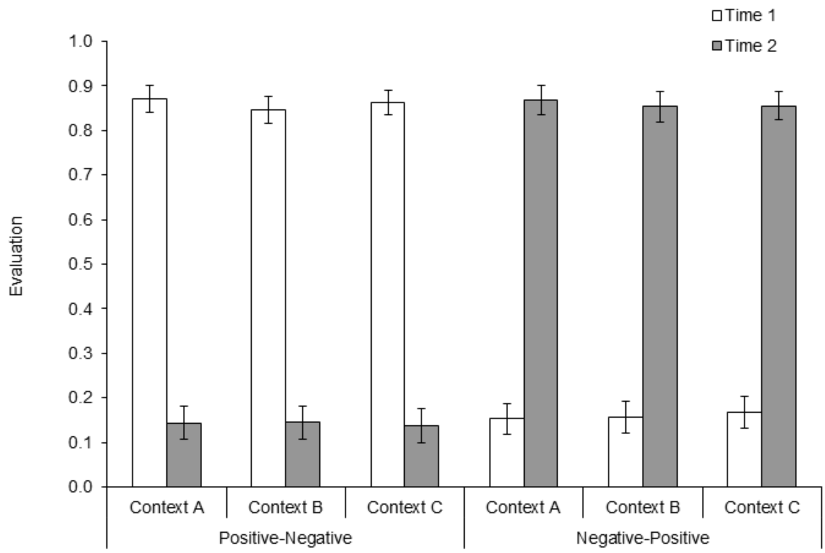

FIGURE 4b. Evaluative responses to target stimuli as a function of valence order (positive-negative vs. negative-positive), time (Time 1 vs. Time 2), and context (Context A vs. Context B vs. Context C) for verbal learning, Experiment 3. Higher scores indicate more positive evaluations. Error bars depict 95% confidence intervals.

$t(237) = 12.11$, $p < .001$, $d = 0.796$, or a novel background (Context C), $t(237) = 11.01$, $p < .001$, $d = 0.732$. Conversely, counterconditioning with positive USs significantly increased favorable CS evaluations from Time 1 to Time 2 regardless of whether CS evaluations were measured against the background of the initial conditioning task (Context A), $t(237) = 9.05$, $p < .001$, $d = 0.587$, the background of the counterconditioning task (Context B), $t(237) = 9.36$, $p < .001$, $d = 0.608$, or a novel background (Context C), $t(237) = 10.72$, $p < .001$, $d = 0.699$. Together, these findings indicate that counterconditioning effectively reversed initially conditioned attitudes regardless of the context.

Verbal Information. For participants in the verbal-information group, the ANOVA revealed a significant two-way interaction of Valence Order and Measurement Time, $F(1, 241) = 1806.03$, $p < .001$, $\eta_p^2 = .882$. This interaction replicated the pattern obtained in the evaluative-conditioning group (see Figure 4b). However, the effect sizes were substantially larger, which presumably drove the significant three-way interaction between Valence Order, Measurement Time, and Task in the omnibus ANOVA. Specifically, at Time 1, targets paired with positive statements in the first block (and negative statements in the second block) were evaluated more favorably compared to targets paired with negative statements in the first block (and negative statements in the second block), $F(1, 241) = 1272.86$, $p < .001$, $\eta_p^2 = .841$. Conversely, at Time 2, targets paired with positive statements in the first block (and negative statements in the second block) were evaluated less favorably compared to targets paired with negative statements in the first block (and negative statements in the second block), $F(1, 241) = 1198.86$, $p < .001$, $\eta_p^2 = .833$. Moreover, targets paired with positive statements in the first block (and negative statements in the second block) were evaluated more favorably at Time 1 compared to Time 2, $F(1, 241) = 1694.77$, $p < .001$, $\eta_p^2 = .876$. Conversely, targets paired with negative statements in the first block (and positive statements in the second block) were evaluated less favorably at Time 1 compared to Time 2, $F(1, 241) = 1494.76$, $p < .001$, $\eta_p^2 = .861$.

Although the three-way interaction between Valence Order, Measurement Time and Context was at the border of statistical significance, $F(2, 482) = 3.01$, $p = .050$, $\eta_p^2 = .012$, post-hoc analyses did not support the hypothesis that counterattitudinal verbal information would be more effective in changing initially formed attitudes in the context of counterattitudinal information compared to the other two contexts. None of the relevant post-hoc comparisons revealed a significant difference between CS evaluations across contexts, all *t*s < 1.65, all *p*s > .10, all *d*s < 0.107. The two-way interaction between Valence Order and Measurement Time was statistically significant regardless of whether target evaluations were measured against the background of the first block (Context A), $F(1, 241) = 1766.88$, $p < .001$, $\eta_p^2 = .880$, the background of the second block (Context B), $F(1, 241) = 1584.70$, $p < .001$, $\eta_p^2 = .868$, or a novel background (Context C), $F(1, 241) = 1610.70$, $p < .001$, $\eta_p^2 = .870$. Further analyses revealed that pairings with counterattitudinal negative statements significantly reduced favorable target evaluations from Time 1 to Time 2 regardless of whether target evaluations were measured against the background of the first block (Context A), $t(241) = 38.78$, $p < .001$, $d = 2.506$, the background of the second block (Context B), $t(241) = 36.73$, $p < .001$, $d = 2.363$, or a novel background (Context C), $t(241) = 37.24$, $p < .001$, $d = 2.398$. Conversely, pairings with counterattitudinal positive statements significantly increased favorable target evaluations from Time 1 to Time 2 regardless of whether target evaluations were measured against the background of the first block (Context A), $t(241) = 36.91$, $p < .001$, $d = 2.373$, the background of the second block (Context B), $t(241) = 35.56$, $p < .001$, $d = 2.287$, or a novel background (Context C), $t(241) = 34.09$, $p < .001$, $d = 2.192$. Thus, counter to previous evidence for contextual renewal effects in evaluative learning tasks using verbal information (e.g., Brannon & Gawronski, 2018a; Gawronski et al., 2010, 2014; Rydell & Gawronski, 2009; Ye et al., 2017), counterattitudinal information effectively reversed initially formed attitudes regardless of the context.

