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Propositional Versus Dual-Process Accounts of Evaluative Conditioning: I. The Effects of Co-Occurrence and Relational Information on Implicit and Explicit Evaluations

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Abstract

Evaluative conditioning (EC) is defined as the change in the evaluation of a conditioned stimulus (CS) due to its pairing with a valenced unconditioned stimulus (US). According to propositional accounts, EC effects should be qualified by the relation between the CS and the US. Dual-process accounts suggest that relational information should qualify EC effects on explicit evaluations, whereas implicit evaluations should reflect the frequency of CS–US co-occurrences. Experiments 1 and 2 showed that, when relational information was provided before the encoding of CS–US pairings, it moderated EC effects on explicit, but not implicit, evaluations. In Experiment 3, relational information moderated EC effects on both explicit and implicit evaluations when it was provided simultaneously with CS–US pairings. Frequency of CS–US pairings had no effect on implicit evaluations. Although the results can be reconciled with both propositional and dual-process accounts, they are more parsimoniously explained by propositional accounts.

Keywords

associative learning, attitudes, dual-process theories, evaluative conditioning, propositional theory

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People have a natural tendency to evaluate objects in their environment. Attitudes—defined as the tendency to evaluate an object with some degree of favor or disfavor (Eagly & Chaiken, 2007)—can have profound effects on behavior by influencing the construal of the current situation, eliciting spontaneous approach-avoidance tendencies, and guiding the formation of deliberate action plans (Fazio, 1990; Strack & Deutsch, 2004). Understanding the origin and behavioral effects of attitudes is important, because it can provide valuable insights for a wide range of questions, including research on consumer behavior (e.g., Gibson, 2008), intergroup relations (e.g., Olson & Fazio, 2006), health behavior (e.g., Hollands, Prestwich, & Marteau, 2011), and affective disorders (e.g., Ouimet, Gawronski, & Dozois, 2009).

One particularly influential paradigm in studying the origin of attitudes is known as evaluative conditioning (EC), which is defined as the change in the evaluation of a conditioned stimulus (CS) due to its pairing with a positive or negative unconditioned stimulus (US; see De Houwer, 2007). Early research suggested that EC is characterized by several unique features that distinguish it from other forms of Pavlovian conditioning, including the insensitivity of EC to statistical contingencies and its resistance to extinction (for reviews, see De Houwer, Thomas, & Baeyens, 2001; Walther, Nagengast, & Trasselli, 2005).[1] Based on this research, it has been proposed that associative learning processes play a dominant role in EC. According to associative accounts, EC effects are due to the automatic formation of associative links between the CS and the US (e.g., Walther, Gawronski, Blank, & Langer, 2009) or the CS and the affective response elicited by the US (e.g., Sweldens, Van Osselaer, & Janiszewski, 2010).

Although research on EC has been guided by associative theories for decades, the available evidence regarding its functional properties is rather mixed and difficult to reconcile with early accounts (for a meta-analysis, see Hofmann, De Houwer, Perugini, Baeyens, & Crombez, 2010). To fill this theoretical gap, researchers have proposed alternative

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theories of the mental processes and representations underlying EC effects (for a review, see Jones, Olson, & Fazio, 2010). Currently, there are two accounts that dominate the debate about the most comprehensive explanation of EC (Gast, Gawronski, & De Houwer, 2012). According to single-process propositional accounts, EC effects are mediated by the non-automatic formation and validation of propositions about the relation between a CS and a US (e.g., De Houwer, 2009, 2014; Mitchell, De Houwer, & Lovibond, 2009). In contrast, dual-process accounts state that EC effects can be the result of either associative or propositional processes (or both), with the relative contribution of the two processes depending on various contextual conditions (e.g., Gawronski & Bodenhausen, 2011; Sweldens et al., 2010). The main goal of the current research was to provide empirical input for this debate by investigating the effects of co-occurrence and relational information on implicit and explicit evaluations.

Effects of Relational Information

A central question in the debate between propositional and dual-process accounts of EC concerns the moderating role of information about the relation between a CS and a US (Gawronski, Brannon, & Bodenhausen, 2017). By definition, propositional processes capture the particular manner in which two events are related (De Houwer, 2009). Thus, to the extent that EC effects are mediated by propositional processes, information about the relation between the CS and the US should moderate CS evaluations in a manner that is consistent with the evaluative meaning of this relation. For example, information that a pharmaceutical product relieves headaches should lead to a positive evaluation of the product due to its positive effect, and this positive evaluation should override potential negative evaluations that may result from repeated co-occurrences of the pharmaceutical product and headaches. From the perspective of propositional accounts, EC effects resulting from mere co-occurrences between a CS and a US can be explained by the formation and validation of propositions about the co-occurrence of the two stimuli. Yet, if more complex information about the relation of the two stimuli is available (e.g., information about their causal relation), CS evaluations should reflect the evaluative meaning of this relation rather than the valence of the co-occurring US (see De Houwer, 2009).

Dual-process accounts also acknowledge the potential contribution of propositional processes to EC effects, but they additionally propose a second learning mechanism involving the formation of unqualified associative links between co-occurring stimuli (e.g., Gawronski & Bodenhausen, 2011; Sweldens et al., 2010). A central assumption of dual-process accounts is that the effects of relational information require propositional inferences, and therefore are more likely to occur for deliberate judgments that reflect the outcome of such inferences (i.e., explicit evaluations). However, observing a moderating effect of relational information on explicit evaluations does not mean that repeated co-occurrences of a CS and a US have no effect at all. Rather, repeated co-occurrences are claimed to produce unqualified associative links that should influence spontaneous responses resulting from the spread of activation between associated concepts (i.e., implicit evaluations). Thus, whereas single-process propositional accounts predict a moderating effect of relational information on both explicit and implicit evaluations, dual-process accounts predict a dissociation, such that relational information should moderate EC effects on explicit, but not implicit, evaluations.

Consistent with the shared prediction of the two accounts for explicit evaluations, numerous studies have shown that information about the relation between a CS and a US moderates the impact of CS–US pairings on explicit evaluations (e.g., Fiedler & Unkelbach, 2011; Förderer & Unkelbach, 2012; Gawronski, Walther, & Blank, 2005; Moran & Bar-Anan, 2013; Zanon, De Houwer, & Gast, 2012; Zanon, De Houwer, Gast, & Smith, 2014). However, the available evidence regarding the conflicting predictions for implicit evaluations is rather mixed. In one of the first studies on this question, Gawronski et al. (2005) found that repeated pairings of a neutral CS face with a positive or negative US face led to corresponding changes in implicit CS evaluations when participants were told that the two individuals like each other. In this case, the CS faces elicited more favorable responses when they were paired with a positive face than when they were paired with a negative face. However, the effect of the pairings on implicit evaluations reversed when participants were told that the two individuals dislike each other. In this case, the CS faces elicited more favorable responses when they were paired with a negative face than when they were paired with a positive face (cf. Heider, 1958). Importantly, such a reversal of EC effects did not occur when participants first learned about the relation between a CS face and a neutral US face and later acquired information that the US face is positive or negative (cf. Walther, 2002). Under these conditions, evaluative responses to the CS faces reflected the subsequent valence of the US faces they had been paired with regardless of whether the two faces liked or disliked each other. These results indicate that the timing of evaluative and relational information might be an important factor that moderates the impact of relational information (see also Langer, Walther, Gawronski, & Blank, 2009). This conclusion is consistent with findings by Zanon et al. (2014) who found similar order effects in an EC paradigm using verbal stimuli and relational information about their semantic meaning (i.e., CS has the same meaning versus opposite meaning as the US).

Although order effects might be important to understand the relative effectiveness of relational information, the available evidence for its impact on implicit evaluations is far from conclusive. For example, counter to the reversed EC effects obtained by Gawronski et al. (2005), some studies found only attenuated, but not reversed, EC effects when relational information suggested an evaluation that was opposite to the valence of the US (e.g., Zanon et al., 2012). Yet, others found regular EC effects that remained unqualified by relational information (e.g., Moran & Bar-Anan, 2013). So far, the most compelling evidence for dual-process accounts has been presented by Moran and Bar-Anan (2013). In their study, participants were presented with neutral stimuli (CS) that started or stopped either pleasant or unpleasant sounds (US). Consistent with the shared prediction for explicit evaluations, participants showed more favorable judgments of stimuli that started pleasant sounds compared with stimuli that started unpleasant sounds. Conversely, participants showed more favorable judgments of stimuli that stopped unpleasant sounds compared with stimuli that stopped pleasant sounds. In contrast, implicit evaluations reflected the co-occurrence of CSs and USs regardless of their relation. That is, participants showed more favorable responses to stimuli that co-occurred with pleasant than unpleasant sounds regardless of whether the stimuli started or stopped the sounds. These results are consistent with the predictions of dual-process accounts. However, they are inconsistent with the predictions of propositional accounts, which imply equivalent effects of relational information on both explicit and implicit evaluations.

