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Co-Occurrence and Relational Information in Evaluative Learning: A Multinomial Modeling Approach

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Dual-process theories of evaluative learning suggest that evaluative representations can be formed via two functionally distinct mechanisms: automatic formation of associative links between co-occurring events (associative learning) and nonautomatic generation and truth assessment of mental propositions about the relation between stimuli (propositional learning). Single-process propositional theories reject the idea of automatic association formation, attributing all instances of evaluative learning to propositional processes. A central question in the debate between the two theories concerns the mechanisms underlying unqualified effects of stimulus co-occurrence when the relation between the co-occurring stimuli suggests an evaluation that is opposite to the one implied by the observed co-occurrence (e.g., sunscreen prevents skin cancer). Addressing interpretational ambiguities in previous research on the differential impact of co-occurrence and relational information on implicit and explicit measures, the current research used a multinomial modeling approach to investigate the functional properties of the effects of co-occurrence and relational information on a single measure of evaluative responses. Although the moderating effects obtained for relational information are consistent with the predictions of the two theories, the obtained properties of co-occurrence effects pose an explanatory challenge to both dual-process and single-process propositional theories. The findings demonstrate the value of multinomial modeling in providing deeper insights into the functional properties of the effects of co-occurrence and relational information, which impose stronger empirical constraints on extant theories of evaluative learning.

Keywords: associative learning, dual-process theory, evaluative conditioning, multinomial modeling, propositional learning

In an effort to reduce smoking rates, several countries around the globe have adopted policies that require tobacco companies to print graphic images of negative health effects of smoking on cigarette packages. The idea underlying these policies is that repeated exposure to pairings of cigarettes and unpleasant images may create negative mental associations with cigarettes even when people reject the message implied by the pairings (e.g., when people dismiss the proposition that smoking causes cancer; see Noar et al., 2016). Now, imagine a similar health campaign that aims to promote the use of sunscreen by printing graphic images of skin cancer on sunscreen packaging. Intuitively, one might be skeptical about the effectiveness of such a health campaign. After all, repeated exposure to pairings of sunscreen and unpleasant images may create negative mental associations with sunscreen even when people fully comprehend the positive meaning of the message that sunscreen prevents skin cancer. This idea is consistent with the assumptions of dual-process theories suggesting that observed co-occurrences can create associative links between the co-occurring events in memory regardless of the broader meaning of the observed co-occurrence (e.g., Gawronski & Bodenhausen, 2006; Rydell & McConnell, 2006; Smith & DeCoster, 2000).[1]

Expanding on theoretical controversies regarding the effects of stimulus co-occurrence and stimulus relations on evaluative responses, the current work aims to provide a deeper understanding of their underlying processes using a multinomial modeling ap-

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[1] Conceptually, co-occurrence may be regarded as a particular type of relation instead of a qualitative distinct type of information. For the sake of simplicity, we use the term *co-occurrence* to describe the spatio-temporal co-occurrence of stimuli and the term *relational information* to describe the particular nature of their relation over and above mere co-occurrence.

proach (see Batchelder & Riefer, 1999; Hütter & Klauer, 2016). Compared with earlier research that investigated the relative impact of co-occurrence and relational information on implicit and explicit measures of evaluation (e.g., Gawronski, Walther, & Blank, 2005; Hu, Gawronski, & Balas, 2017; Moran & Bar-Anan, 2013; Zanon, De Houwer, Gast, & Smith, 2014), a major advantage of the current approach is that it quantifies simultaneous effects of co-occurrence and relational information on a single measure of evaluative responses. This aspect resolves ambiguities in the interpretation of dissociations between implicit and explicit measures as being attributable to either (a) processes during the learning of evaluative information or (b) processes during the expression of an evaluative response (see De Houwer, 2018; Gast, Gawronski, & De Houwer, 2012; Gawronski, Balas, & Hu, 2016; Gawronski, Brannon, & Bodenhausen, 2017). Moreover, the roles of learning-related and response-related processes can be investigated more directly by independently manipulating processing conditions during (a) the learning of evaluative information and (b) the expression of an evaluative response. The evidence obtained with this multinomial modeling approach not only imposes tighter empirical constraints for current theoretical debates about the processes underlying the effects of co-occurrence and relational information; it also provides valuable practical information on how to improve the effectiveness of persuasive communication in applied settings.

Evaluative Conditioning

The question of when and how co-occurrence of an object with a valenced stimulus influences evaluative responses to the object is closely related to the notion of evaluative conditioning (EC; for a review, see De Houwer, Thomas, & Baeyens, 2001). For decades, EC researchers have used the term *evaluative conditioning* to refer to three conceptually distinct aspects: (a) the *procedure* of pairing a conditioned stimulus (CS) with a positive or negative unconditioned stimulus (US), (b) the *effect* of CS–US pairings on evaluations of the CS, and (c) the *mechanism* by which CS–US pairings lead to changes in evaluations of the CS. To avoid conceptual confusion resulting from this terminological inconsistency, De Houwer (2007) proposed a formal definition of EC as a behavioral effect: the change in the evaluation of a CS attributable to its pairing with a positive or negative US. This definition is now widely accepted among EC researchers, and has led to substantial empirical and theoretical advances (for reviews, see Gast et al., 2012; Hütter & Fiedler, 2016). One of the most significant developments is the insight that automatic association formation should be regarded as a mechanism that explains EC effects, and this mechanism should be distinguished from EC as a behavioral effect (De Houwer, Gawronski, & Barnes-Holmes, 2013). Although it has been widely assumed that EC effects are mediated by the automatic formation of associative links in memory, a definition of EC as a behavioral effect implies that claims about the mental processes underlying EC are theoretical hypotheses that require empirical support.

In fact, based on the available evidence, some researchers have dismissed the idea of automatic association formation as a mechanism underlying EC effects (e.g., Corneille & Stahl, 2019; De Houwer, 2009, 2014a, 2018; Mitchell, De Houwer, & Lovibond, 2009). As an alternative to associative accounts, De Houwer (2009, 2014a, 2018) suggested that EC effects are mediated by the nonautomatic generation and truth assessment of mental propositions about the relation between a CS and a US. This hypothesis is based on research showing that EC effects depend on the availability of cognitive resources (e.g., Pleyers, Corneille, Yzerbyt, & Luminet, 2009), processing goals (e.g., Corneille, Yzerbyt, Pleyers, & Mussweiler, 2009), and higher-order construals of CS–US relations (e.g., Fiedler & Unkelbach, 2011) during the encoding of CS–US pairings as well as recollective memory for CS–US pairings at the time of judgment (e.g., Pleyers, Corneille, Luminet, & Yzerbyt, 2007). Together, these findings are consistent with the hypothesis that EC effects are mediated by the nonautomatic generation and truth assessment of mental propositions about CS–US relations. Yet, they are inconsistent with the hypothesis that EC effects are attributable to the automatic formation of mental associations.