DISCUSSION

Counter to the prediction that contextual renewal effects should emerge in the verbal-information group, but not in the evaluative-conditioning group, neither group revealed evidence for contextual renewal effects. In the evaluative-conditioning group, counterconditioning reversed initially conditioned attitudes to the same extent irrespective of the context. Similarly, in the verbal-information group, counterattitudinal information reversed initially formed attitudes to the same extent irrespective of the context. Although the findings in the evaluative-conditioning group are consistent with the hypothesis that evaluative conditioning may be unaffected by contextual renewal, the lack of evidence for contextual renewal in the verbal-information group raises important questions about the boundary conditions of contextual renewal effects more broadly (see Gawronski, Hu et al., 2015). Experiments 4 and 5 aimed to address these questions.

EXPERIMENT 4

A potential explanation for the absence of contextual renewal effects in the verbal-information group in Experiment 3 is that the time to encode the verbal information was too short for an integration of the context into the representation of the counterattitudinal information. According to Gawronski and colleagues (2010), contextual renewal effects are driven by enhanced attention to context during the encoding of expectancy-violating information. Thus, when attentional resources are fully devoted to focal information about the target (e.g., when expectancy-violating information is presented only for a short time), a critical precondition for the integration of the context may be undermined. In such cases, expectancy-violating counterattitudinal information should change initially formed attitudes regardless of the context (for similar effects in the updating of social stereotypes in response to counterstereotypical information, see Moreno & Bodenhausen, 1999; Yzerbyt, Coull, & Rocher, 1999). Importantly, because the presentation times of CS-US pairings in Experiments 1 and 2 were as short as the presentation times in Experiment 3 (or even shorter), such an explanation would revive the original question of whether there is a genuine difference between contextual renewal effects in attitude formation and change via EC versus verbal information. Experiment 4 aimed to address these questions by increasing the presentation times from 3000 ms to 5000 ms. Based on earlier research using similar presentation times (e.g., Brannon & Gawronski, 2018a; Gawronski et al., 2010, 2014; Rydell & Gawronski, 2009; Ye et al., 2017), we expected to replicate previous evidence for contextual renewal effects in the verbal-information group. An open question was whether longer CS-US pairings would lead to contextual renewal effects in the evaluative-conditioning group, or if counterconditioning would continue to change initially conditioned attitudes to the same extent irrespective of the context, as observed in Experiments 1–3.

METHODS

Participants and Design. A total of 494 undergraduate students at the University of Texas at Austin were recruited for a one-hour battery that included the present experiment and one additional study that was unrelated to this experiment.[5] Participants received research credit for an introductory psychology course. Due to a computer malfunction, data from two participants were lost, leaving us with

5. The sample size was determined prior to data collection to include 480 participants. Because G*Power (Faul et al., 2007) does not permit power estimates for the mixed-factorial design of Experiment 4 and we were unable to find alternative tools to obtain power estimates, the sample size was determined in a heuristic fashion based on the sample size in Experiment 3.

a final sample of 492 participants (354 women, 138 men, M_{age} = 18.87, SD_{age} = 1.93). The study included a 2 (Valence-Order: positive-negative vs. negative-positive) × 2 (Measurement Time: time 1 vs. time 2) × 3 (Context: Context A vs. Context B. vs. Context C) × 2 (Task: evaluative conditioning vs. verbal information) mixed design with the last variable as a between-subjects factor and the other three as within-subjects factors.

Procedure. The procedure in Experiment 4 was identical to Experiment 3, the only difference being that the presentation times in the evaluative learning tasks were increased from 3000 ms to 5000 ms.

RESULTS

Responses on the speeded evaluation task were aggregated in line with the procedures in Experiment 1. Submitted to a 2 (Valence Order) × 3 (Context) × 2 (Measurement Time) × 2 (Task) mixed ANOVA, evaluation scores revealed a significant main effect of Valence Order, $F(1, 490) = 5.28$, $p = .022$, $\eta_p^2 = .011$, a significant main effect of Measurement Time, $F(1, 490) = 17.78$, $p < .001$, $\eta_p^2 = .035$, and a significant two-way interaction of Measurement Time and Task, $F(1, 490) = 12.42$, $p < .001$, $\eta_p^2 = .025$. More important for the current investigation, there was a significant two-way interaction between Valence Order and Measurement Time, $F(1, 490) = 1180.97$, $p < .001$, $\eta_p^2 = .707$, which was qualified by a significant three-way interaction between Valence Order, Measurement Time, and Task, $F(1, 490) = 196.92$, $p < .001$, $\eta_p^2 = .287$. To decompose this interaction, we conducted separate 2 (Valence Order) × 3 (Context) × 2 (Measurement Time) repeated-measures ANOVAs for the two Task conditions.

Evaluative Conditioning. For participants in the evaluative-conditioning group, the ANOVA revealed a significant main effect of Measurement Time, $F(1, 239) = 20.37$, $p < .001$, $\eta_p^2 = .079$. This main effect was qualified by a significant two-way interaction of Valence Order and Measurement Time, $F(1, 239) = 167.63$, $p < .001$, $\eta_p^2 = .412$, which replicated the findings of Experiments 1-3 (see Figure 5a). At Time 1, CSs paired with positive USs in the initial conditioning task (and negative USs in the counterconditioning task) were evaluated more favorably compared to CSs paired with negative USs in the initial conditioning task (and negative USs in the counterconditioning task), $F(1, 239) = 127.64$, $p < .001$, $\eta_p^2 = .348$. Conversely, at Time 2, CSs paired with positive USs in the initial conditioning task (and negative USs in the counterconditioning task) were evaluated less favorably compared to CSs paired with negative USs in the initial conditioning task (and negative USs in the counterconditioning task), $F(1, 239) = 55.60$, $p < .001$, $\eta_p^2 = .189$. Moreover, CSs paired with positive USs in the initial conditioning task (and negative USs in the counterconditioning task) were evaluated more favorably at Time 1 compared to Time 2, $F(1, 239) = 190.59$, $p < .001$, $\eta_p^2 = .444$. Conversely, CSs paired with negative USs in the initial conditioning task (and positive USs in the counterconditioning task) were evaluated less favorably at Time 1 compared to Time 2, $F(1, 239) = 86.97$, $p < .001$, $\eta_p^2 = .267$.