Effects of Reinforcement

The conflicting evidence regarding the effects of relational information suggests that there are important boundary conditions that moderate its impact on implicit evaluations. Although the order of information acquisition might be one such moderator (Gawronski et al., 2005; Zanon et al., 2014), it is still unclear why some studies found a moderating effect of relational information when this information was available during the encoding of CS–US pairings (e.g., Gawronski et al., 2005) whereas others did not find a moderating effect under similar conditions (e.g., Moran & Bar-Anan, 2013). One potential factor that has received relatively little attention so far is the frequency of CS–US pairings. From an associative view, stronger reinforcement with a larger number of CS–US pairings should strengthen the resulting associative links, thereby increasing their impact on implicit evaluations. Thus, to the extent that propositional inferences can have top-down effects on implicit evaluations in the absence of strong associations (Gawronski & Bodenhausen, 2006), unqualified associative effects of CS–US pairings may be limited to conditions of strong reinforcement with large numbers of pairings. Yet, with small numbers of pairings, propositional inferences about CS–US relations may fully override the effects of weak associative links, thereby leading to a moderating effect of relational information on both explicit and implicit evaluations.

To test these hypotheses, the current research investigated the joint effects of relational information and CS–US repetitions on EC effects on explicit and implicit evaluations. Toward this end, we repeatedly paired images of pharmaceutical products (CSs) with images of positive and negative health conditions (USs). As a manipulation of reinforcement, the CS–US pairings were presented with either a low or a high number of repetitions. As a manipulation of relational information, participants were informed that the pharmaceutical products either cause or prevent the depicted health conditions. In Experiments 1 and 2, this relational information was provided before participants were presented with the CS–US pairings. In Experiment 3, relational information was provided on a trial-by-trial basis for each CS–US pair.

According to single-process propositional accounts, both explicit and implicit evaluations should reflect the relation between the CSs and the USs, such that products that cause positive health conditions should be evaluated more favorably than products that cause negative health conditions. Conversely, products that prevent negative health conditions should be evaluated more favorably than products that prevent positive health conditions. Although propositional accounts do not explicitly address the role of reinforcement, either of these effects may be enhanced by CS–US repetitions, given that frequent exposure should facilitate any type of learning.

These predictions differ from the ones implied by dual-process accounts, which suggest that relational information may qualify EC effects on implicit evaluations only for low, but not for high, CS–US repetitions. In the latter case, implicit evaluations should reflect unqualified co-occurrence effects irrespective of the manner in which the co-occurring stimuli are related. For explicit evaluations, dual-process accounts predict the same outcome as single-process propositional accounts. Thus, the two central questions of the current research are (a) whether relational information moderates EC effects on implicit evaluations, and (b) whether the impact of relational information on implicit evaluations depends on the frequency of CS–US pairings.[2]

Experiment 1

Method

Participants and design. Two-hundred-and-twenty-nine undergraduate students (160 women, 69 men) at the University of Western Ontario were recruited for a 1-hr battery that included the current experiment and two unrelated studies. Participants received CAD$10 as a compensation for their time. The study included a 2 (US Valence: positive vs. negative) × 2 (CS–US Repetition: low vs. high) × 2 (CS–US Relation: cause vs. prevent) mixed-model design with the first two factors varying within-subjects and the third factor varying between-subjects. Four participants had missing data in at least one of the two evaluation measures (i.e., they did not complete the explicit ratings or had no valid trials on the evaluative priming task). These participants were excluded from the analyses, leaving us with a final sample of 225 participants.

EC procedure. Four images of pharmaceutical products were used as CSs to be paired with a positive or negative US. One additional pharmaceutical product was used as a neutral baseline CS that was not paired with a US. Four pictures depicting health-related conditions were used as USs. Two of the US pictures showed positive health conditions (i.e., an elderly couple riding bikes; a woman waving her long hair); the other two pictures showed negative health conditions (i.e., eczema on a man's legs; an infant with an eye infection). Two of the four CSs were paired with a positive US; the remaining two CSs were paired with a negative US. The particular pairings of CSs and USs were counterbalanced by means of a Latin square. On each trial of the EC task, participants were presented with one of the CSs in the center of the screen for 1,000 ms, which was followed by the US in the same location for 1,000 ms. The inter-trial-interval (ITI) was 2,000 ms. Two of the CS–US pairs were presented eight times (low repetition); the other two pairs were presented 24 times (high repetition), summing up to a total of 64 trials.

Participants were informed that the study investigates how people process information about consumer products and that they will be presented with images of pharmaceutical products followed by visual information about their effects. Participants were told that many pharmaceutical products have positive effects, but some products also have negative side-effects. For the manipulation of relational information, participants received the following instructions:

> Your task is to think of the image pairs, such that the pharmaceutical product causes [prevents] what is displayed in the following image. For example, if a product is paired with a positive image, you should think of the product as causing [preventing] the positive outcome displayed in the image. Conversely, if a product is paired with a negative image, you should think of the product as causing [preventing] the negative outcome displayed in the image.

Measures. To measure explicit evaluations, participants were asked to rate the five pharmaceutical products on two 7-point items. The first item asked them how positive or negative they find each product, with response options ranging from 1 (*very negative*) to 7 (*very positive*). The second item asked them how good or bad they find each product, with response options ranging from 1 (*very bad*) to 7 (*very good*). As a measure of implicit evaluations, we used Fazio, Jackson, Dunton, and Williams' (1995) evaluative priming task. The evaluative priming task included the five CSs as primes and positive and negative adjectives as targets, using the procedural details of earlier applications to EC (e.g., Gawronski, Balas, & Creighton, 2014; Gawronski, Mitchell, & Balas, 2015). The positive target words were *pleasant*, *good*, *outstanding*, *beautiful*, *magnificent*, *marvelous*, *excellent*, *appealing*, *delightful*, *nice*; the negative target words were *unpleasant*, *bad*, *horrible*, *miserable*, *hideous*, *dreadful*, *painful*, *repulsive*, *awful*, *ugly*. Each trial started with a fixation cross that was displayed for 500 ms in the center of the screen. The fixation cross was followed by a prime stimulus, which was replaced by the target word after 200 ms. Participants' task was to press a right-hand key (*Numpad 5*) as quickly as possible when the target word was positive and a left-hand key (*A*) when the target word was negative. The target words remained on the screen until participants made their response. Incorrect responses were followed by the word *ERROR!* for 1,500 ms before the next trial started. The inter-trial interval was set to 500 ms. Each of the five CSs was presented once with each of the 10 positive target words and once with each of the 10 negative words, summing up to a total of 100 trials. The order of the evaluative rating measure and the evaluative priming task was counterbalanced across participants.[3] After completion of the two evaluation tasks, we assessed participants' memory for the CS–US pairings with a variant of the four-picture recognition task (Walther & Nagengast, 2006). The memory task asked them to identify which of the four USs had been paired with a given CS. For this purpose, participants were presented with the four USs at the top of the screen and one of the CSs at the bottom of the screen. Each US was marked with a number from 1 to 4. A fifth response option, marked with the number 9, was labeled *none of the above*. Participants were asked to make their response by pressing the corresponding key on the computer keyboard.

Results

Data aggregation. Baseline-corrected scores of explicit CS evaluations were calculated by averaging participants' ratings on the two evaluation items for each of the five CSs and then subtracting participants' average ratings of the neutral baseline CS from their average ratings of each of the four CSs that had been paired with a positive or negative US. Thus, higher values indicate more favorable evaluations of a given CS compared with baseline. Before aggregating the response latency data of the evaluative priming task, we excluded latencies from trials with incorrect responses (7.4%) and truncated latencies higher than 800 ms (see Gawronski et al., 2005, 2015). For each CS that had been paired with a valenced US, a positivity index was calculated by subtracting the mean response latency to positive target words preceded by a given CS from the mean response latency to positive target words preceded by the neutral baseline CS (Wentura & Degner, 2010). Negativity indices were calculated accordingly by subtracting the mean response latency to negative target words preceded by a given CS from the mean response latency to negative target words preceded by the neutral baseline CS. The negativity scores of each CS were then subtracted from the positivity scores of the same CS. Thus, higher values indicate more favorable evaluations of the CS compared with baseline.

Table 1. Means and 95% CIs of Explicit CS Evaluations as a Function of US Valence (Positive vs. Negative), CS–US Relation (CS Causes US vs. CS Prevents US), and CS–US Repetition (Low vs. High).