Despite the positive evidence for the role of nonautomatic propositional processes in EC, the total body of evidence is still mixed and difficult to reconcile with single-process propositional accounts (e.g., Balas & Gawronski, 2012; Gawronski, Balas, & Creighton, 2014; Hütter, Sweldens, Stahl, Unkelbach, & Klauer, 2012; Sweldens, Van Osselaer, & Janiszewski, 2010). To fill this explanatory gap, some researchers have proposed dual-process accounts, which assume that EC effects can be the result of either associative learning or propositional learning, or both (e.g., Gawronski & Bodenhausen, 2011, 2014, 2018). Yet, although the contribution of propositional processes to EC is now widely accepted among EC researchers, the presumed role of automatic association formation is still under debate (for a review, see Corneille & Stahl, 2019). One important question in this debate is whether CS–US pairings can lead to unqualified EC effects even when their relation suggests an evaluation of the CS that is opposite to the valence of the US.

CS–US Co-Occurrence Versus CS–US Relations

A defining feature of propositional learning is that it is sensitive to the particular manner in which two stimuli are related (De Houwer, 2009, 2014a, 2018). Thus, to the extent that EC effects are mediated by propositional learning, information about the relation between a CS and a 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 reduces headaches should lead to a positive evaluation of the product because of its positive effect, rather than a negative evaluation that may result from repeated co-occurrences of the pharmaceutical product and headaches. According to the Integrated Propositional Model (IPM; De Houwer, 2018), EC effects resulting from mere co-occurrence of a CS and a US can be explained by the generation and truth assessment of mental propositions about the co-occurrence of the two stimuli (e.g., *X regularly co-occurs with a negative event*). Yet, if the generated propositions include more complex information about the relation of two co-occurring stimuli (e.g., *X prevents something negative*), CS evaluations should reflect the valence implied by this relation rather than the valence of the co-occurring US.

Dual-process accounts acknowledge the contribution of propositional processes to EC effects, but they additionally propose a second, independent learning mechanism that involves the formation of unqualified associative links between co-occurring stimuli. For example, according to the Associative-Propositional Evaluation (APE) Model (Gawronski & Bodenhausen, 2011, 2014, 2018), effects of relational information require propositional inferences, and therefore are more likely to occur for deliberate evaluative judgments that reflect the outcome of such inferences (i.e., self-reported evaluations captured by explicit measures). In contrast, repeated co-occurrences are claimed to produce unqualified associative links that should influence spontaneous evaluative reactions resulting from the spread of activation between associated concepts (i.e., spontaneous evaluations captured by implicit measures). Thus, whereas single-process propositional accounts such as the IPM predict an effect of relational information on both explicit and implicit measures, dual-process accounts such as the APE model predict a dissociation such that relational information should moderate EC effects on explicit, but not implicit, measures (for a method-focused overview of implicit measures, see Gawronski & De Houwer, 2014).

Consistent with the shared prediction of the two accounts for explicit measures, numerous studies have shown that information about contrastive relations between a CS and a US (e.g., *CS prevents US*; *CS dislikes US*) reverses the impact of CS–US pairings on explicit measures (e.g., Fiedler & Unkelbach, 2011; Förderer & Unkelbach, 2012; Gawronski et al., 2005; Hu et al., 2017; Moran & Bar-Anan, 2013; Zanon et al., 2014). However, the available evidence regarding the conflicting predictions for implicit measures is rather mixed. Whereas some studies found that contrastive relations led to a full reversal of EC effects on implicit measures (e.g., Gawronski et al., 2005, Experiment 1; Hu et al., 2017, Experiment 3), other studies found only attenuated, but not reversed, EC effects (e.g., Zanon, De Houwer, & Gast, 2012; Zanon et al., 2014) or unqualified EC effects (e.g., Gawronski et al., 2005, Experiment 2; Hu et al., 2017, Experiments 1 & 2; Moran & Bar-Anan, 2013).

Although the conflicting findings can be reconciled with both accounts by means of several ad hoc assumptions (see Hu et al., 2017), a major obstacle in their interpretation is that different outcomes on implicit and explicit measures could be attributable to either (a) processes during the formation of evaluative representations or (b) processes during the expression of an evaluative response (see De Houwer, 2018; Gast et al., 2012; Gawronski et al., 2016). On the one hand, it is possible that differential effects of co-occurrence and relational information on implicit and explicit measures reflect the operation of two functionally distinct learning mechanisms (see Gawronski & Bodenhausen, 2011, 2014, 2018). On the other hand, differential effects of co-occurrence and relational information on implicit and explicit measures could reflect differences in the retrieval of stored relational information during the expression of an evaluative response (see De Houwer, 2014b, 2018). In line with the latter argument, impaired processing conditions during the expression of an evaluative response may lead to unqualified EC effects as a result of incomplete retrieval of stored information about CS–US relations (e.g., retrieval of *X is related to something negative* instead of *X prevents something negative*) rather than two functionally distinct learning mechanisms (Van Dessel, Gawronski, & De Houwer, in press). In this case, CS–US relations may influence evaluative responses only on measures that provide sufficient time and resources for a complete retrieval of stored relational information (as it is the case for explicit measures). However, effects of CS–US relations may be reduced or eliminated on measures that capture speeded responses under time pressure (as it is the case for implicit measures).[2]

To clearly distinguish between learning-related and response-related effects, it is necessary to (a) use a method that allows researchers to quantify effects of CS–US co-occurrences and CS–US relations within the same measure and (b) investigate their sensitivity to experimental manipulations during the encoding of CS–US pairings and the measurement of evaluative responses to the CS.[3] The main goal of the current work was to develop and utilize such a method to gain deeper insights into the effects of co-occurrence and relational information on evaluative responses.