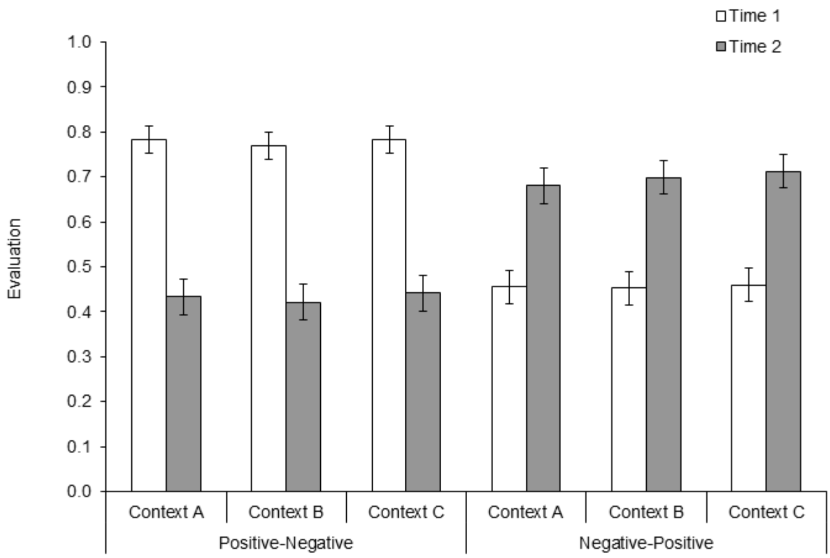

FIGURE 5a. Evaluative responses to target stimuli as a function of valence order (positive-negative vs. negative-positive), time (Time 1 vs. Time 2), and context (Context A vs. Context B vs. Context C) for evaluative conditioning, Experiment 4. Higher scores indicate more positive evaluations. Error bars depict 95% confidence intervals.

Importantly, the two-way interaction between Valence Order and Measurement Time was not qualified by a higher-order interaction with Context, $F(2, 478) = 0.53$, $p = .588$, $\eta_p^2 = .002$. The two-way interaction between Valence Order and Measurement Time was statistically significant regardless of whether CS evaluations were measured against the background of the initial conditioning task (Context A), $F(1, 239) = 143.85$, $p < .001$, $\eta_p^2 = .376$, the background of the counterconditioning task (Context B), $F(1, 239) = 166.71$, $p < .001$, $\eta_p^2 = .411$, or a novel background (Context C), $F(1, 239) = 158.57$, $p < .001$, $\eta_p^2 = .399$. Further analyses revealed that counterconditioning with negative USs significantly reduced favorable CS evaluations from Time 1 to Time 2 regardless of whether CS evaluations were measured against the background of the initial conditioning task (Context A), $t(239) = 13.26$, $p < .001$, $d = 0.871$, the background of the counterconditioning task (Context B), $t(239) = 12.89$, $p < .001$, $d = 0.845$, or a novel background (Context C), $t(239) = 12.68$, $p < .001$, $d = 0.835$. Conversely, counterconditioning with positive USs significantly increased favorable CS evaluations from Time 1 to Time 2 regardless of whether CS evaluations were measured against the background of the initial conditioning task (Context A), $t(239) = 7.86$, $p < .001$, $d = 0.509$, the background of the counterconditioning task (Context B), $t(239) = 9.13$, $p < .001$, $d = 0.592$, or a novel background (Context C), $t(239) = 9.35$, $p < .001$, $d = 0.606$. Together, these findings indicate that counterconditioning effectively reversed initially conditioned attitudes regardless of the context.

Verbal Information. For participants in the verbal-information group, the ANOVA revealed a significant main effect of Valence Order, $F(1, 251) = 5.86$, $p = .016$,

$\eta_p^2 = .023$, which was qualified by a significant two-way interaction between Valence Order and Measurement Time, $F(1, 251) = 1489.45$, $p < .001$, $\eta_p^2 = .856$. This interaction replicated the pattern obtained in the evaluative-conditioning group (see Figure 5b). However, as in Experiment 3, the effect sizes were again substantially larger, which presumably drove the significant three-way interaction between Valence Order, Measurement Time, and Task in the omnibus ANOVA. Specifically, at Time 1, targets paired with positive statements in the first block (and negative statements in the second block) were evaluated more favorably compared to targets paired with negative statements in the first block (and negative statements in the second block), $F(1, 251) = 1612.81$, $p < .001$, $\eta_p^2 = .865$. Conversely, at Time 2, targets paired with positive statements in the first block (and negative statements in the second block) were evaluated less favorably compared to targets paired with negative statements in the first block (and negative statements in the second block), $F(1, 251) = 717.98$, $p < .001$, $\eta_p^2 = .741$. Moreover, targets paired with positive statements in the first block (and negative statements in the second block) were evaluated more favorably at Time 1 compared to Time 2, $F(1, 251) = 1387.84$, $p < .001$, $\eta_p^2 = .847$. Conversely, targets paired with negative statements in the first block (and positive statements in the second block) were evaluated less favorably at Time 1 compared to Time 2, $F(1, 251) = 1194.05$, $p < .001$, $\eta_p^2 = .826$.

Importantly, counter to the notion of contextual renewal, the three-way interaction between Valence Order, Measurement Time, and Context was not statistically significant, $F(2, 502) = 0.05$, $p = .95$, $\eta_p^2 < .001$, and the two-way interaction between

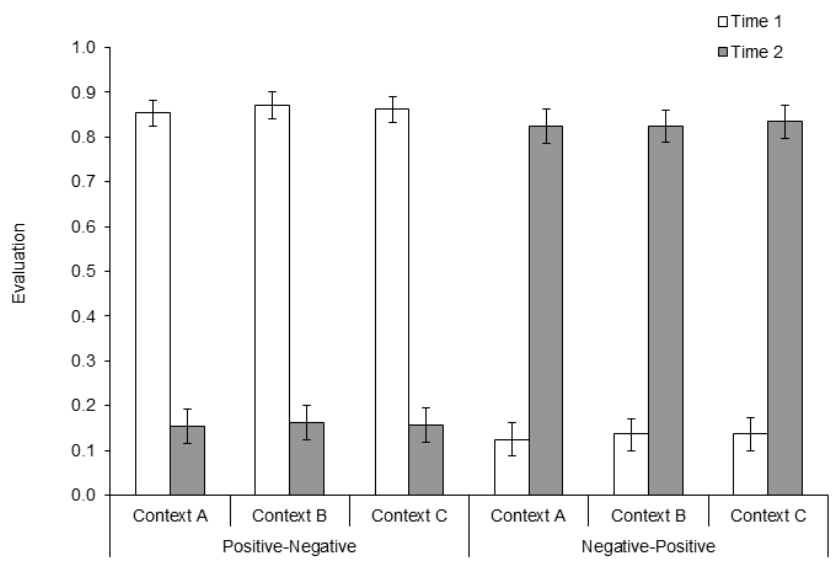

FIGURE 5b. Evaluative responses to target stimuli as a function of valence order (positive-negative vs. negative-positive), time (Time 1 vs. Time 2), and context (Context A vs. Context B vs. Context C) for verbal learning, Experiment 4. Higher scores indicate more positive evaluations. Error bars depict 95% confidence intervals.