	Positive US		Negative US	
	M	95% CI	*M*	95% CI
Experiment 1				
CS causes US				
Low repetition	2.42	[2.07, 2.77]	−2.46	[−2.81, −2.12]
High repetition	2.45	[2.08, 2.82]	−2.59	[−2.94, −2.25]
CS prevents US				
Low repetition	−0.21	[−0.55, 0.14]	0.37	[0.02, 0.71]
High repetition	−0.31	[−0.68, 0.06]	0.41	[0.06, 0.76]
Experiment 2				
CS causes US				
Low repetition	2.06	[1.72, 2.40]	−2.53	[−2.82, −2.24]
High repetition	2.35	[2.02, 2.69]	−2.55	[−2.86, −2.24]
CS prevents US				
Low repetition	−0.23	[−0.56, 0.11]	0.63	[0.34, 0.92]
High repetition	−0.28	[−0.62, 0.05]	0.69	[0.38, 0.99]
Experiment 3				
CS causes US				
Low repetition	1.85	[1.56, 2.14]	−1.39	[−1.72, −1.07]
High repetition	2.56	[2.34, 2.78]	−1.94	[−2.25, −1.63]
CS prevents US				
Low repetition	−0.71	[−0.99, −0.43]	1.06	[0.72, 1.40]
High repetition	−1.03	[−1.34, −0.71]	1.74	[1.40, 2.08]

Note. CS = conditioned stimulus; US = unconditioned stimulus; CI = confidence interval.

Explicit evaluations. Means and confidence intervals of explicit CS evaluations are presented in Table 1. Submitted to a 2 (US Valence) × 2 (CS–US Repetition) × 2 (CS–US Relation) mixed-model analysis of variance (ANOVA), explicit evaluations revealed a significant main effect of US Valence, $F(1, 223) = 102.18$, $p < .001$, $\eta_p^2 = .314$, indicating that CSs that had been paired with positive USs were evaluated more favorably than CSs that had been paired with negative USs. This effect was qualified by a significant two-way interaction of US Valence and CS–US Relation, $F(1, 223) = 172.79$, $p < .001$, $\eta_p^2 = .437$, indicating a regular EC effect in the *cause* condition and a non-significant tendency for a reversed EC effect in the *prevent* condition (see Figure 1). When the CSs caused the USs, CSs that had been paired with positive USs were evaluated more favorably than CSs that had been paired with negative USs, $F(1, 111) = 1555.35$, $p < .001$, $\eta_p^2 = .933$. In contrast, when the CSs prevented the USs, CSs that had been paired with positive USs were evaluated less favorably than CSs that had been paired with negative USs, although this reversed EC effect failed to reach statistical significance, $F(1, 112) = 2.54$, $p = .11$, $\eta_p^2 = .022$. The ANOVA also revealed a marginally significant three-way interaction between US Valence, CS–US Relation, and CS–US Repetition, $F(1, 223) = 3.25$, $p = .07$, $\eta_p^2 = .014$. Further analyses indicated that regular EC effects tended to be somewhat larger for high- compared with low-repetition pairings when the CSs caused the USs, but this effect failed to reach statistical significance, $F(1, 111) = 2.27$, $p = .13$, $\eta_p^2 = .020$. Repetition did not significantly influence the size of reversed EC effects when the CSs prevented the USs, $F(1, 112) = 1.22$, $p = .27$, $\eta_p^2 = .011$.

Implicit evaluations. Means and confidence intervals of implicit evaluation scores are presented in Table 2. Submitted to the same ANOVA, implicit evaluations revealed a significant main effect of US Valence, $F(1, 223) = 5.75$, $p = .02$, $\eta_p^2 = .025$, indicating that the CSs elicited more favorable responses when they had been paired with positive USs than when they had been paired with negative USs. Unlike explicit evaluations, the main effect of US Valence was not moderated by relational information, $F(1, 223) = 0.01$, $p = .93$, $\eta_p^2 < .001$ (see Figure 2). The three-way interaction of US Valence, CS–US Relation, and CS–US Repetition failed to reach statistical significance, $F(1, 223) = 2.47$, $p = .12$, $\eta_p^2 = .011$. No other main or interaction effect reached statistical significance (all Fs < 1, all ps > .33).

Comparison of implicit and explicit evaluation. To test whether relational information differentially influenced implicit and explicit evaluations, we calculated a single EC score for each of the two measures by subtracting the average evaluation scores of CSs that had been paired with negative USs from

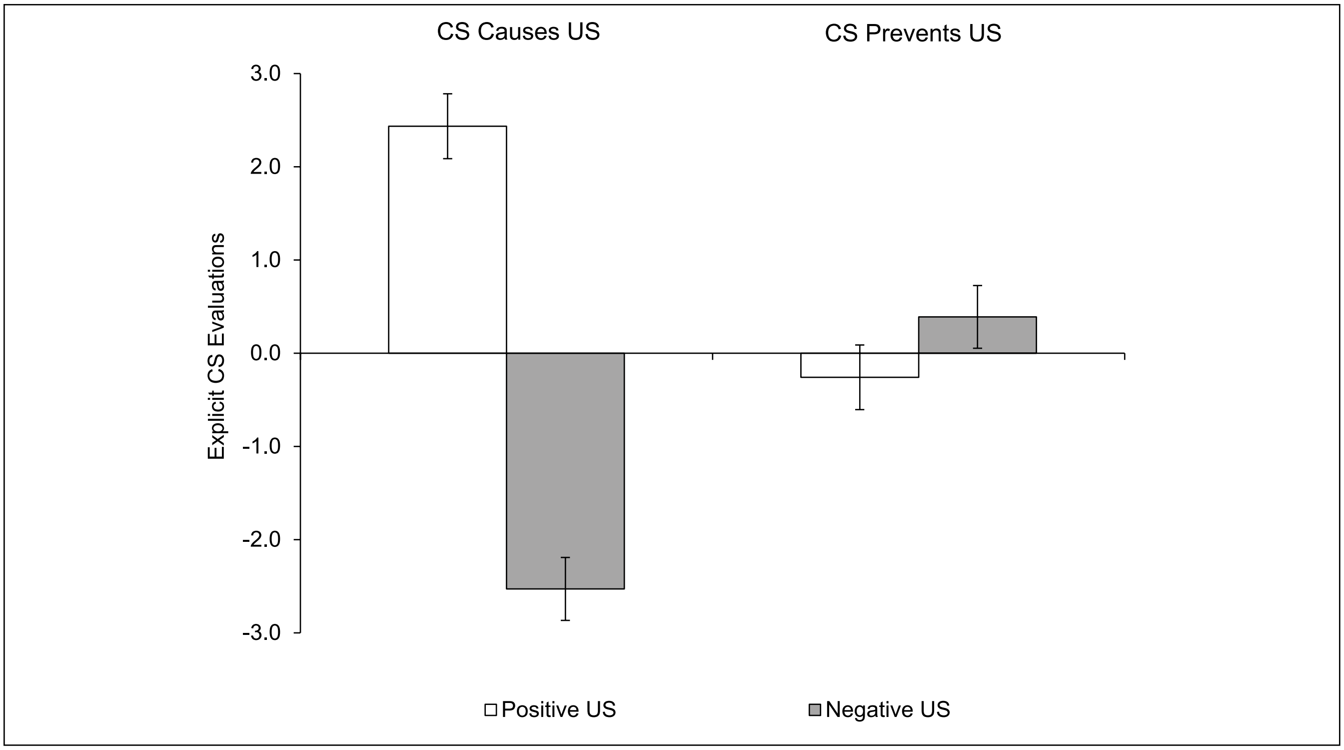

Figure 1. Explicit CS evaluations as a function of US Valence (positive vs. negative) and CS–US Relation (CS causes US vs. CS prevents US), Experiment 1.
Note. Error bars depict 95% confidence intervals. CS = conditioned stimulus; US = unconditioned stimulus.

Table 2. Means and 95% CIs of Implicit CS Evaluations as a Function of US Valence (Positive vs. Negative), CS–US Relation (CS Causes US vs. CS Prevents US), and CS–US Repetition (Low vs. High).

	Positive US		Negative US	
	M	95% CI	*M*	95% CI
Experiment 1				
CS causes US				
Low repetition	−3.05	[−15.65, 9.56]	−2.43	[−16.68, 11.82]
High repetition	3.01	[−8.24, 14.26]	−16.32	[−29.40, −3.25]
CS prevents US				
Low repetition	−0.17	[−12.72, 12.37]	−12.64	[−26.83, 1.55]
High repetition	−2.73	[−13.93, 8.47]	−10.41	[−23.42, 2.61]
Experiment 2				
CS causes US				
Low repetition	15.05	[1.46, 28.63]	2.16	[−12.20, 16.52]
High repetition	12.47	[−0.61, 25.54]	−0.13	[−15.10, 14.83]
CS prevents US				
Low repetition	19.75	[6.20, 33.31]	8.07	[−6.10, 22.23]
High repetition	14.08	[1.48, 26.68]	13.68	[−0.70, 28.07]
Experiment 3				
CS causes US				
Low repetition	1.49	[−9.01, 11.98]	−5.09	[−15.69, 5.50]
High repetition	2.98	[−8.76, 14.71]	−0.62	[−11.07, 9.83]
CS prevents US				
Low repetition	−6.74	[−17.00, 3.53]	5.99	[−3.99, 15.96]
High repetition	−5.21	[−15.87, 5.46]	−3.02	[−14.33, 8.29]

Note. CS = conditioned stimulus; US = unconditioned stimulus; CI = confidence interval.