A Multinomial Model of Co-Occurrence and Relational Effects

Multinomial modeling is a variant of statistical modeling designed to disentangle the unique contributions of multiple processes to overt responses (Batchelder & Riefer, 1999; Hütter & Klauer, 2016). In research on EC, multinomial modeling has been used to investigate the role of recollective memory for CS–US pairings (e.g., Hütter & Sweldens, 2013; Hütter et al., 2012) and the controllability of EC effects on evaluative judgments (Hütter & Sweldens, 2018). A shared feature of this earlier work is that it aimed to disentangle automatic versus nonautomatic processes by means of distinct model parameters (e.g., conscious vs. unconscious; controllable vs. uncontrollable). However, because this work has focused exclusively on effects of CS–US pairings, it remains silent about the more fundamental role of mere co-occurrence and relational information that is central to the debate between dual-process and single-process propositional accounts (see also Hütter & De Houwer, 2017; Mierop, Hütter, & Corneille, 2017). To gain deeper insights into effects of CS–US co-occurrences and CS–US relations, the current work used a multinomial modeling approach to disentangle the two kinds of effects, investigating the properties of their underlying processes by manipulating processing conditions during the learning of evaluative information and the expression of an evaluative response. To illustrate the mathematical underpinnings of multinomial modeling

[2] An alternative interpretation is that (a) people generate and store two propositions for the same event, one capturing relational information (e.g., *X prevents something negative*) and one capturing co-occurrence information (e.g., *X co-occurs with something negative*), and (b) propositions capturing co-occurrence information can be processed automatically because of their low level of complexity (De Houwer, 2014a, 2014b). Because such an interpretation would make single-process propositional accounts empirically indistinguishable from dual-process accounts, it is not considered in the derivation of predictions for the current studies. Another possibility suggested by the IPM (De Houwer, 2018) is that people generate and store abstract evaluative propositions about the CS (e.g., *X is good*) instead of episodic propositions about CS–US relations (e.g., *X prevents something negative*). Although such a hypothesis may seem plausible, it is unable to explain effects of CS–US co-occurrence as a result of incomplete retrieval of relational information, because abstract evaluative propositions about the CS do not contain any information about CS-US relations.

[3] It is worth noting that, although manipulations during the measurement of evaluative responses can influence evaluative responses only via response-related processes, manipulations during encoding may influence evaluative responses via learning-related or response-related processes (for a discussion, see Gawronski et al., 2016).

and its application to the current question, we first describe the experimental paradigm of our studies and then explain the application of multinomial modeling to analyze the data obtained in this paradigm.

The experimental paradigm is based on a procedure by Hu et al. (2017, Experiment 3), involving repeated pairings of pharmaceutical products (CS) with images of positive or negative health conditions (US). For half of the pairings, participants receive information that the pharmaceutical product causes the depicted health condition; for the remaining half, participants receive information that the pharmaceutical product prevents the depicted health condition. Participants' task is to form an impression of the pharmaceutical products based on the presented information. After the presentation of the image pairs, participants are presented with the pharmaceutical products one-by-one and asked to indicate whether or not they would choose the product (*yes* vs. *no*).[4] By aggregating responses across participants and stimuli, the data obtained with this paradigm provide statistical probabilities for choosing the products as a function of US valence (positive vs. negative) and CS–US relation (cause vs. prevent). These probabilities can be analyzed by means of multinomial modeling to quantify the extent to which responses in the choice task were influenced by CS–US co-occurrence and CS–US relations.

The logic of multinomial modeling can be illustrated with a processing tree that depicts the respective outcomes for each of the four stimulus conditions (i.e., CS causes positive US; CS prevents positive US; CS causes negative US; CS prevents negative US) as a function of whether CS–US co-occurrence and CS–US relation determine the observed choice on a given trial (see Figure 1). To control for general response biases, the processing tree also captures the two potential cases that participants' choices reflect a general positivity bias or a general negativity bias regardless of US valence and CS–US relation. The four paths on the left side of the figure depict the four potential cases that (a) a given response is driven by the CS–US relation (captured by R), (b) a given response is driven by the CS–US co-occurrence (captured by C), (c) a given response is driven by a general positivity bias (captured by B), and (d) a given response is driven by a general negativity bias (captured by $1 - B$). The table on the right side of the figure depicts the predicted response patterns for each of the four cases as a function of US valence and CS–US relation.

If the response to a given CS is driven by the CS–US relation, participants should respond *yes* when the CS causes a positive US or when the CS prevents a negative US, and participants should respond *no* when the CS prevents a positive US or when the CS causes a negative US (first path in Figure 1). If the response to a given CS is not driven by the CS–US relation but its mere co-occurrence with the US, participants should respond *yes* when the CS was paired with a positive US and they should respond *no* when the CS was paired with a negative US regardless of the CS–US relation (second path in Figure 1). If the response to a given CS is driven by a general positivity bias, participants should respond *yes* regardless of the valence of the US and regardless of whether the CS causes or prevents the US (third path in Figure 1). Conversely, if the response to a given CS is driven by a general negativity bias, participants should respond *no* regardless of the valence of the US and regardless of whether the CS causes or prevents the US (fourth path in Figure 1).

By means of the processing paths depicted in Figure 1, it is possible to generate mathematical equations that delineate the probability of a particular choice in each of the four stimulus conditions (i.e., CS causes positive US; CS prevents positive US; CS causes negative US; CS prevents negative US) as a function of CS–US relations, CS–US occurrences, and general response biases. For example, the probability of a *yes* response for CSs that prevent a positive US is represented by the cases where (a) CS–US co-occurrence drives the response when the CS–US relation does not drive the response $(1 - R) \times C$, and (b) a general positivity bias drives the response when neither the CS–US relation nor CS–US co-occurrence drive the response $(1 - R) \times (1 - C) \times B$. In algebraic terms, this probability is represented by the equation:

$$p(\text{yes}|\text{prevent, positive}) = [(1-R) \times C] + [(1-R) \times (1-C) \times B]$$

Conversely, the probability of a *no* response for CSs that prevent a positive US is represented by the cases where (a) the CS–US relation drives the response, R, and (b) a general negativity bias drives the response when neither the CS–US relation nor the CS–US co-occurrence drives the response $(1 - R) \times (1 - C) \times (1 - B)$. In algebraic terms, this probability is represented by the equation:

$$p(\text{no}|\text{prevent, positive}) = R + [(1-R) \times (1-C) \times (1-B)]$$

Although the equations for *yes* and *no* responses can be derived independently from the processing tree, it is important to note that the probabilities of *yes* and *no* responses within a given stimulus condition are statistically redundant, in that the probability of a *no* response is equal to 1 minus the probability of a *yes* response, and vice versa. Thus, when the same logic is applied to the four stimulus conditions depicted in the table on the right side of the figure, it is possible to derive four nonredundant equations with three unknowns: (a) the unknown R reflects the size of CS–US relation effects, (b) the unknown C reflects the size of CS–US co-occurrence effects, and (c) the unknown B reflects the size and direction of a general response bias (see Appendix). Based on the labels of the three parameters, we call this multinomial processing tree model the *RCB model*.

Using the empirically observed probabilities of participants' *yes* and *no* responses within each of the four experimental conditions, multinomial modeling identifies parameter estimates for each of the three unknowns by means of maximum likelihood statistics. Specifically, multinomial modeling involves systematic adjustments in the parameter values to minimize the differences between the actual probabilities of observed responses and the probabilities predicted by the model equations. The deviation between actual and predicted probabilities serves as the basis for statistical tests of goodness-of-fit, which provides evidence regarding the validity of the model in describing the data. If the deviation between actual and predicted probabilities is small, fit statistics will reveal a nonsignificant deviation between the two, suggesting that the model accurately describes the data. If, however, the deviation

[4] Although evaluations may not always translate into choices (see Kruglanski et al., 2015), the choices in the current paradigm can be interpreted as downstream outcomes of experimentally manipulated CS evaluations. The choice measure was preferred over a simple evaluation measure to illustrate the practical significance of the effects of co-occurrence and relational information.