Valence Order and Measurement Time was statistically significant regardless of whether target evaluations were measured against the background of the initial conditioning task (Context A), $F(1, 251) = 1279.96$, $p < .001$, $\eta_p^2 = .836$, the background of the counterconditioning task (Context B), $F(1, 251) = 1454.42$, $p < .001$, $\eta_p^2 = .853$, or a novel background (Context C), $F(1, 251) = 1441.49$, $p < .001$, $\eta_p^2 = .852$. Further analyses revealed that pairings with counterattitudinal negative statements significantly reduced favorable target evaluations from Time 1 to Time 2 regardless of whether target evaluations were measured against the background of the first block (Context A), $t(251) = 33.75$, $p < .001$, $d = 2.130$, the background of the second block (Context B), $t(251) = 35.23$, $p < .001$, $d = 2.231$, or a novel background (Context C), $t(251) = 35.56$, $p < .001$, $d = 2.245$. Conversely, pairings with counterattitudinal positive statements significantly increased favorable target evaluations from Time 1 to Time 2 regardless of whether target evaluations were measured against the background of the first block (Context A), $t(251) = 30.81$, $p < .001$, $d = 1.965$, the background of the second block (Context B), $t(251) = 33.06$, $p < .001$, $d = 2.108$, or a novel background (Context C), $t(251) = 33.64$, $p < .001$, $d = 2.145$. Thus, counter to previous evidence for contextual renewal effects in evaluative learning tasks using verbal information (e.g., Brannon & Gawronski, 2018a; Gawronski et al., 2010, 2014; Rydell & Gawronski, 2009; Ye et al., 2017), counterattitudinal information effectively reversed initially formed attitudes regardless of the context, replicating the findings of Experiment 3.

DISCUSSION

Counter to our prediction that an increase in presentation times would lead to contextual renewal effects in attitude change based on verbal information, Experiment 4 found no evidence for contextual renewal effects in the verbal-information group as well as the evaluative-conditioning group. Together with the findings of Experiment 3, these findings raise important questions about the reliability of contextual renewal effects in attitude change based on verbal information (see Gawronski, Hu et al., 2015). To the extent that contextual renewal effects fail to replicate in evaluative learning tasks using verbal information, the absence of contextual renewal effects in EC may be more informative about the unreliability of contextual renewal effects than about differences between EC and evaluative learning based on verbal information. In the final study, we aimed to address this issue more directly by using procedural parameters that are identical to the ones in earlier studies that obtained significant contextual renewal effects in attitude change based on verbal information.

EXPERIMENT 5

The procedural parameters in the verbal-information group of Experiment 4 were almost identical to the ones used in three prior studies on contextualized attitude change (Brannon & Gawronski, 2018a, Experiments 1 and 2; Gawronski et al.2014, Experiment 2), the only difference being the measurement of evaluative responses between the two learning blocks. Whereas Experiment 4 of the current work measured evaluative responses after the first learning block and then again after the second learning block, the relevant prior studies measured evaluative responses only after the second block. Although meta-analytic data by Gawronski, Hu, and colleagues (2015) did not reveal any effect of repeated measurements on the average size of ABA and ABC renewal effects, some studies suggest that repeated measurements can alter well-established findings in the EC literature. For example, counter to evidence suggesting that EC is resistant to extinction (for reviews, see De Houwer et al., 2001; Walther et al., 2005), some studies found that measurement of CS evaluations between acquisition and extinction trials makes EC susceptible to extinction (e.g., Gawronski, Gast, & De Houwer, 2015; Lipp & Purkis, 2006). Although we did not have a strong theoretical basis to expect contextual renewal effects to be influenced by repeated measurements of target evaluations, it is the only procedural factor that may explain the different outcomes in Experiment 4 of the current work and prior research using otherwise identical procedural parameters (Brannon & Gawronski, 2018a, Experiments 1 and 2; Gawronski et al., 2014, Experiment 2). Based on these considerations, Experiment 5 manipulated measurement time between-subjects rather than within-subjects to discern whether repeated measurement undermined the emergence of contextual renewal effects in the verbal-information group of Experiment 4. To the extent that repeated measurement moderates the emergence of contextual renewal effects in attitude change based on verbal information, an important follow-up question is whether contextual renewal effects occur for EC under the same procedural conditions.

METHODS

Participants and Design. A total of 495 undergraduate students at the University of Texas at Austin were recruited for a one-hour battery that included the present experiment and one additional study that was unrelated to this experiment.[6] Participants received research credit for an introductory psychology course. Due to computer malfunctions, data from seven participants were lost, leaving us with a final sample of 488 participants (348 women, 140 men, M_{age} = 18.78, SD_{age} = 1.04). The study included a 2 (Valence-Order: positive-negative vs. negative-positive) × 3 (Context: Context A vs. Context B. vs. Context C) × 2 (Measurement Time: time 1 vs. time 2) × 2 (Task: evaluative conditioning vs. verbal information) mixed design with the first two variables as within-subjects factors and the last two variables as between-subjects factors.

6. The sample size was determined prior to data collection to include 480 participants. Because G*Power (Faul et al., 2007) does not permit power estimates for the mixed-factorial design of Experiment 5 and we were unable to find alternative tools to obtain power estimates, the sample size was determined in a heuristic fashion based on the sample sizes in Experiments 3 and 4.

Procedure. The procedure in Experiment 5 was identical to Experiment 4, the only difference being that half of the participants completed the speeded evaluation task after the first block (Time 1) and the remaining half after the second block (Time 2).

RESULTS

Responses on the speeded evaluation task were aggregated in line with the procedures in Experiment 1. Submitted to a 2 (Valence Order) × 3 (Context) × 2 (Measurement Time) × 2 (Task) mixed ANOVA, evaluation scores revealed a significant main effect of Task, $F(1, 484) = 37.42$, $p < .001$, $\eta_p^2 = .072$, a significant main effect of Valence Order, $F(1, 484) = 243.17$, $p < .001$, $\eta_p^2 = .334$, a significant two-way interaction of Valence Order and Task, $F(1, 484) = 52.49$, $p < .001$, $\eta_p^2 = .098$, a significant two-way interaction of Valence Order and Measurement Time, $F(1, 484) = 179.88$, $p < .001$, $\eta_p^2 = .271$, and a significant two-way interaction of Valence Order and Context, $F(2, 968) = 9.89$, $p < .001$, $\eta_p^2 = .020$. More important for the current investigation, these effects were qualified by a significant three-way interaction between Valence Order, Measurement Time, and Task, $F(1, 484) = 29.19$, $p < .001$, $\eta_p^2 = .057$, and a significant three-way interaction between Valence Order, Context, and Measurement Time, $F(2, 968) = 11.56$, $p < .001$, $\eta_p^2 = .023$. There was also a marginal four-way interaction between Valence Order, Measurement Time, Context, and Task, $F(2, 968) = 2.80$, $p = .062$, $\eta_p^2 = .006$. To specify the pattern of these higher-order interactions, we conducted separate 2 (Valence Order) × 3 (Context) × 2 (Measurement Time) mixed ANOVAs for the two Task conditions.