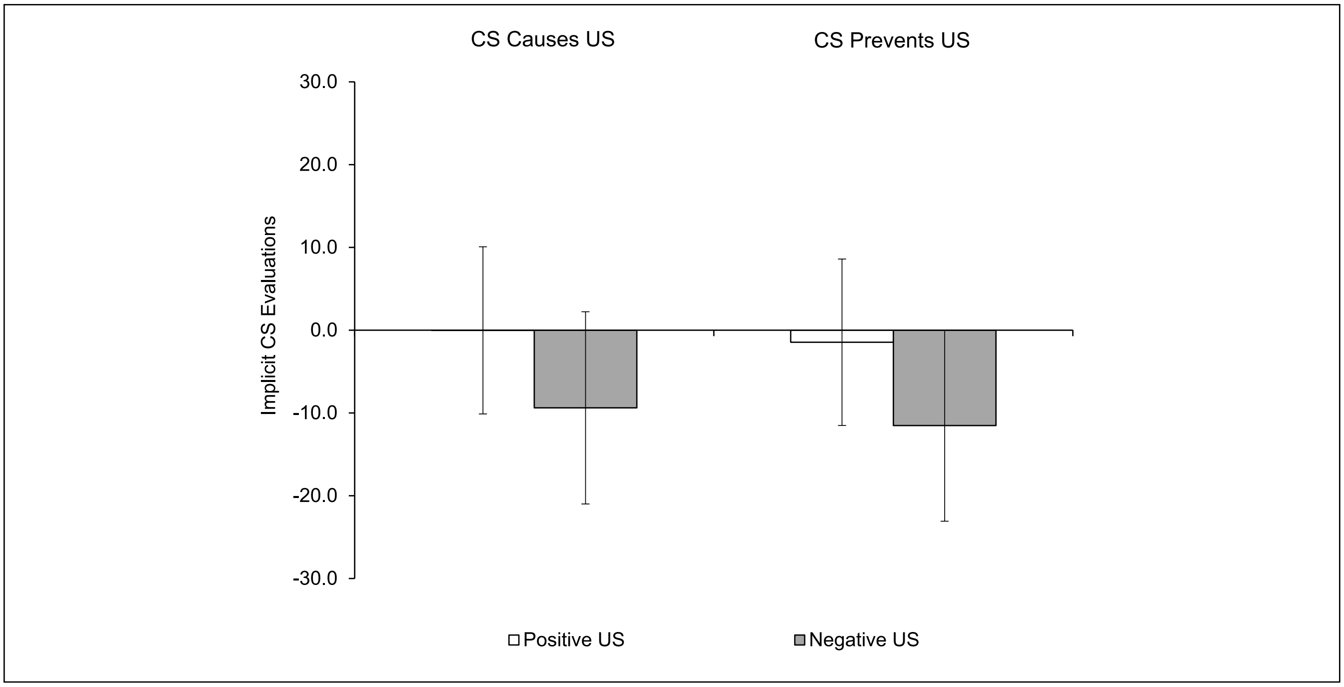

Figure 2. Implicit CS evaluations as a function of US Valence (positive vs. negative) and CS–US Relation (CS causes US vs. CS prevents US), Experiment 1.
Note. Error bars depict 95% confidence intervals. CS = conditioned stimulus; US = unconditioned stimulus.

the average evaluation scores of CSs that had been paired with positive USs. The resulting EC scores were standardized and submitted to a 2 (Measure: explicit vs. implicit) × 2 (CS–US Relation: cause vs. prevent) mixed-model ANOVA. A significant two-way interaction of Measure and CS–US Relation confirmed that the impact of relational information differed for the two kinds of evaluations, $F(1, 223) = 71.91$, $p < .001$, $\eta_p^2 = .244$.

Memory. Participants showed highly accurate memory for CS–US pairings with an average score of 90.2% (including the neutral baseline CS).[4] A 2 (US Valence) × 2 (CS–US Repetition) × 2 (CS–US Relation) mixed-model ANOVA on memory performance scores revealed a significant main effect of CS–US Relation, $F(1, 222) = 12.95$, $p < .001$, $\eta_p^2 = .055$, indicating that participants in the *cause* condition showed better recognition performance than participants in the *prevent* condition (96.2% vs. 85.0%). This main effect was qualified by a significant two-way interaction of CS–US Repetition and CS–US Relation, $F(1, 222) = 4.88$, $p = .03$, $\eta_p^2 = .022$. Follow-up analyses showed that participants in the *prevent* condition tended to show better recognition performance when CS–US Repetition was high than when it was low (87.1% vs. 83.0%), $F(1, 111) = 3.31$, $p = .07$, $\eta_p^2 = .029$. For participants in the *cause* condition, recognition performance was not significantly influenced by repetition (95.5% vs. 96.9%), $F(1, 111) = 1.81$, $p = .18$, $\eta_p^2 = .016$. No other main or interaction effects reached statistical significance (all Fs < 2.4, all ps > .12).

Discussion

Experiment 1 found that relational information qualified EC effects on explicit, but not implicit, evaluations. In the current study, participants showed more favorable explicit evaluations of pharmaceutical products when they caused positive health outcomes than when they caused negative health outcomes. Conversely, explicit evaluations tended to be more favorable when the products prevented negative health outcomes than when they prevented positive health outcomes. Yet, different from the pattern observed for explicit evaluations, implicit evaluations reflected the valence of co-occurring health outcomes regardless of whether the products caused or prevented these outcomes. Moreover, counter to the assumption that mere co-occurrence effects require reinforcement with a large number of CS–US repetitions, EC effects on implicit evaluations remained unqualified by relational information for both low-repetition and high-repetition pairings. Repetition simply enhanced regular EC effects on explicit evaluations when the products caused the co-occurring outcomes. Together, these findings provide partial support for dual-process accounts, which suggest that relational information should qualify EC effects on explicit, but not implicit, evaluations. However, the current findings are inconsistent with the dual-process hypothesis that relational information may qualify implicit evaluations when the number of CS–US pairings is low, but not when it is high.

To test the generality of these findings, we conducted a second study that used a slightly modified EC procedure.

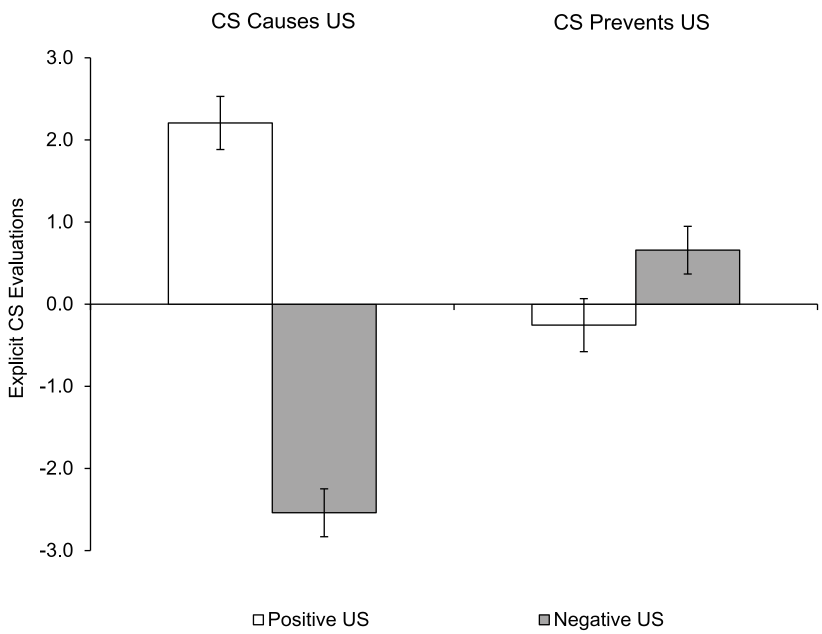

Figure 3. Explicit CS evaluations as a function of US Valence (positive vs. negative) and CS–US Relation (CS causes US vs. CS prevents US), Experiment 2.
Note. Error bars depict 95% confidence intervals. CS = conditioned stimulus; US = unconditioned stimulus.

Some studies suggest that EC effects can have different functional properties depending on whether the CSs and the USs appear sequentially or simultaneously (e.g., Hütter & Sweldens, 2013). Thus, to provide a more comprehensive picture of the differential effect of relational information on explicit and implicit evaluations, Experiment 2 aimed to replicate the findings of Experiment 1 using an EC paradigm that involved simultaneous instead of sequential pairings.

Experiment 2

Method

Participants and design. Two-hundred-and-eighty-four undergraduates (178 women, 103 men, three missing) at the University of Texas at Austin were recruited for a 1-hr battery that included the current experiment and two unrelated studies. Participants received research credit for an introductory psychology course. The design was identical to Experiment 1. Eight participants had missing data in at least one of the two evaluation measures (i.e., they did not complete the explicit ratings or had no valid trials on the evaluative priming task); one additional participant showed an excessive number of errors and outliers (60% of the trials) in the evaluative priming task. These participants were excluded from the analyses, leaving us with a final sample of 275 participants.