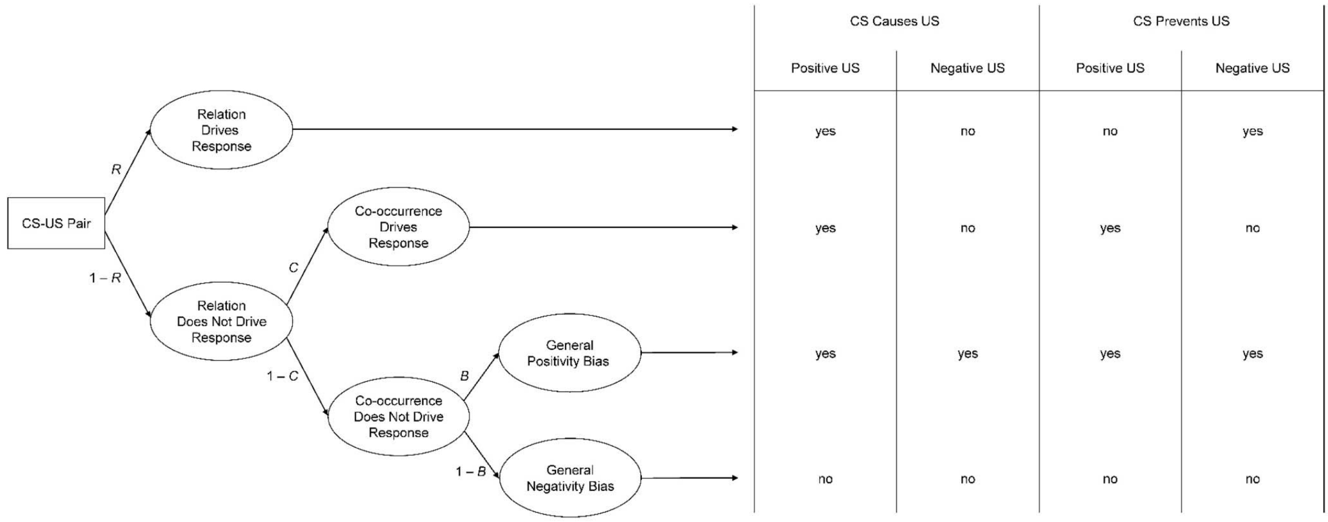

Figure 1. Multinomial processing tree depicting effects of CS–US relation, CS–US co-occurrence, and general response biases on evaluative decisions (positive vs. negative) for CSs that cause or prevent either positive or negative USs.

between actual and predicted probabilities is large, fit statistics will reveal a significant deviation between the two, indicating that the model does not accurately describe the data.

Differences in parameter estimates across groups (e.g., experimental groups under high vs. low time pressure to respond) can be tested by enforcing equal estimates for a given parameter across groups. If setting a given parameter equal across groups leads to a significant reduction in model fit, it can be inferred that the parameter estimates for the two groups are significantly different. If setting a given parameter equal across groups does not lead to a significant reduction in model fit, the parameters for the two groups are not significantly different from each other. Similar tests can be conducted to investigate whether a given parameter estimate significantly differs from a reference value. For example, to test the overall effect of CS–US co-occurrence on choice responses, the *C* parameter is set equal to zero and the resulting model fit is compared with the fit of the model that does not include any restrictions for the *C* parameter. To the extent that enforcing a parameter estimate of zero leads to a significant reduction in model fit, it can be inferred that CS–US co-occurrence significantly influenced participants' responses in the choice task. The same approach can be used to test the influence of CS–US relations captured by the *R* parameter. For the *B* parameter, comparisons with reference values are equivalent, except that the reference value reflecting the absence of a general response bias is 0.5. Whereas values higher than 0.5 reflect a general bias to respond *yes*, values lower than 0.5 reflect a general bias to respond *no*.

For the purpose of the current research, it is important to note that the *C* and the *R* parameters do not provide direct reflections of associative and propositional learning mechanisms (see De Houwer et al., 2013). They simply reflect the strength of CS–US co-occurrence and CS–US relation effects on choice responses, which could be mediated by either associative learning or propositional learning, or both. The major difference between dual-process and single-process propositional theories is that they imply different predictions about the moderators of CS–US co-occurrence and CS–US relation effects. In this sense, the three parameters of the RCB model reflect stimulus-response relations at the functional level of analysis, whereas the assumptions of dual-process and single-process theories refer to their underlying mental processes at the cognitive level of analysis (see De Houwer, 2011; De Houwer et al., 2013).[5] A major advantage of the RCB model is that it permits stringent tests of competing predictions that do not suffer from the interpretational ambiguities of previous research using implicit and explicit measures (e.g., Gawronski et al., 2005; Hu et al., 2017; Moran & Bar-Anan, 2013; Zanon et al., 2014).

Another advantage of the RCB model is that it overcomes interpretational ambiguities of traditional data analytic approaches. Using analysis of variance (ANOVA), one could argue that mere co-occurrence effects are captured by the main effect of US valence and effects of relational information are captured by the two-way interaction of US valence and CS–US relation (e.g., Moran, Bar-Anan, & Nosek, 2016). However, to the extent that the two-way interaction of US valence and CS–US relation is statis-

[5] An important difference between functional and cognitive interpretations of the parameters obtained by multinomial models is that functional interpretations are exclusively based on the stimulus-response relations described by the model, whereas cognitive interpretations include additional assumptions about the mental processes underlying the stimulus-response relation captured by a given parameter. Thus, cognitive interpretations require independent validation of these assumptions, verifying that the parameters indeed reflect the presumed underlying mental processes (Klauer, 2015). Functional interpretations do not require such construct validations, because the conceptual meaning of a given parameter is exclusively based on the stimulus–response relations specified by the model (e.g., the descriptive interpretation of the *C* parameter as an indicator of co-occurrence effects is based on the fact that it captures cases where CSs co-occurring with positive USs lead to positive responses to the CSs and CSs co-occurring with negative USs lead to negative responses to the CSs; see Figure 1).

tically significant, a significant main effect of US valence becomes difficult to interpret, because the interaction indicates that the main effect is conditional upon CS–US relation. In general, a significant interaction undermines unambiguous interpretations of its lower-order main effects (Kirk, 1982), which poses a challenge to the idea that the main effect of US valence could be used as an indicator for mere co-occurrence effects in cases where the main effect of US valence is qualified by CS–US relation. This issue becomes even murkier in cases involving significant higher-order interactions with a third factor (e.g., cognitive load during learning). In such cases, a lower-order interaction between US valence and the third factor would be difficult to interpret, because it may reflect either (a) a moderating influence of the third factor on the effect of mere co-occurrence or (b) a moderating influence of the third factor on the effect of relational information (or both). The RCB model overcomes these ambiguities by estimating the strength of co-occurrence effects in a manner that is conditional upon the absence of an effect of relational information (see Figure 1). This hierarchical approach allows one to (a) simultaneously estimate effects of co-occurrence and relational information via two distinct parameters and (b) test independent effects of other variables on each of the two parameters.