Evaluative Conditioning. For participants in the evaluative-conditioning group, the ANOVA revealed a significant main effect of Valence Order, $F(1, 239) = 27.20$, $p < .001$, $\eta_p^2 = .102$, and a significant two-way interaction of Valence Order of Measurement Time, $F(1, 239) = 25.03$, $p < .001$, $\eta_p^2 = .095$, which were qualified by a significant three-way interaction between Valence Order, Context, and Time, $F(2, 478) = 3.34$, $p = .036$, $\eta_p^2 = .014$ (see Figure 6a). To specify this interaction, we conducted separate 2 (Valence Order) × 3 (Context) ANOVAs for the two Measurement Time conditions. For CS evaluations at Time 1, the ANOVA revealed a significant main effect of Valence Order, $F(1, 123) = 56.85$, $p < .001$, $\eta_p^2 = .316$, indicating that CSs paired with positive USs in the initial conditioning task (and negative USs in the counterconditioning task) were evaluated more favorably compared to CSs paired with negative USs in the initial conditioning task (and negative USs in the counterconditioning task). For CS evaluations at Time 2, the same ANOVA revealed a significant two-way interaction between Valence Order and Context, $F(2, 232) = 3.82$, $p = .023$, $\eta_p^2 = .032$. Although this interaction suggests that CS evaluations after counterconditioning depended on the context, post-hoc analyses did not support the hypothesis that counterconditioning would be more effective in changing initially conditioned attitudes in the context of the counterconditioning task compared to the other two contexts. Although there was a tendency for CSs paired with negative USs in the initial conditioning task (and positive USs in the

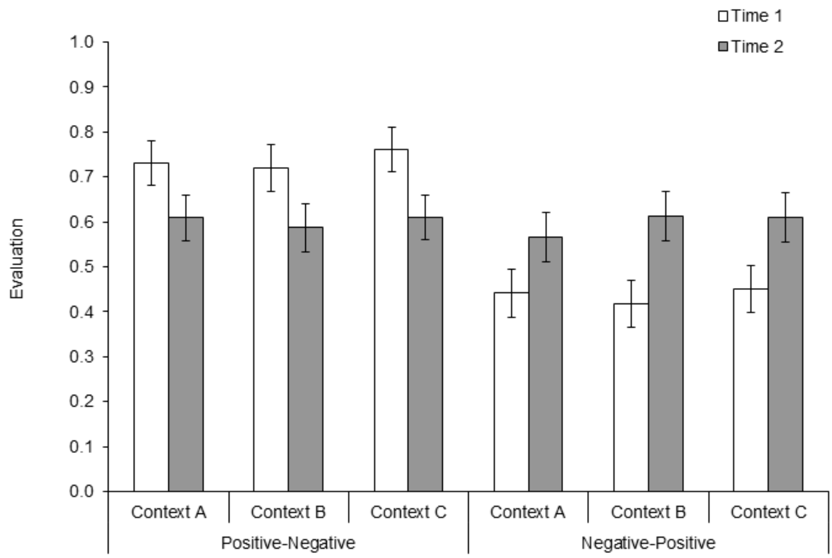

FIGURE 6a. Evaluative responses to target stimuli as a function of valence order (positive-negative vs. negative-positive), time (Time 1 vs. Time 2), and context (Context A vs. Context B vs. Context C) for evaluative conditioning, Experiment 5. Higher scores indicate more positive evaluations. Error bars depict 95% confidence intervals.

counterconditioning task) to be evaluated less favorably against the background of the initial conditioning task (Context A) compared to the background of the counterconditioning task (Context B), $t(116) = 1.81$, $p = .073$, $d = 0.167$, none of the other relevant comparisons revealed a significant difference between CS evaluations across contexts, all ts < 1.13, all ps > .26, all ds < 0.105. Moreover, there was no significant difference between evaluations of CSs that were paired with positive USs in the initial conditioning task (and negative USs in the counterconditioning task) and evaluations of CSs that were paired with negative USs in the initial conditioning task (and positive USs in the counterconditioning task) in any of the three contexts, all $|t|$s < 1, all ps > .33, all ds < 0.09.

Further analyses revealed that counterconditioning with negative USs significantly reduced favorable CS evaluations from Time 1 to Time 2 regardless of whether CS evaluations were measured against the background of the initial conditioning task (Context A), $t(239) = 2.97$, $p = .003$, $d = 0.382$, the background of the counterconditioning task (Context B), $t(239) = 3.06$, $p = .002$, $d = 0.394$, or a novel background (Context C), $t(239) = 3.75$, $p < .001$, $d = 0.482$. Conversely, counterconditioning with positive USs significantly increased favorable CS evaluations from Time 1 to Time 2 regardless of whether CS evaluations were measured against the background of the initial conditioning task (Context A), $t(239) = 2.77$, $p = .006$, $d = 0.358$, the background of the counterconditioning task (Context B), $t(239) = 4.33$, $p < .001$, $d = 0.557$, or a novel background (Context C), $t(239) = 3.55$,

$p < .001$, $d = 0.457$. Together, these findings indicate that counterconditioning effectively neutralized initially conditioned attitudes regardless of the context.