Procedure and measures. The EC procedure was identical to Experiment 1, the only difference being that the CSs and the USs were presented simultaneously for 1,000 ms. The CSs were always presented at the bottom of the screen and the USs at the top. All measures and instructions were identical to Experiment 1.

Results

Explicit evaluations. Baseline-corrected scores of explicit CS evaluations were calculated in line with the procedures of Experiment 1. Means and confidence intervals of explicit evaluations are presented in Table 1. A 2 (US Valence) × 2 (CS–US Repetition) × 2 (CS–US Relation) mixed-model ANOVA revealed a significant main effect of US Valence, $F(1, 273) = 102.15$, $p < .001$, $\eta_p^2 = .272$, and a significant main effect of CS–US Relations, $F(1, 273) = 10.58$, $p = .001$, $\eta_p^2 = .037$, which were qualified by a significant two-way interaction of the two factors, $F(1, 273) = 222.65$, $p < .001$, $\eta_p^2 = .449$ (see Figure 3). Further analyses revealed that, when the CSs caused the USs, CSs that had been paired with a positive US were evaluated more favorably than CSs that had been paired with a negative US, $F(1, 136) = 1{,}588.43$, $p < .001$, $\eta_p^2 = .921$. In contrast, when the CSs prevented the USs, CSs that had been paired with positive USs were evaluated less favorably than CSs that had been paired with negative USs, $F(1, 137) = 6.47$, $p = .01$, $\eta_p^2 = .045$. In addition to these effects, the ANOVA revealed a marginally significant main effect of CS–US Repetition, $F(1, 273) = 3.16$, $p = .08$, $\eta_p^2 = .011$, and a marginally significant two-way interaction of CS–US Relation and CS–US Repetition, $F(1, 273) = 3.16$,

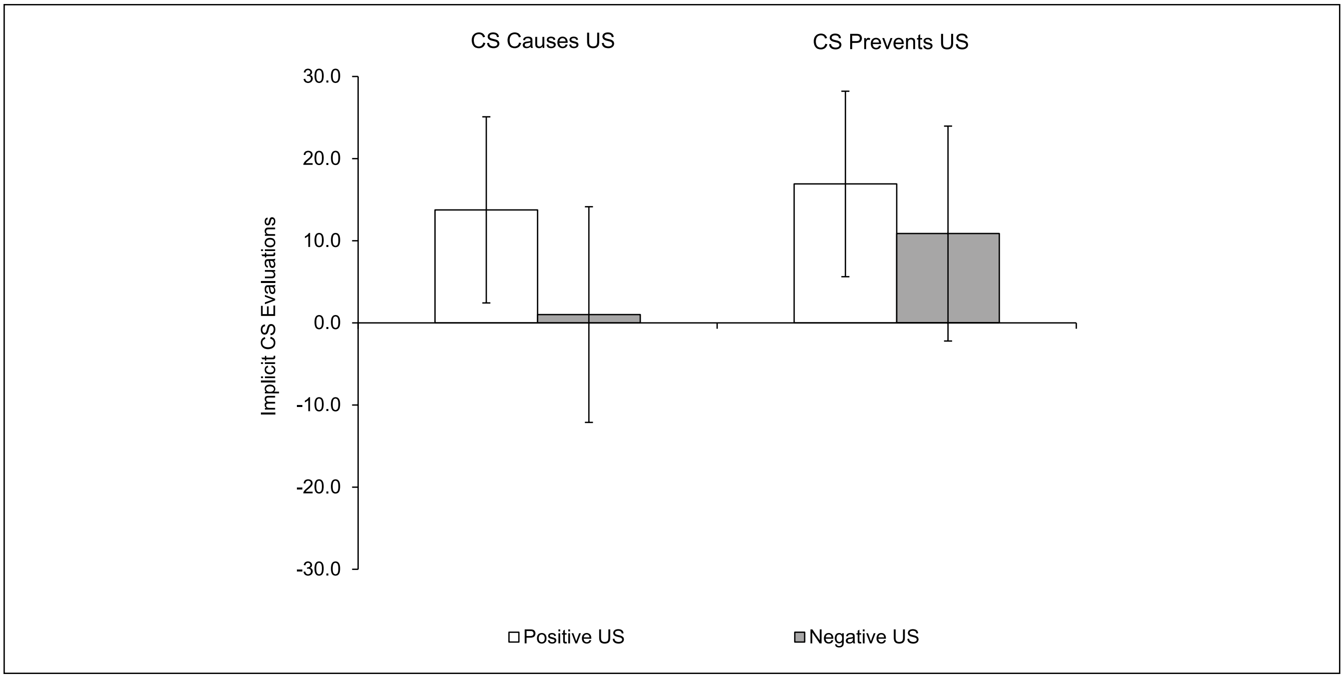

Figure 4. Implicit CS evaluations as a function of US Valence (positive vs. negative) and CS–US Relation (CS causes US vs. CS prevents US), Experiment 2.
Note. Error bars depict 95% confidence intervals. CS = conditioned stimulus; US = unconditioned stimulus.

p = .08, η_p^2= .011, which were qualified by a significant three-way interaction of US Valence, CS–US Relation, and CS–US Repetition, $F(1, 273) = 6.52$, $p = .01$, $\eta_p^2 = .023$. Further analyses revealed that high repetition of CS–US pairings increased regular EC effects when the CSs caused the USs, $F(1, 136) = 8.41$, $p = .004$, $\eta_p^2 = .058$. There was no significant influence of repetition on the effect of US Valence when the CSs prevented the USs, $F(1, 137) = 0.75$, $p = .39$, $\eta_p^2 = .005$.

Implicit evaluations. Before aggregating the response latency data of the evaluative priming task, we excluded latencies from trials with incorrect responses (8.1%) and truncated latencies higher than 800 ms (see Gawronski et al., 2005, 2015). Baseline-corrected scores of implicit CS evaluations were calculated in line with the procedures of Experiment 1. Means and confidence intervals of implicit evaluations are presented in Table 2. A 2 (US Valence) × 2 (CS–US Repetition) × 2 (CS–US Relation) mixed-model ANOVA revealed a significant main effect of US Valence, $F(1, 273) = 8.47$, $p = .004$, $\eta_p^2 = .030$, indicating CSs that had been paired with a positive US elicited more favorable responses than CSs that had been paired with a negative US. Replicating the pattern of Experiment 1, this effect was not qualified by CS–US Relation, $F(1, 273) = 1.08$, $p = .30$, $\eta_p^2 = .004$ (see Figure 4). There was also no significant three-way interaction of US Valence, CS–US Relation, and CS–US Repetition, $F(1, 273) = 0.76$, $p = .38$, η_p^2= .003. No other main or interaction effects reached statistical significance (all Fs < 1, all ps > .30).

Comparison of implicit and explicit evaluation. To test whether relational information differentially influenced implicit and explicit evaluations, we again calculated standardized EC scores for each of the two measures and submitted them to a 2 (Measure) × 2 (CS–US Relation) mixed-model ANOVA. A significant two-way interaction of Measure and CS–US Relation confirmed that the impact of relational information differed for the two kinds of evaluations, $F(1, 273) = 69.03$, $p < .001$, $\eta_p^2 = .202$.

Memory. Participants showed highly accurate memory for CS–US pairings with an average score of 90.7% (including the neutral baseline CS).[5] A 2 (US Valence) × 2 (CS–US Repetition) × 2 (CS–US Relation) mixed-model ANOVA revealed a significant main effect of CS–US Relation, $F(1, 269) = 5.17$, $p = .02$, $\eta_p^2 = .019$, showing that participants in the *cause* condition showed better recognition performance than participants in the *prevent* condition (95.3% vs. 89.2%). A marginally significant main effect CS–US Repetition further indicated that recognition performance tended to be better for high repetition than low repetition pairings (93.4% vs. 91.2%), $F(1, 269) = 3.64$, $p = .06$, $\eta_p^2 = .013$. Finally, a marginally significant main effect of US Valence indicated that recognition memory tended to be better for CSs that had been paired with positive USs than CSs that had been paired with negative USs (93.2% vs. 91.4%), $F(1, 269) = 3.69$, $p = .06$, $\eta_p^2 = .014$. No other main or interaction effects reached statistical significance (all Fs < 2.4, all ps > .12).

Discussion

Experiment 2 replicated the main findings of Experiment 1, showing that relational information qualified EC effects on explicit, but not implicit, evaluations. Whereas explicit evaluations were sensitive to the evaluative meaning of the causal relation between a CS and a US, implicit evaluations reflected CS–US co-occurrences irrespective of their relation. Although the differential effect of relational information provides partial support for dual-process accounts, we again did not obtain the predicted effect of CS–US repetition on implicit evaluations. Counter to the hypothesis that relational information should qualify implicit evaluations when the number of CS–US pairings is low but not when it is high, EC effects on implicit evaluations remained unqualified by relational information regardless of CS–US Repetitions.