The Current Research

To demonstrate the insights that can be gained from studying effects of co-occurrence and relational information with the RCB model, we present the results of six studies. Experiment 1 tested (a) whether the RCB model provides an accurate description of choice responses in Hu et al.'s (2017, Experiment 3) paradigm and (b) whether choice responses are significantly influenced by both CS–US co-occurrence and CS–US relations. To gain deeper insights into the mechanisms underlying effects of co-occurrence and relational information, Experiments 2a and 2b investigated the influence of time during encoding; Experiment 3 tested the influence of stimulus repetition during encoding. Expanding on the argument that differential effects of co-occurrence and relational information may depend on response-related rather than learning-related processes (see De Houwer, 2014a, 2014b, 2018), Experiment 4 tested the influence of time during judgment; Experiment 5 test the influence of temporal delay between encoding and judgment.

To assess the goodness-of-fit of the RCB model in describing the data and to calculate estimates for the three parameters, we used the free software multiTree by Moshagen (2010) and the statistical software R (Version 3.4.0; R Core Team, 2017).[6] In addition to goodness-of-fit statistics and estimates for the three parameters, these analysis tools provide standard errors and 95% confidence intervals for the estimated parameter values. All of the reported studies used the same estimation algorithm with random start values drawn from a uniform distribution. With the single processing condition in Experiment 1, the model has four free categories (i.e., responses to four types of CSs) and three parameters, which result in a difference of 1 for the degrees of freedom. With two processing conditions in each of the remaining studies, the model has eight free categories (i.e., responses to four combinations of US valence and CS–US relations for each of the two processing conditions) and six parameters (i.e., three parameters estimated for each of the two processing conditions), which result in a difference of 2 for the degrees of freedom. A zip-file with a multiTree template and a tutorial on how to analyze data with the RCB model are available at: http://www.bertramgawronski.com/documents/RCB-Model_Materials.zip. In addition to the tutorial and the multiTree template file, the zip-file also includes template files for studies with our experimental paradigm using the psychological lab software Inquisit by Millisecond and MediaLab/DirectRT by Empirisoft.[7]

For the single-condition design in Experiment 1, we aimed to recruit 100 participants; for the three studies using a between-subjects manipulation of two processing conditions (Experiments 2a, 2b, and 4), we aimed to recruit 400 participants; and for the two studies using a within-subjects manipulation of two processing conditions, we aimed to recruit 200 participants (Experiments 3 and 5). Based on a meta-analytic effect size for EC effects of $d = 0.52$ (Hofmann, De Houwer, Perugini, Baeyens, & Crombez, 2010), these samples provide a power greater than 99% to detect a significant EC effect in a traditional t test for dependent means (two-tailed).[8] For our manipulations of processing conditions, a sample size of $N = 400$ in the studies involving a between-subjects manipulation provides a power of 85% to detect a small moderating effect of $d = 0.30$ in a traditional t test for independent means (two-tailed); a sample size of $N = 200$ in the studies involving a within-subjects manipulation provides a power of 99% to detect a small moderating effect of $d = 0.30$ in a traditional t test for dependent means (two-tailed). By default, we excluded all participants who failed to pass an instructional manipulation check (see Oppenheimer, Meyvis, & Davidenko, 2009) or reported that they did not pay attention to the stimuli or did not take their responses seriously (see Aust, Diedenhofen, Ullrich, & Musch, 2013). The data for each study were collected in one shot without intermittent statistical analyses. We report all measures, all conditions, and all data exclusions. The materials, raw data, and analysis files for all studies are publicly available at https://osf.io/7ac4d/. The reported studies have been approved by the Institutional Review Board of the University of Texas at Austin under protocol # 2016–11-0092.

Experiment 1

The main goal of Experiment 1 was to test whether the RCB model provides an accurate description of choice responses in Hu et al.'s (2017, Experiment 3) paradigm (i.e., does the model fit the data?). In addition, we investigated whether responses in the choice task are significantly influenced by both CS–US co-occurrence and CS–US relations (i.e., are the C and the R parameter significantly greater than zero?).

[6] The software multiTree can be downloaded for free at: http://psycho3.uni-mannheim.de/Home/Research/Software/multiTree/. For the analyses using R, we also used the R-packages afex (Version 0.17.8; Singmann, Bolker, Westfall, & Aust, 2017), MPTinR (Version 1.10.3; Singmann & Kellen, 2013), and papaja (Version 0.1.0.9709; Aust & Barth, 2017).

[7] The zip-file with these materials is also available at https://osf.io/7ac4d/.

[8] Because power analyses within multinomial modeling require simulations with expected population values for the three parameters and we did not have any empirical basis for the required population values prior to running the reported studies, we based our a priori power analyses on simple comparisons of mean values using t tests.

Method

Participants and design. Participants were recruited for a study entitled “How Do We Form Impressions of Novel Objects?” via Amazon’s Mechanical Turk (MTurk). Eligibility for participation was limited to MTurk workers in the United States with a HIT approval rate of at least 95%. Participants received compensation of $1.00 for completing the study. Of the 111 MTurk workers who initially began the study, 100 completed all measures.[9] Two participants failed to pass an instructional manipulation check (see below); one participant reported not paying attention to the images during the learning task (see below); one participant reported that the task did not work properly; and one participant reported that they pressed the wrong key in the choice task. Data from these participants were excluded from the statistical analyses, leaving us with a final sample of 95 participants (51 women, 44 men, $M_{\text{age}} =$ 37.54, $SD_{\text{age}} = 12.16$). The study used a 2 (US Valence: positive vs. negative) × 2 (CS–US Relation: causes vs. prevents) design, with both factors being manipulated within participants.

Materials. For the CSs, we created 12 images of hypothetical pharmaceutical products. As USs, we used six positive and six negative images from various sources, including the International Affective Picture System (Lang, Bradley, & Cuthbert, 2008) and Google Internet searches (see https://osf.io/7ac4d). The positive images depicted healthy physical conditions (i.e., a woman with voluminous hair, an elderly couple on bicycles); the negative images depicted unhealthy physical conditions (i.e., skin rash, tooth decay).