Verbal Information. For participants in the verbal-information group, the ANOVA revealed a significant main effect of Valence Order, $F(1, 245) = 356.34$, $p < .001$, $\eta_p^2 = .593$, a significant two-way interaction of Valence Order and Measurement Time, $F(1, 245) = 241.84$, $p < .001$, $\eta_p^2 = .497$, and a significant two-way interaction between Valence Order and Context, $F(2, 490) = 9.84$, $p < .001$, $\eta_p^2 = .039$, which were qualified by a significant three-way interaction between Valence Order, Time, and Context, $F(2, 490) = 10.06$, $p < .001$, $\eta_p^2 = .039$ (see Figure 6b). To specify this interaction, we conducted separate 2 (Valence Order) × 3 (Context) ANOVAs for the two Measurement Time conditions. For target evaluations at Time 1, the ANOVA revealed a significant main effect of Valence Order, $F(1, 122) = 1016.50$, $p < .001$, $\eta_p^2 = .893$, indicating that targets paired with positive statements in the first block (and negative statements in the second block) were evaluated more favorably compared to targets paired with negative statements in the first block (and negative statements in the second block). For target evaluations at Time 2, the same ANOVA revealed a significant main effect of Valence Order, $F(1, 123) = 3.92$, $p = .050$, $\eta_p^2 = .031$, which was qualified by a significant two-way interaction between Valence Order and Context, $F(2, 246) = 13.37$, $p < .001$, $\eta_p^2 = .098$.

Consistent with the notion of contextual renewal, post-hoc analyses revealed that counterattitudinal information was more effective in changing initially

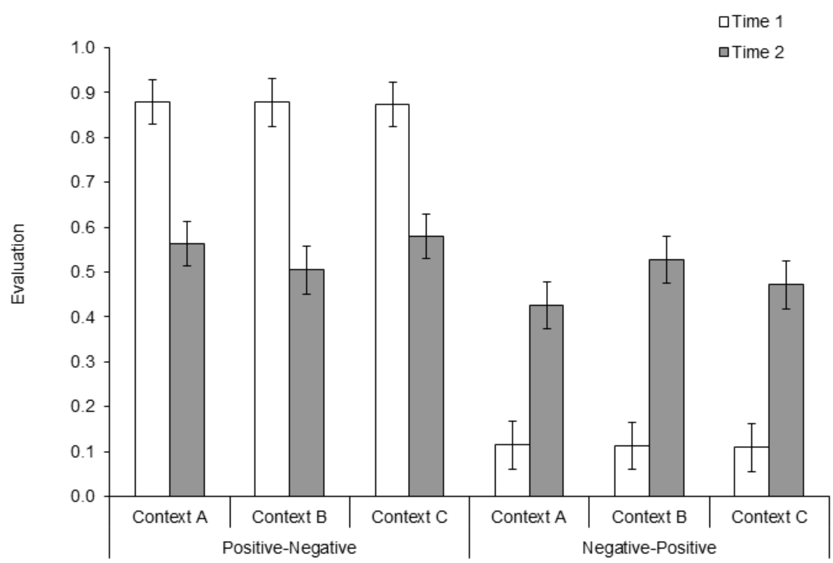

FIGURE 6b. Evaluative responses to target stimuli as a function of valence order (positive-negative vs. negative-positive), time (Time 1 vs. Time 2), and context (Context A vs. Context B vs. Context C) for verbal learning, Experiment 5. Higher scores indicate more positive evaluations. Error bars depict 95% confidence intervals.

formed attitudes in the context in which the counterattitudinal information had been learned compared to the other two contexts. Specifically, targets paired with negative statements in the first block (and positive statements in the second block) were evaluated more favorably against the background of the second block (Context B) compared to the background of the first block (Context A), $t(123) = 3.67$, $p < .001$, $d = 0.330$, and compared to a novel background that was not part of the learning task (Context C), $t(123) = 2.33$, $p = .021$, $d = 0.209$. Conversely, targets paired with positive statements in the first block (and negative statements in the second block) were evaluated less favorably against the background of the second block (Context B) compared to the background of the first block (Context A), $t(123) = 1.84$, $p = .069$, $d = 0.165$, and compared to a novel background that was not part of the learning task (Context C), $t(123) = 3.34$, $p = .001$, $d = 0.300$. Moreover, the relative impact of initial attitudinal and subsequent counterattitudinal information on evaluations at Time 2 depended on the context, in that evaluations reflected the initial attitudinal information in Context A and Context C, but not in Context B. Specifically, when the targets were presented against the background of the first block (Context A), targets paired with positive statements in the first block (and negative statements in the second block) were evaluated more favorably than targets paired with negative statements in the first block (and positive statements in the second block), $t(123) = 3.26$, $p = .001$, $d = 0.292$. Similarly, when the targets were presented against a novel background (Context C), targets paired with positive statements in the first block (and negative statements in the second block) were evaluated more favorably than targets paired with negative statements in the first block (and positive statements in the second block), $t(123) = 2.82$, $p = .006$, $d = 0.253$. In contrast, when the targets were presented against the background of the second block (Context B), there was no significant difference between evaluations of targets that were paired with positive statements in the first block (and negative statements in the second block) and evaluations of targets that were paired with negative statements in the first block (and positive statements in the second block), $t(123) = -0.53$, $p = .600$, $d = 0.047$. Nevertheless, pairings with counterattitudinal positive statements significantly increased favorable target evaluations from Time 1 to Time 2 regardless of whether target evaluations were measured against the background of the first block (Context A), $t(245) = 10.39$, $p < .001$, $d = 1.324$, the background of the second block (Context B), $t(245) = 13.51$, $p < .001$, $d = 1.721$, or a novel background (Context C), $t(245) = 11.86$, $p < .001$, $d = 1.511$. Conversely, pairings with counterattitudinal negative statements significantly reduced favorable target evaluations from Time 1 to Time 2 regardless of whether target evaluations were measured against the background of the first block (Context A), $t(245) = 10.30$, $p < .001$, $d = 1.312$, the background of the second block (Context B), $t(245) = 11.43$, $p < .001$, $d = 1.456$, or a novel background (Context C), $t(245) = 9.71$, $p < .001$, $d = 1.236$. Together, these results suggest that although counterattitudinal information effectively neutralized initially formed attitudes in the context in which the counterattitudinal information had been learned, initial attitudinal information continued to dominate evaluations in the other two contexts.

DISCUSSION

Consistent with the post-hoc assumption that repeated measurement eliminated contextual renewal effects in the verbal-information group of Experiment 4, Experiment 5 obtained evidence for contextual renewal in the verbal-information group when there was no measurement of evaluations between the presentation of initial attitudinal information and the presentation of counterattitudinal information. Moreover, consistent with one of the initial predictions we sought to test, counterconditioning changed initially conditioned attitudes to the same extent irrespective of the context. Although the findings of Experiment 5 permit an alternative interpretation of the non-significant effects of context on the effectiveness of counterconditioning in Experiments 1–4, the combined findings of the five experiments suggest two important boundary conditions for the emergence of contextual renewal effects in evaluative learning. First, contextual renewal effects are less likely to occur for EC than for evaluative learning based on verbal information. Second, contextual renewal effects in evaluative learning based on verbal information are less likely to occur when evaluations are measured between the acquisition of initial attitudes and the learning of counterattitudinal information. Although the latter conclusion is based on an informal comparison of the findings in Experiments 3–5 rather than a direct manipulation of repeated measurement, the obtained difference between studies reconciles the current findings with previous research showing contextual renewal effects with materials and procedures that were identical to the ones in Experiment 5 (Brannon & Gawronski, 2018a, Experiments 1 and 2; Gawronski et al., 2014, Experiment 2).