Although the differential effect of relational information on implicit and explicit evaluations is consistent with dual-process accounts, the lack of a significant repetition effect on implicit evaluations raises the question of whether the current findings can be reconciled with single-process propositional accounts. One procedural feature that suggests a potential propositional interpretation is that the relational information was presented before the CS–US pairings, such that it had to be applied to all of the following CS–US pairings. Hence, it is possible that the learning processes in the two studies involved the independent acquisition of two distinct pieces of information: (a) a general rule that all of the CSs either cause or prevent the USs they are paired with, and (b) the specific US that a given CS is paired with. To the extent that (a) the independent acquisition of the two pieces of information undermines their mental integration during the encoding of the CS–US pairings and (b) a post hoc application of the rule during the expression of an evaluative response requires time and cognitive resources, a successful integration may occur only for deliberate evaluative judgments (i.e., explicit evaluations), but not for spontaneous evaluative reactions (i.e., implicit evaluations). In this case, the differential effect of relational information would reflect the higher cognitive demands of applying the relational rule to the independently acquired co-occurrence information during the expression of an evaluative response (cf. Gawronski, Balas, & Hu, in press). Importantly, this interpretation does not require any assumptions about two functionally distinct learning mechanisms. Although it is possible that the relational rule is acquired via propositional learning and CS–US co-occurrences influence CS representations via associative learning, the proposed interpretation in terms of ineffective integration is perfectly consistent with the assumption that both pieces of information are acquired via propositional learning. In fact, given that we did not obtain the predicted effect of CS–US repetition, one could argue that single-process propositional accounts offer a more parsimonious explanation for the current pattern of results.

To address this ambiguity, Experiment 3 aimed to create conditions that rule out ineffective integration as an explanation for differential effects of relational information. Toward this end, Experiment 3 manipulated relational information as a within-subjects factor, with the respective relations being presented trial-by-trial for each CS–US pair. This procedural modification was assumed to promote simultaneous processing of relational information and CS–US pairings, and thereby promote effective integration of the two pieces of information. Thus, if the differential effect of relational information in Experiments 1 and 2 reflects the higher cognitive demands of applying a relational rule to independently acquired co-occurrence information during the expression of an evaluative response, the procedural changes in Experiment 3 should lead to corresponding effects of relational information on explicit and implicit evaluations. In contrast, if differential effects of relational information reflect the simultaneous operation of associative and propositional processes during the encoding of CS–US pairings, Experiment 3 should produce the same pattern of results that was observed in Experiments 1 and 2.

Experiment 3

Method

Participants and design. One-hundred-and-forty-two undergraduates (82 women, 57 men, three missing) at the University of Texas at Austin were recruited for a 1-hr battery that included the current experiment and one unrelated study. Participants received research credit for an introductory psychology course. Due to computer malfunctions, data from three participants were lost. Three additional participants were excluded from the analyses due to missing data in the implicit evaluation measure (i.e., they had no valid trials on the evaluative priming task). These exclusions left us with a final sample of 136 participants. The study included a 2 (US Valence: positive vs. negative) × 2 (CS–US Repetition: low vs. high) × 2 (CS–US Relation: cause vs. prevent) within-subjects design.

EC procedure. Eight images of pharmaceutical products were used as CSs to be paired with a positive or negative US. One additional pharmaceutical product was used as a neutral baseline CS that was not paired with a US. Eight pictures depicting positive and negative health conditions were used as USs. Four of the eight CSs were paired with a positive US; the remaining four CSs were paired with a negative US. Each CS–US pair was assigned to one of the eight experimental conditions implied by the manipulation of CS–US Repetition and CS–US Relation. The particular pairings of CSs and USs and their assignment to the eight conditions were counterbalanced by means of a Latin square. To strengthen the manipulation of CS–US Repetition, we reduced the number of trials in the low-repetition condition from eight to five and

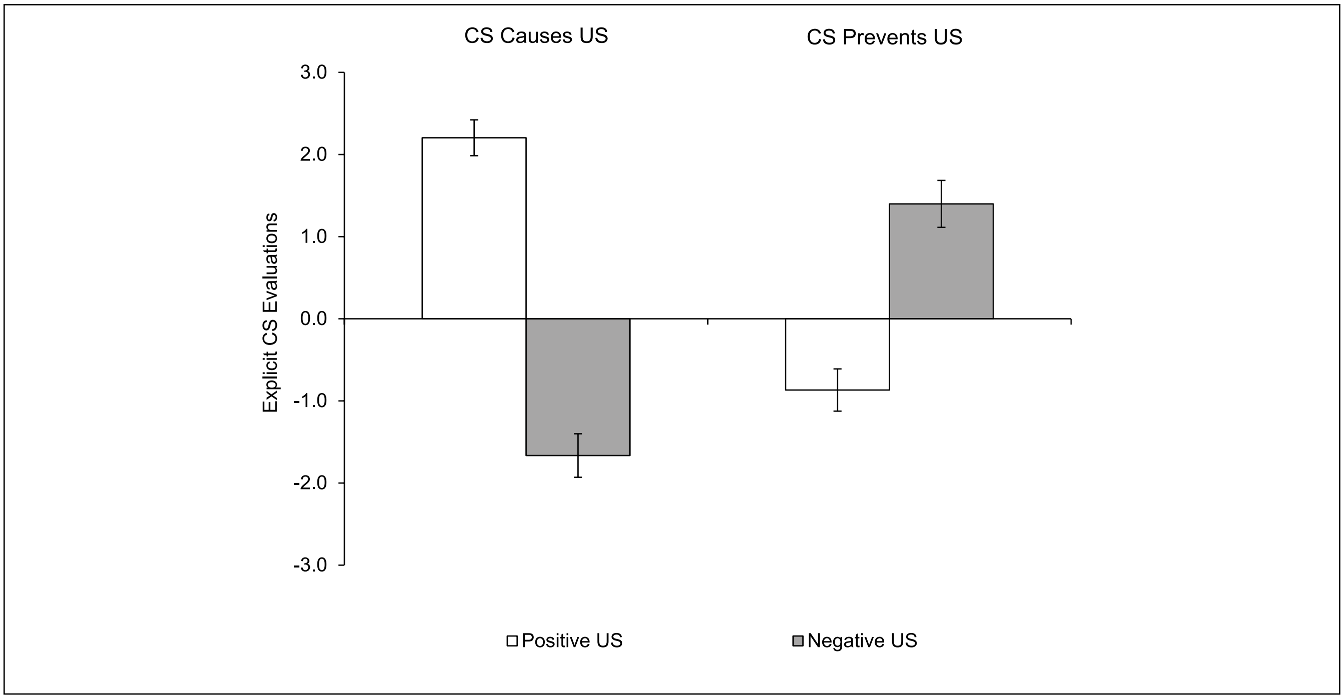

Figure 5. Explicit CS evaluations as a function of US Valence (positive vs. negative) and CS–US Relation (CS causes US vs. CS prevents US), Experiment 3.
Note. Error bars depict 95% confidence intervals. CS = conditioned stimulus; US = unconditioned stimulus.

increased the number of trials in the high-repetition condition from 24 to 35, summing up to a total of 160 trials. On each trial, participants were presented with one of the CSs in the center of the screen for 1,500 ms. Five-hundred milliseconds after the onset of the CS, one of the relational terms *CAUSES* or *PREVENTS* appeared slightly below the CS. After the two stimuli were simultaneously presented for 1,000 ms, they were replaced by the US, which was presented for 1000 ms in the center of the screen. The inter-trial-interval was 1,000 ms. The instructions were similar to the ones in Experiments 1 and 2, the only difference being that participants received the relational information on a trial-by-trial basis during the presentation of the CS–US pairings rather than in the instructions.

Measures. The measures of explicit and implicit evaluation were identical to the ones in Experiments 1 and 2, the only difference being the larger number of trials in the evaluative priming task (i.e., 180 trials) that resulted from the use of nine (instead of five) pharmaceutical products. The measure of recollective memory was not included in the current study.