Learning task. Participants were told that the study investigates how people form first impressions of novel objects. Based on the procedures by Hu et al. (2017, Experiment 3), participants received the following instructions:

> In this study, you will be presented with images of pharmaceutical products and visual information about their effects. As you know, many pharmaceutical products have positive effects, but some products also have negative side effects. For each product, you will see whether this product causes or prevents a health outcome. Your task is to think of the image pairs, such that the pharmaceutical product CAUSES or PREVENTS what is displayed in the other photograph. For example, if a product is paired with a positive image, and it says “causes,” you should think of the product in terms of it causing the positive outcome displayed in the image. Conversely, if a product is paired with a negative image, and it says “causes,” you should think of the product in terms of it causing the negative outcome displayed in the image. If a product is paired with a positive image, and it says “prevents,” you should think of the product in terms of it preventing the positive outcome displayed in the image. Conversely, if a product is paired with a negative image, and it says “prevents,” you should think of the product in terms of it preventing the negative outcome displayed in the image. Again, please think of the image pairs in terms of the relation mentioned on the screen (causes or prevents).

On each trial of the learning task, a CS was presented on the left and a US on the right, with one of the two qualifiers *causes* or *prevents* being presented in the center of the screen between the two images. Each stimulus combination was presented for 3000 ms with an intertrial interval of 1000 ms. Three CSs were presented with a positive US and the qualifier *causes*; three CSs were presented with a negative US and the qualifier *causes*; three CSs were presented with a positive US and the qualifier *prevents*; and three CSs were presented with a negative US and the qualifier *prevents*. For each participant, the same CS was always presented together with the same US. The use of a given CS for pairings with positive versus negative USs and the qualifiers *causes* versus *prevents* was counterbalanced by means of a Latin square. The learning phase consisted of four blocks with self-paced breaks between blocks. Within each block, each CS–US-qualifier combination was presented twice, summing up to eight presentations of each stimulus combination over the four blocks. Thus, with 12 unique CS–US-qualifier combinations, the learning task included a total of 96 trials.

Choice task. After the learning task, participants completed a speeded choice task in which they were asked to indicate whether or not they would chose a given product by selecting one of two response options (*no* = A; *yes* = K). On each trial of the task, a CS was shown in the center of the screen, and participants had 1000 ms to indicate whether or not they would choose the presented product. If participants did not respond within the 1000 ms response window, the message *Too slow* was displayed in red in the center of the screen for 750 ms. Only valid responses within the 1000 ms response window were used in the analysis. Each trial started with a blank screen for 100 ms, followed by a 900 ms fixation cross in the center of the screen. During the 1000 ms presentation of a given CS, labels for the two response options were displayed on the bottom-left side (*no* = A) and bottom-right side (*yes* = K) of the screen, with the question *Would you choose this product?* being displayed slightly below the CS. The choice task included three blocks, with each CS being presented once in each block, summing up to a total of 36 trials. The order of CSs within each block was randomized separately for each participant.

Evaluating ratings. After the speeded choice task, participants were asked to evaluate each CS on 7-point rating scales ranging from 1 (*very negative*) to 7 (*very positive*). The order of CSs in the rating task was randomized individually for each participant. The rating measure was included for exploratory purposes to compare the results of the choice task with the results of a traditional measure of CS evaluations.

Additional measures. At the end of the study, participants were asked to complete a one-item instructional manipulation check (Oppenheimer et al., 2009). The measure included the following instructions:

> Most modern theories of decision-making recognize the fact that decisions do not take place in a vacuum. Individual preferences and knowledge, along with situational variables can greatly impact the decision process. To facilitate our research on decision-making we are interested in knowing certain factors about you, the decision maker. Specifically, we are interested in whether you actually take the time to read the directions; if not, then some of our manipulations that rely on changes in the instructions will be ineffective. So, to demonstrate that you have read the instructions, please ignore the sports items below. Instead, simply continue on to the next page after the options. Thank you very much.

Below the instructions, participants were presented with the question *Which of these activities do you engage in regularly? (check all that apply)* and the response options: *Football, Soccer,*

[9] One participant completed the study twice. The second data set of this participant was not included in the analysis.

Dancing, Watersports, Triathlon, Running, Volleyball. By default, we excluded all participants from the analyses who, counter to the instructions, checked one or more of the response options on this item. After the instructional manipulation check, participants were asked to report their gender, age, and ethnicity. The demographic questions were followed by two questions asking participants (a) whether they paid attention to the images presented throughout the task and (b) whether they took their responses in the study seriously. Participants were informed that their responses on these two items would not affect their compensation. By default, we excluded all participants from the analyses who reported that they did not pay attention to the images or did not take their responses seriously (see Aust et al., 2013).

Results

Evaluative ratings. The rating data were aggregated by averaging the responses for the three CSs within each of the four stimulus categories, implied by the manipulations of US Valence and CS–US Relation. Submitted to a 2 (US Valence) × 2 (CS–US Relation) ANOVA for repeated measures, rating scores revealed a significant main effect of US Valence, $F(1, 94) = 23.90$, $p < .001$, $\eta^2_G = .045$, which was qualified by a significant two-way interaction between US Valence and CS–US Relation, $F(1, 94) = 115.66$, $p < .001$, $\eta^2_G = .331$. Post hoc tests showed that, when the CSs were described as causing the USs, CSs paired with positive USs were evaluated more positively than CSs paired with negative USs (Ms = 5.05 vs. 2.63, respectively), $t(94) = 12.36$, $p < .001$, $d = 1.27$. Conversely, when the CSs were described as preventing the USs, CSs paired with positive USs were rated less positively than CSs paired with negative USs (Ms = 3.34 vs. 4.61, respectively), $t(94) = 5.82$, $p < .001$, $d = 0.60$. Moreover, when the CSs were paired with positive USs, CSs that were described as causing the USs were evaluated more positively than CSs that were described as preventing the USs, $t(94) = 8.99$, $p < .001$, $d = 0.92$. Conversely, when the CSs were paired with negative USs, CSs that were described as causing the USs were evaluated less positively than CSs that were described as preventing the USs, $t(94) = 9.63$, $p < .001$, $d = 0.99$.

Choice responses. The speeded choice data were aggregated by calculating the relative proportions of *yes* versus *no* responses for each of the four categories of CSs. Submitted to the same 2 (US Valence) × 2 (CS–US Relation) ANOVA, the choice data revealed a significant main effect of US Valence, $F(1, 94) = 6.78$, $p = .011$, $\eta^2_G = .014$, which was qualified by a significant two-way interaction between US Valence and CS–US Relation, $F(1, 94) = 24.63$, $p < .001$, $\eta^2_G = .071$ (see Table 1). Post hoc tests showed that, when the CSs were described as causing the USs, CSs paired with positive USs were chosen more frequently than CSs paired with negative USs, $t(94) = 5.31$, $p < .001$, $d = 0.54$. Conversely, when the CSs were described as preventing the USs, CSs paired with positive USs tended to be chosen less frequently than CSs paired with negative USs $t(94) = 2.24$, $p = .027$, $d = 0.23$. Moreover, when the CSs were paired with positive USs, CSs that were described as causing the USs were chosen more frequently than CSs that were described as preventing the USs, $t(94) = 3.51$, $p < .001$, $d = 0.36$. Conversely, when the CSs were paired with negative USs, CSs that were described as causing the USs were chosen less frequently than CSs that were described as preventing the USs, $t(94) = 4.61$, $p < .001$, $d = 0.47$.