GENERAL DISCUSSION

Expanding on the concepts of contextual renewal (Bouton, 2004) and contextualized attitude change (Gawronski et al., 2018), the main question of the current research was whether counterconditioning of previously conditioned attitudes determines evaluations only in the context in which counterconditioning occurred, with previously conditioned attitudes determining evaluations in any other context. The current endeavor to address this question revealed two sets of findings, one expected and the other unexpected.

First, consistent with the assumptions that (1) CS-US pairings can influence evaluative responses independent of conscious expectancies and (2) contextual renewal effects are driven by enhanced attention to context during the encoding of expectancy-violating information, we did not find any evidence for contextual renewal effects in the relative impact of counterconditioning. Even integrative analyses (Curran & Hussong, 2009) and Bayesian analyses (Rouder, Speckman, Sun, Morey, & Iverson, 2009) of the data from all five studies failed to reveal a consistent pattern of effects that would be in line with the concepts of contextual renewal and contextualized attitude change (see Table 1).

Second, counter to the results of previous studies, we did not find any evidence for contextual renewal effects in two of the three studies that included verbal information as a control condition. An informal comparison of the three studies suggests that the failure to replicate earlier evidence for contextual renewal effects in attitude change based on verbal information was due to the inclusion of an evaluation measure between the acquisition of initial attitudes and the presentation of counterattitudinal information. Together, the two sets of findings suggest that contextualized attitude change depends on two hitherto unidentified boundary conditions that need to be reconciled with extant theories of evaluative learning.

IMPLICATIONS FOR EVALUATIVE CONDITIONING

Although it seems possible that the repeated measurement design in Experiments 1–4 contributed to the lack of contextual renewal effects in EC, the results of Experiment 5 suggest that the absence of contextual renewal in EC is a more general phenomenon that is independent of repeated measurement. This conclusion is consistent with evidence suggesting that CS-US pairings can influence evaluative responses to the CS independent of conscious expectancies (see De Houwer et al., 2001; Walther et al., 2005). Although distinct effects of CS-US pairings on liking and expectancies may not necessarily stem from distinct underlying processes (Aust et al., 2019; De Houwer et al., 2019), the current findings are in line with the idea that conscious expectancies may not be necessary for the emergence of EC effects. Additionally, the current findings contribute to the body of evidence suggesting that CS-US pairings can show effects that are different from the effects produced by verbal information (e.g., Hu, Gawronski, & Balas, 2017; Kurdi & Banaji, 2019).

For the sake of theoretical clarity, it is worth noting that the current findings do not rule out a potential role of conscious expectancies in EC. Assuming that contextual renewal effects are driven by enhanced attention to context during the encoding of expectancy-violating information (Gawronski et al., 2018; Ogallar et al., 2017), the current findings are consistent with the hypothesis that CS-US pairings *can* lead to EC effects independent of conscious expectancies, and they are inconsistent with the hypothesis that conscious expectancies are *necessary* for EC

TABLE 1. Effect sizes (Cohen's *d*) and JZS Bayes factors (BF) of context effects after counterconditioning as a function of valence order (positive-negative) and context comparison (Context B vs. Context A; Context B vs. Context C), combined data from evaluative-conditioning groups in Experiments 1-5 (n = 893). Effect sizes are coded such that higher scores reflect a greater impact of counterconditioning in the context in which countercounditioning occurred (Context B) compared to the context of initial conditioning (Context A) and a novel context (Context C), respectively.

	d	BF	BF Interpretation
Positive-Negative			
Context B vs. Context A	0.019	BF_{01} = 22.77	strong evidence for H_0
Context B vs. Context C	0.104	BF_{10} = 211.45	decisive evidence for H_1
Negative-Positive			
Context B vs. Context A	0.061	BF_{01} = 4.96	substantial evidence for H_0
Context B vs. Context C	-0.060	BF_{01} = 5.33	substantial evidence for H_0

effects. However, neither of these conclusions implies that conscious expectancies do not contribute to EC effects over and above the contribution of expectancy-independent learning mechanisms (see Gawronski & Bodenhausen, 2018).

An important question is why the current studies failed to obtain any evidence for contextual renewal effects in EC, although contextual renewal effects have been found for various other instances of classical conditioning (for a review, see Bouton, 2004). For example, in research on conditioned aversive responses (e.g., conditioned fear response to a neutral sound due to pairings of the sound with electric shocks), effects of extinction and counterconditioning have been found to be limited to the context in which extinction and counterconditioning occurred (e.g., Bouton & Bolles, 1979). Similarly, in research on conditioned appetitive responses (e.g., conditioned saliva response to a neutral sound due to pairings of the sound with food), effects of extinction and counterconditioning have been found to be limited to the context in which extinction and counterconditioning occurred (e.g., Bouton & Peck, 1989). Although the status of EC as a distinct phenomenon is still under debate (see Bar-Anan & Balas, 2018; Corneille & Stahl, 2019; De Houwer, 2009, 2014, 2018; Gawronski & Bodenhausen, 2018; Mitchell, De Houwer, & Lovibond, 2009), some evidence suggests that EC differs from other instances of classical conditioning in that EC effects do not depend on conscious expectancies (for reviews, see De Houwer et al., 2001; Walther et al., 2005). For example, when a neutral sound is repeatedly followed by an electric shock, the neutral sound acquires the status of a signal for the electric shock, leading to the expectation of receiving an electric shock upon hearing the sound. Similarly, when a neutral sound is repeatedly followed by palatable food, the neutral sound acquires the status of a signal for the food, leading to the expectation of receiving the food upon hearing the sound. EC has been claimed to be different from such cases of expectancy learning in that repeated pairings of a neutral CS and a valenced US can lead to changes in the (dis)liking of the CS independent of conscious expectancies about CS-US relations (see De Houwer et al., 2001; Walther et al., 2005). For example, a sound that used to signal an electric shock may still be disliked even when the sound is no longer expected to signal an upcoming shock. Similarly, a sound that used to signal palatable food may still be liked even when the sound is no longer expected to signal upcoming food. Although the theoretical implications of dissociations between conditioned expectancies and conditioned liking are still under debate (Aust et al., 2019; De Houwer et al., 2019), there is sufficient evidence to treat the two as functionally distinct phenomena.