Results

Explicit evaluations. Baseline-corrected scores of explicit CS evaluations were calculated in line with the procedures of Experiment 1. Means and confidence intervals of explicit evaluations are presented in Table 1. A 2 (US Valence) × 2 (CS–US Repetition) × 2 (CS–US Relation) ANOVA for repeated measures revealed a significant main effect of US Valence, $F(1, 135) = 64.21$, $p < .001$, $\eta_p^2 = .322$, which was qualified by a significant two-way interaction of US Valence and CS–US Relation, $F(1, 135) = 436.79$, $p < .001$, $\eta_p^2 = .764$ (see Figure 5). Specifically, explicit evaluations showed a regular EC effect when the CSs caused the USs, $F(1, 135) = 676.69$, $p < .001$, $\eta_p^2 = .834$, and a reversed EC effect when the CSs prevented the USs, $F(1, 135) = 125.36$, $p < .001$, $\eta_p^2 = .481$. The ANOVA also revealed a significant three-way interaction between US Valence, CS–US Relation, and CS–US Repetition, $F(1, 135) = 37.09$, $p < .001$, $\eta_p^2 = .216$. Further analyses indicated that, when the CSs caused the USs, repetition significantly increased the size of regular EC effects, $F(1, 135) = 31.90$, $p < .001$, $\eta_p^2 = .191$. Conversely, when the CSs prevented the USs, repetition significantly increased the size of reversed EC effects, $F(1, 135) = 14.16$, $p < .001$, $\eta_p^2 = .095$.

Implicit evaluations. Before aggregating the response latency data of the evaluative priming task, we excluded latencies from trials with incorrect responses (4.1%) and truncated latencies higher than 800 ms (see Gawronski et al., 2005, 2015). Baseline-corrected scores of implicit CS evaluations were calculated in line with the procedures of Experiment 1. Means and confidence intervals of implicit evaluations are presented in Table 2. A 2 (US Valence) × 2 (CS–US Repetition) × 2 (CS–US Relation) mixed-model ANOVA revealed

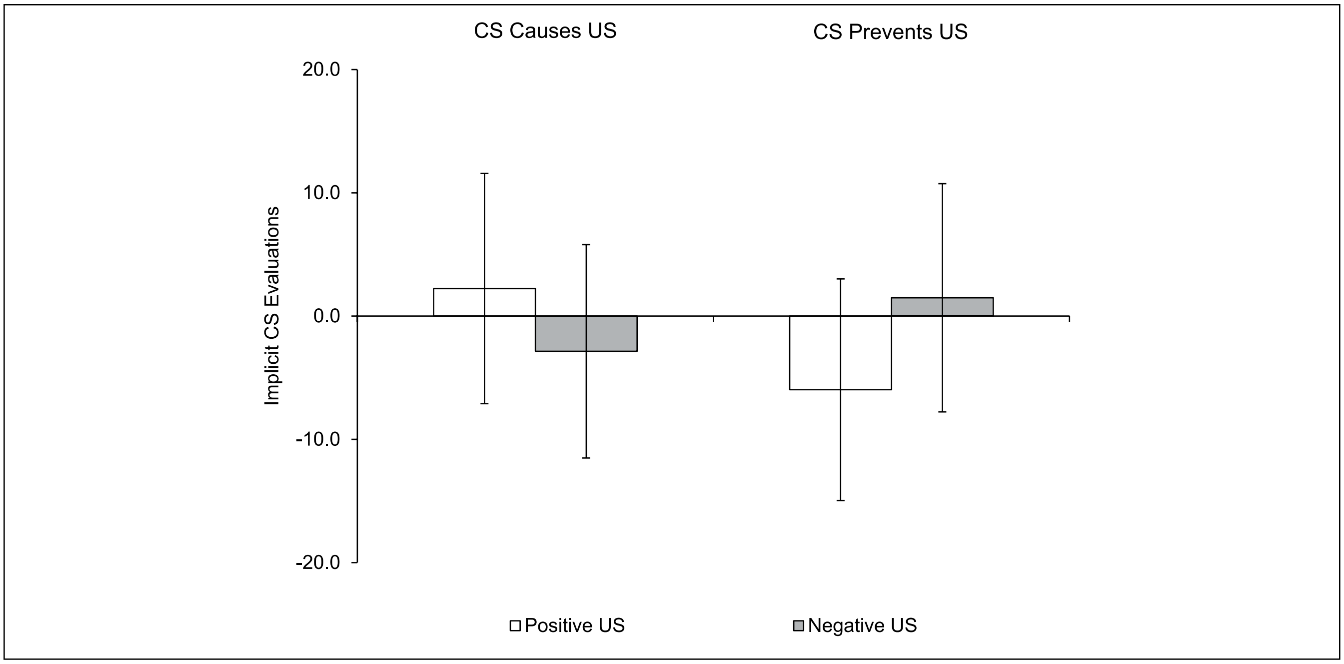

Figure 6. Implicit CS evaluations as a function of US Valence (positive vs. negative) and CS–US Relation (CS causes US vs. CS prevents US), Experiment 3.
Note. Error bars depict 95% confidence intervals. CS = conditioned stimulus; US = unconditioned stimulus.

a significant two-way interaction of US Valence and CS–US Relation, $F(1, 135) = 4.76$, $p = .03$, $\eta_p^2 = .034$ (see Figure 6). Follow-up analysis showed that, when the CSs caused the USs, there was a non-significant pattern of means consistent with a regular EC effect, $F(1, 135) = 1.81$, $p = .18$, $\eta_p^2 = .013$. In contrast, when the CSs prevented the USs, there was a marginally significant reversed EC effect, $F(1, 135) = 2.99$, $p = .09$, $\eta_p^2 = .022$. The three-way interaction of US Valence, CS–US Relation, and CS–US Repetition failed to reach statistical significance, $F(1, 135) = 1.67$, $p = .20$, $\eta_p^2 = .012$. No other main or interaction effect reached statistical significance (all Fs < 1.1, all ps > .30).[6]

Comparison of implicit and explicit evaluation. As with Experiments 1 and 2, we calculated standardized EC scores for each of the two measures and submitted them to a 2 (Measure) × 2 (CS–US Relation) ANOVA for repeated measures. This analysis revealed a significant main effect of CS–US Relation, $F(1, 135) = 163.54$, $p < .001$, $\eta_p^2 = .548$, which was qualified by a significant two-way interaction of Measure and CS–US Relation, $F(1, 135) = 79.32$, $p < .001$, $\eta_p^2 = .37$. Although EC effects on both measures were significantly influenced by CS–US Relation (see above), the effect of CS–US Relation was larger for explicit compared with implicit evaluations.

Discussion

Experiment 3 supports an alternative interpretation that attributes the findings of Experiments 1 and 2 to ineffective integration of relational and co-occurrence information during the encoding of CS–US pairings. Because relational information in the previous two studies was presented before the CS–US pairings, such that it had to be applied to all of the following CS–US pairings, it is possible that the learning processes involved the independent acquisition of two distinct pieces of information: (a) a general rule that all of the CSs either cause or prevent the USs they are paired with, and (b) the specific US a given CS is paired with. To the extent that the two pieces of information are not integrated during the encoding of the CS–US pairings and a post hoc application of the rule during the expression of an evaluative response requires time and cognitive resources, a successful integration may occur only for deliberate evaluative judgments (i.e., explicit evaluations), but not for spontaneous evaluative reactions (i.e., implicit evaluations).

To test this interpretation, Experiment 3 aimed to facilitate the integration of co-occurrence and relational information during the encoding of CS–US pairings. Toward this end, we manipulated relational information as a within-subjects factor, with the respective relations being presented trial-by-trial for each CS–US pair. Counter to the findings of Experiments 1 and 2, this procedural modification led to corresponding effects of relational information on implicit and explicit evaluations. Although the moderating effect of relational information was weaker for implicit compared with explicit evaluations, relational information that the CS prevents the US led to reversed EC effects on both explicit and implicit evaluations. These results suggest that the differential effect of relational information in Experiments 1 and 2 reflects the higher cognitive demands of applying a relational rule to independently acquired co-occurrence information during the expression of an evaluative response. Importantly, because this interpretation does not require any assumptions about two functionally distinct learning mechanisms, it reopens the door for single-process propositional accounts as a viable alternative. In fact, given that (a) the differential effect of relational information in Experiments 1 and 2 can be explained by ineffective integration of co-occurrence and relational information during the encoding of CS–US pairings, (b) relational information qualified EC effects on both implicit and explicit evaluations in the current study, and (c) we did not obtain the predicted effect of CS–US repetition in any of the three studies, one could argue that single-process propositional accounts offer a more parsimonious explanation for the current set of findings than dual-process accounts.

General Discussion

According to single-process propositional accounts of EC, CS evaluations should reflect the evaluative meaning implied by the relation of a CS to a co-occurring US, and this should be the case for both explicit and implicit evaluations. In contrast, dual-process accounts suggest that relational information should qualify EC effects on explicit, but not implicit, evaluations. Previous research has revealed mixed evidence for these predictions, in that some studies confirmed the predictions of single-process propositional accounts (e.g., Gawronski et al., 2005), whereas others supported the predictions of dual-process accounts (e.g., Moran & Bar-Anan, 2013). To reconcile the conflicting evidence, the current research investigated whether frequency of CS–US pairings moderate the impact of relational information on implicit evaluations. This question was based on the assumption that stronger reinforcement with a larger number of CS–US pairings should strengthen the resulting associative links, and thereby increase their impact on implicit evaluations. Thus, to the extent that propositional inferences can have top-down effects on implicit evaluations in the absence of strong associations (Gawronski & Bodenhausen, 2006), unqualified associative effects of CS–US pairings may be limited to conditions of strong reinforcement with large numbers of pairings. Yet, with small numbers of pairings, propositional inferences about CS–US relations may fully override the effects of weak associative links, thereby leading to a moderating effect of relational information on implicit evaluations.