RCB model. The ANOVA results for both evaluative ratings and speeded choices suggest that relational information fully qualified the effects of US valence. However, more nuanced insights were gained when the speeded-choice data were analyzed with the RCB model. Overall, the model fit the data well with three free parameters, $G^2(1) = 1.33$, $p = .248$ (see Table 2). Consistent with the ANOVA results, the R parameter was significantly greater than zero, $\Delta G^2(1) = 68.53$, $p < .001$, indicating that relational information significantly influenced responses on the task. The C

Table 1
Mean Probabilities and 95% Confidence Intervals of Choice Responses for CSs That Cause or Prevent a Positive or Negative US

	CS causes US				CS prevents US			
	Positive US		Negative US		Positive US		Negative US	
Experiment	M	95% CI	M	95% CI	M	95% CI	M	95% CI
Experiment 1								
Baseline	.61	[.56, .67]	.40	[.34, .46]	.48	[.43, .54]	.57	[.51, .62]
Experiment 2a								
2000 ms (encoding)	.56	[.52, .60]	.34	[.30, .37]	.42	[.38, .46]	.50	[.47, .54]
5000 ms (encoding)	.57	[.53, .62]	.32	[.28, .36]	.41	[.36, .45]	.50	[.46, .55]
Experiment 2b								
1000 ms (encoding)	.56	[.52, .60]	.35	[.31, .39]	.47	[.43, .51]	.50	[.46, .54]
5000 ms (encoding)	.59	[.54, .63]	.33	[.29, .37]	.43	[.38, .47]	.50	[.46, .55]
Experiment 3								
4 repetitions	.60	[.54, .65]	.34	[.29, .40]	.37	[.32, .43]	.48	[.43, .54]
24 repetitions	.68	[.62, .73]	.30	[.25, .35]	.41	[.36, .47]	.58	[.52, .63]
Experiment 4								
750 ms (judgment)	.53	[.49, .57]	.39	[.35, .42]	.43	[.39, .46]	.48	[.44, .52]
2500 ms (judgment)	.65	[.61, .69]	.26	[.22, .30]	.38	[.34, .43]	.56	[.52, .60]
Experiment 5								
Immediate	.60	[.56, .64]	.34	[.30, .38]	.44	[.40, .48]	.52	[.48, .56]
2-day delay	.52	[.48, .57]	.33	[.29, .37]	.42	[.37, .46]	.48	[.44, .52]

Note. Higher scores reflect higher probabilities of yes versus no responses.

Table 2
Parameter Estimates Without Model Restrictions, Experiment 1

Parameter	Estimate	95% CI	*p*
R	.15	[.11, .18]	<.001
C	.08	[.04, .12]	<.001
B	.52	[.50, .54]	.110

Note. The *R* parameter captures effects of relational information; the *C* parameter captures effects of co-occurrence; the *B* parameter captures general response biases. The *p* values refer to differences between parameter estimates and neutral reference points. The neutral reference point for *R* and *C* is 0; the neutral reference point for *B* is .5, with scores higher than .5 reflecting a general bias toward positive responses and scores lower than .5 reflecting a general bias toward negative responses.

parameter was significantly greater than zero as well, $\Delta G^2(1) = 13.37$, $p < .001$, indicating that mere co-occurrence with a positive or negative US influenced responses to the CSs over and above relational information. The *B* parameter did not significantly differ from its reference point of 0.5, $\Delta G^2(1) = 2.56$, $p = .110$, indicating that, on average, participants did not show a general response tendency to choose or reject the products.

Discussion

The results of Experiment 1 support the validity of the RCB model in describing responses in the employed paradigm (see Hu et al., 2017, Experiment 3), in that the probabilities of choices predicted by the model did not significantly differ from the probabilities of observed choices (i.e., the model fit the data). Moreover, both the *C* and the *R* parameter were significantly greater than zero, indicating that responses in the choice task were influenced by both CS–US co-occurrence and CS–US relations. Traditional analyses using ANOVA provided a less nuanced picture, suggesting that relational information qualified the effects of US valence on both evaluative ratings and speeded choices. Together, these results support the validity of the RCB model and its potential to provide deeper insights into the effects of co-occurrence and relational information. According to dual-process theories (e.g., Gawronski & Bodenhausen, 2011, 2014, 2018), effects of co-occurrence and relational information are the result of two functionally distinct learning mechanisms (i.e., associative vs. propositional learning). However, effects of mere co-occurrence are also consistent with single-process propositional theories (e.g., De Houwer, 2009, 2014a, 2018), which explain unqualified co-occurrence effects as the result of incomplete retrieval of stored relational information during the expression of an evaluative response (e.g., retrieval of *X is related to something negative* instead of *X prevents something negative*). Thus, expanding on the conflicting explanations of mere co-occurrence effects, we conducted a series of follow-up studies to investigate whether effects of co-occurrence and relational information are differentially affected by (a) processing conditions during the encoding of CS–US pairings and (b) processing conditions during the expression of an evaluative response.

Experiment 2a

Experiment 2a tested the impact of time during encoding on the effects of CS–US co-occurrence and CS–US relations. Toward this end, we used presentation times during encoding that were slightly longer or slightly shorter compared with the ones in Experiment 1. For half of the participants, the CS–US-qualifier combinations were presented for 2000 ms; for the remaining half the CS–US-qualifier combinations were presented for 5000 ms (compared with 3000 ms in Experiment 1). According to dual-process accounts such as the APE model (Gawronski & Bodenhausen, 2011, 2014, 2018), effects of relational information are mediated by a resource-inefficient propositional learning process, whereas co-occurrence effects are mediated by a resource-efficient associative learning process. From this perspective, less time during encoding should reduce scores on the *R* parameter without affecting scores on the *C* parameter. In contrast, single-process propositional accounts such as the IPM (De Houwer, 2018) suggest that mere co-occurrence effects are the result of incomplete retrieval of stored relational information during the expression of an evaluative response. Based on this assumption, time during encoding could influence evaluative responses in two ways. First, less time during encoding may undermine the *generation* of mental propositions about observed CS–US relations, which should lead to lower scores on the *R* parameter. Moreover, because mere-occurrence effects inherently depend on the generation of mental propositions about CS–US relations during encoding, disruptive effects on the generation of mental propositions should also lead to lower scores on the *C* parameter. Second, less time during encoding may interfere with the *storage* of generated mental propositions about observed CS–US relations in long-term memory, which may increase the likelihood of incomplete retrieval of the stored information during the expression of an evaluative response. In this case, less time during encoding should reduce scores on the *R* parameter and increase scores on the *C* parameter. Experiment 2a tested these predictions using the RCB model.