Nevertheless, it is important to note that EC is not a uniform phenomenon. Although EC effects are often treated as the outcome of one particular mechanism (for discussions, see Bar-Anan & Balas, 2018; Jones, Olson, & Fazio, 2010), there is evidence suggesting that procedural factors of CS-US pairings influence the functional properties of EC effects. For example, EC effects have been found to be differentially sensitive to subsequent changes in the valence of the US depending on whether a CS has been paired with a single US or multiple USs of the same valence (Sweldens, Van Osselaer, & Janiszewski, 2010). Moreover, EC effects have been claimed to differ in their dependency on recollective memory for CS-US pairings as a function of whether the pairings involve simultaneous or sequential presentations of the CS and the US (Hütter & Sweldens, 2013; but see Mierop, Hütter, & Corneille, 2017). In the current research, we utilized an EC paradigm involving simultaneous presentations of a CS with a single US, which has been claimed to produce EC effects independent of conscious expectancies (Hütter & Sweldens, 2013; Sweldens et al., 2010). Future research may help to establish the generalizability of our findings to EC paradigms with different procedural parameters (see Gawronski, Gast et al., 2015).

IMPLICATIONS FOR CONTEXTUALIZED ATTITUDE CHANGE VIA VERBAL INFORMATION

One unexpected finding of the current research is that measurement of evaluative responses between the acquisition of initial attitudes and the presentation of counterattitudinal information eliminated contextual renewal effects in attitude change based on verbal information. This finding seems even more surprising in light of meta-analytic results by Gawronski, Hu and colleagues (2015) who found no significant difference in the size of ABA and ABC renewal effects depending on whether the study did or did not include repeated measurements of evaluative responses. However, an important aspect of their meta-analytic database is that all studies with repeated measurements used an evaluative learning task with one target individual and a between-subjects manipulation of valence order (e.g., Gawronski et al., 2010; Rydell & Gawronski, 2009). In this procedure, context and valence are systematically related, in that positive statements are presented in one context and negative statements are presented in the other context. This aspect is different in the evaluative learning task in the current studies, which included two target individuals and a within-subjects manipulation of valence order (e.g., Brannon & Gawronski, 2018a; Gawronski et al., 2014). In this procedure, context and valence are unrelated, in that each context includes an equal number of positive and negative statements. This difference is important, because systematic relations between context and valence can create direct associations between the two, such that the context itself may directly elicit an evaluative response (see Bouton, 2010; Gawronski et al., 2018; Vervliet, Baeyens, Van den Bergh, & Hermans, 2013). Such direct associations between context and valence seem unlikely when there is no systematic relation between the two. In this case, contextual renewal effects are more likely driven by contextualized representations that moderate the evaluative response elicited by the target (see Gawronski et al., 2014).

Applied to the current findings, these considerations suggest that repeated measurement may not undermine the formation of direct context-valence associations, as suggested by the null effect of repeated measurement in the Gawronski, Hu, and colleagues' (2015) meta-analysis. Yet, repeated measurement seems to undermine the formation of contextualized representations that moderate the response elicited by the target, as suggested by the moderating effect of repeated measurement in the current studies. Although it is unclear why repeated measurement influences the formation of contextualized representations in the latter case (for similar effects in the extinction of conditioned attitudes, see Gawronski, Gast et al., 2015; Lipp & Purkis, 2006), the current findings echo earlier quests for more nuanced analyses of the mental representations underlying contextual renewal effects (Bouton, 2010; Urcelay & Miller, 2010; Vervliet et al., 2013).

OPEN QUESTIONS

Although the current findings are consistent with accounts suggesting that (1) CS-US pairings can influence evaluative responses independent of conscious expectancies and (2) contextual renewal effects are driven by enhanced attention to context during the encoding of expectancy-violating information, an important question is whether other factors might have contributed to the lack of contextual renewal effects in EC. For example, research by Brannon and Gawronski (2017) suggests that extreme information can be effective in producing context-independent changes of initially formed attitudes. To the extent that the USs in the evaluative-conditioning group were more extreme than the statements in the verbal-information group, the obtained asymmetry might have been driven by differences in the extremity of the materials in the two conditions rather than genuine differences in the effects of CS-US pairings and verbal information. However, counter to this hypothesis, the verbal information in the current studies generally showed larger effects compared to the CS-US pairings. Based on this finding, Brannon and Gawronski's (2017) results would suggest that contextual renewal effects should be more likely to occur for EC than evaluative learning based on verbal information, which was not the case. Instead, we found the opposite pattern.

Another important question is whether different instructions in the two learning conditions contributed to the obtained asymmetry in contextual renewal effects. Whereas participants in the verbal-information group were asked to form an impression of the targets based on the presented information, participants in the evaluative-conditioning group were asked to pay close attention to the images without impression-formation instructions. Thus, it is possible that the obtained asymmetry in contextual renewal effects was driven by the absence of an impression-formation goal in the evaluative-conditioning group rather than a fundamental difference in the effects of CS-US pairings and verbal information. Although we cannot rule out that different processing goals contributed to the obtained results, it is worth noting that instructions to form impressions based on CS-US pairings should promote the formation of conscious expectancies. From this perspective, contextual renewal effects in EC with impression-formation instructions would be consistent with the proposed role of expectancy violations in the emergence of contextual renewal effects. Together with the current findings, these considerations suggest that counterconditioning might be more effective in producing context-independent changes of initially conditioned attitudes under conditions of incidental learning compared to conditions of intentional learning. Future research manipulating processing goals during the encoding of CS-US pairings may help to provide deeper insights into these questions.

CONCLUSION

The main goal of the current research was to investigate the emergence of contextual renewal effects in EC. Our findings suggest that counterconditioning changes initially conditioned attitudes to the same extent irrespective of the context. These findings are consistent with accounts suggesting that (1) CS-US pairings can influence evaluative responses independent of conscious expectancies and (2) contextual renewal effects are driven by enhanced attention to context during the encoding of expectancy-violating information. Moreover, context renewal effects in attitude change based on verbal information were found to depend on the measurement of evaluations between the formation of initial attitudes and the learning of counterattitudinal information. In addition to identifying two hitherto unknown boundary conditions of contextual renewal, these findings contribute to the growing body of evidence suggesting important differences between evaluative learning based on stimulus pairings and verbal information, which need to be reconciled with extant theories of evaluative learning.

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