Counter to these predictions, Experiments 1 and 2 did not obtain any effect of CS–US repetition on implicit evaluations. Instead, the two studies found that relational information moderated EC effects on explicit evaluations, whereas implicit evaluations showed a regular EC effect that remained unqualified by relational information. Although the differential effect of relational information on implicit and explicit evaluations could be interpreted as partial support for dual-process accounts, the results of Experiment 3 suggest a more nuanced interpretation. In Experiments 1 and 2, relational informational was presented before the CS–US pairings, such that it had to be applied to all of the following CS–US pairings. Because this procedural feature may lead to independent processing of co-occurrence and relational information, the differential effect of relational information might have been the result of ineffective integration of the two pieces of information during the encoding of CS–US pairings. Consistent with this interpretation, Experiment 3 found that relational information qualified EC effects on both explicit and implicit evaluations when relational information was manipulated as a within-subjects factor, with the respective information being presented trial-by-trial for each CS–US pair. Together with the findings of Experiments 1 and 2, these results suggest that qualifying effects of relational information on implicit evaluations depend on the mental integration of co-occurrence and relational information during the encoding of CS–US pairings. To the extent that the two pieces of information are encoded separately, a post hoc application of relational information during the expression of an evaluative response may require time and cognitive resources, leading to a qualifying effect on explicit, but not implicit, evaluations. Importantly, because this interpretation does not require any assumptions about two functionally distinct learning mechanisms, the current findings can be explained entirely with single-process propositional accounts.

Given that (a) the differential effect of relational information in Experiments 1 and 2 can be explained by ineffective integration of co-occurrence and relational information during the encoding of CS–US pairings, (b) relational information qualified EC effects on implicit evaluations in Experiment 3, and (c) the current studies did not provide any evidence for the predicted effect of CS–US repetition, a major question concerns the implications of these findings for dual-process accounts of EC. In contrast to single-process propositional accounts, which explain the entire pattern of results with one auxiliary assumption about the mental integration of co-occurrence and relational information, dual-process accounts are more difficult to reconcile with the current findings. Nevertheless, they could be saved with a number of additional assumptions. First, the qualifying effect of relational information in Experiment 3 could be explained with the additional assumption that the modified procedure facilitated "top-down" effects of propositional inferences on the formation of associations (see Gawronski & Bodenhausen, 2006, 2011). That is, participants may have inferred the CS valence implied by the relational information, and the rehearsal of this inference on every trial of the task led to the formation of an association between the CS and the inferred valence (for similar findings on the effects of repeated negation, see Deutsch, Gawronski, & Strack, 2006). Second, to the extent that such "top-down" effects of propositional inferences should become stronger as a function of repetition (see Deutsch et al., 2006), they may compensate for repetition effects of mere co-occurrences. That is, repetition may promote the formation of corresponding associations for both (a) observed co-occurrences between a CS and US and (b) the inferred valence of the CS on the basis of relational information. Thus, when the two associations have conflicting evaluative implications (e.g., when the CS prevents the US), the two kinds of repetition effects should compensate each other, and thereby lead to a null effect on implicit evaluations, as observed in the current studies. However, in the absence of empirical evidence for these additional assumptions, a single-process propositional interpretation seems superior, because (a) it requires only one auxiliary assumption to explain the current set of findings and (b) this auxiliary assumption led to a novel prediction that was empirically confirmed in Experiment 3 (see Gawronski & Bodenhausen, 2015).

The current findings expand on related research by Peters and Gawronski (2011), who investigated the impact of another aspect of propositional reasoning on implicit and explicit evaluations: the perceived validity of co-occurrence information. In their study, participants were presented with evaluative statements about four target individuals. For two of the four targets, 75% of the statements were positive and 25% were negative. For the other two targets, 75% of the statements were negative and 25% were positive. Participants' task was to guess whether each statement was correct or incorrect. Orthogonal to the manipulation of valence proportions, participants received feedback on their individual guesses, such that for two of the targets the majority information was always correct and the minority information was always incorrect; for the remaining two targets the feedback suggested that the minority information was correct and the majority information was incorrect.

Counter to the dual-process hypothesis that validity information should influence only explicit, but not implicit, evaluations, Peters and Gawronski found that validity information qualified co-occurrence effects for both explicit and implicit evaluations. A differential effect of validity information occurred only when the presentation of validity information was delayed, but not when it was presented immediately after the encoding of the valence information. The current research expands on these findings, suggesting that the reduced effectiveness of qualifying information in moderating co-occurrence effects on implicit evaluations may not necessarily depend on the delayed processing of the qualifying information. Instead, the critical factor seems to be the mental integration of the two pieces of information, which can be undermined even when the qualifying information is available *before* the encoding of co-occurrence information (see Experiments 1 and 2). Put differently, any disconnection in the processing of co-occurrence and qualifying information may disrupt the mental integration of the two pieces of information regardless of whether the qualifying information is encoded before or after the observed co-occurrence (see also Gawronski et al., 2005; Langer et al., 2009). Yet, when co-occurrence and qualifying information are encoded simultaneously, the qualifying information seems to be effective in influencing implicit evaluations regardless of whether the qualifying information involves information about CS–US relations (see Experiment 3) or the validity of evaluative statements (see Peters & Gawronski, 2011).

By providing deeper insights into the conditions under which relational information moderates mere co-occurrence effects, the current findings also have important implications for applied research. One example concerns the effectiveness of advertisements for products that counteract something negative (e.g., insurance policies, pharmaceutical products). Marketers of such products face the challenge of designing advertisements that do not produce unqualified co-occurrence effects, such that the product becomes mentally linked with the negative event that is supposed to be counteracted by the product (e.g., sunscreen being linked to skin cancer). The current findings suggest that avoiding such co-occurrence effects requires a mental integration of the observed co-occurrence with the qualifying information (e.g., sunscreen prevents skin cancer), which can be disrupted when the two pieces of information are not processed simultaneously. Thus, in advertisements for products that counteract something negative, any factor that disrupts the simultaneous processing of co-occurrence and relational information can lead to counterintentional effects.

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Notes

1. Another feature that has been claimed to distinguish evaluative conditioning (EC) from other forms of Pavlovian conditioning is its independence of contingency awareness. However, counter to earlier claims based on findings with recollective memory tasks, the role of contingency awareness in EC is still unclear due to the scarcity of studies that used experimental designs to manipulate contingency awareness during the encoding of conditional stimulus

(CS)–unconditional stimulus (US) pairings (see Gawronski & Walther, 2012; Sweldens, Corneille, & Yzerbyt, 2014).

2. For the three studies reported in this article, we report all measures, all conditions, and all data exclusions. Based on previous EC studies in our lab, we aimed for at least 100 participants for each cell of the experimental designs. Thus, for the between-subjects manipulation of relational information in Experiments 1 and 2, the predetermined sample size was set to at least 200 participants; for the within-subjects manipulation of relational information in Experiment 3, the predetermined sample size was set to at least 100 participants. Based on the availability of participants during the academic terms of the data collections, all studies include somewhat larger samples. All data were collected in one shot without intermittent statistical analyses.
3. Because measurement order did not produce consistent effects across the three studies, it was not included as a factor in the following analyses. The only significant effect involving measurement order was a significant two-way interaction of Measurement Order and CS–US Repetition for explicit evaluations in Experiment 3, which was independent of US Valence.
4. One participant failed to complete the memory task and was therefore excluded from the analysis of memory data.
5. Three participants failed to complete the memory task and were therefore excluded from the analysis of memory data.
6. Because the two-way interaction effect of US Valence and CS–US Relation on implicit evaluations was rather weak, we conducted additional analyses to ensure that this interaction replicates across different outlier treatments (see Gawronski, Cunningham, LeBel, & Deutsch, 2010). Using a lower cutoff of 300 ms and an upper cutoff of 1,000 ms (e.g., Gawronski, Balas, & Creighton, 2014), the analysis of variance produced a stronger interaction of US Valence and CS–US Relation, $F(1, 135) = 10.11$, $p = .002$, $\eta_p^2 = .070$. Confirming the reliability of the reported effect, there was a significant regular EC effect when the CSs caused the USs, $F(1, 135) = 4.88$, $p = .03$, $\eta_p^2 = .035$, and a significant reversed EC effects when the CSs prevented the USs, $F(1, 135) = 5.36$, $p = .02$, $\eta_p^2 = .038$. For the sake of consistency, our main analysis used the same outlier treatment that was used in Experiments 1 and 2.

Supplemental Material

The online supplemental material is available at http://pspb.sagepub.com/supplemental.

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