Method

Participants and design. Participants were recruited for a study entitled "How Do We Form Impressions of Novel Objects?" via Amazon's MTurk. Eligibility for participation was limited to MTurk workers in the United States with a HIT approval rate of at least 95% who did not participate in prior studies from our lab using the same paradigm. Participants received compensation of \$2.00 for completing the study. Of the 438 MTurk workers who initially began the study, 400 completed all measures. Five participants failed to pass the instructional manipulation check, and 15 participants reported not paying attention to the images or not taking their responses seriously. Data from these participants were excluded from the statistical analyses, leaving us with a final sample of 380 participants (199 women, 179 men, 2 other, $M_{age} = 36.98$, $SD_{age} = 11.00$). The study used a 2 (US Valence: positive vs. negative) × 2 (CS–US Relation: causes vs. prevents) × 2 (Time During Encoding: 2000 ms vs. 5000 ms) mixed design, with the first two factors being manipulated within participants and the last one being manipulated between participants.

Procedure. The procedures and materials in Experiment 2a were identical to Experiment 1, the only differences being that (a) the evaluative rating measure was dropped and (b) participants were randomly assigned to one of two between-subjects conditions with either short (2000 ms) or long (5000 ms) presentations of the CS–US-qualifier combinations.

Results

Traditional analysis. Speeded choice data were aggregated in line with the procedures in Experiment 1. Submitted to a 2 (US Valence) × 2 (CS–US Relation) × 2 (Time During Encoding) mixed ANOVA, choice scores revealed a significant main effect of US Valence, $F(1, 376) = 41.14$, $p < .001$, $\eta^2_G = .017$, which was qualified by a significant two-way interaction between US Valence and CS–US Relation, $F(1, 376) = 111.12$, $p < .001$, $\eta^2_G = .078$ (see Table 1). Replicating the pattern obtained in Experiment 1, post hoc tests showed that, when the CSs were described as causing the USs, CSs paired with positive USs were chosen more frequently than CSs paired with negative USs, $t(377) = 11.52$, $p < .001$, $d = 0.59$. Conversely, when the CSs were described as preventing the USs, CSs paired with positive USs were chosen less frequently than CSs paired with negative USs, $t(377) = 4.97$, $p < .001$, $d = 0.26$. Moreover, when the CSs were paired with positive USs, CSs that were described as causing the USs were chosen more frequently than CSs that were described as preventing the USs, $t(377) = 8.28$, $p < .001$, $d = 0.43$. Conversely, when the CSs were paired with negative USs, CSs that were described as causing the USs were chosen less frequently than CSs that were described as preventing the USs, $t(377) = 9.42$, $p < .001$, $d = 0.48$. The three-way interaction between US Valence, CS–US Relation, and Time During Encoding was not statistically significant, $F(1, 376) = 0.64$, $p = .425$, $\eta^2_G = .000$.

RCB model. The ANOVA results suggest that (a) relational information fully qualified the effects of US valence and (b) time during encoding did not moderate these effects. Again, more nuanced insights were gained when the data were analyzed with the RCB model. Overall, the model fit the data well with six free parameters (i.e., three per condition), $G^2(2) = 1.50$, $p = .472$, and the *R* and the *C* parameter were significantly greater than zero in both encoding time conditions (see Table 3). The *R* parameter tended to be somewhat smaller in the 2000 ms condition compared with the 5000 ms condition, but this difference failed to reach the conventional level of statistical significance, $\Delta G^2(1) = 3.29$, $p = .070$. The *C* parameter was not significantly different across encoding time conditions, $\Delta G^2(1) = 0.66$, $p = .418$. There was also no significant effect of the encoding time manipulation on the *B* parameter, $\Delta G^2(1) = 0.62$, $p = .433$.

Table 3
Parameter Estimates Without Model Restrictions as a Function of Time During Encoding (2000 ms vs. 5000 ms), Experiment 2a

Parameter	Estimate	95% CI	*p*
R			
2000 ms	.15	[.13, .17]	<.001
5000 ms	.18	[.16, .21]	<.001
C			
2000 ms	.08	[.05, .11]	<.001
5000 ms	.10	[.07, .13]	<.001
B			
2000 ms	.44	[.43, .46]	<.001
5000 ms	.43	[.42, .45]	<.001

Note. The *R* parameter captures effects of relational information; the *C* parameter captures effects of co-occurrence; the *B* parameter captures general response biases. The *p* values refer to differences between parameter estimates and neutral reference points. The neutral reference point for *R* and *C* is 0; the neutral reference point for *B* is .5, with scores higher than .5 reflecting a general bias toward positive responses and scores lower than .5 reflecting a general bias toward negative responses.

Discussion

Experiment 2a investigated the impact of time during encoding on the effects of CS–US co-occurrence and CS–US relations. Although effects of CS–US relations captured by the *R* parameter tended to be weaker in the short processing time condition compared with the long processing time condition, this difference failed to reach the conventional level of statistical significance. There was no evidence for an effect of time during encoding on the effect of CS–US co-occurrence captured by the *C* parameter. Although the differential effects of time during encoding on the *R* and the *C* parameter are consistent with the predictions of dual-process accounts, the obtained effect on the *R* parameter is inconclusive, because it failed to reach the conventional level of statistical significance. It is possible that the employed manipulation was not strong enough to produce a significant effect on the *R* parameter, but it seems premature to draw strong conclusions from a marginal effect. Thus, to provide more compelling evidence for the effects of time during encoding, we conducted a follow-up study with a stronger manipulation of time during encoding.

Experiment 2b

Experiment 2b investigated the impact of time during encoding on the effects of co-occurrence and relational information using a stronger manipulation of time during encoding. Toward this end, CS–US-qualifier combinations were presented for either 1000 ms or 5000 ms.

Method

Participants and design. Participants were recruited for a study entitled "How Do We Form Impressions of Novel Objects?" via Amazon's MTurk. Eligibility for participation was limited to MTurk workers in the United States with a HIT approval rate of at least 95% who did not participate in prior studies from our lab using the same paradigm. Participants received compensation of $2.00 for completing the study. Of the 434 MTurk workers who initially began the study, 402 completed all measures. Seven participants failed to pass the instructional manipulation check, and five participants reported not paying attention to the images or not taking their responses seriously. Data from these participants were excluded from the statistical analyses, leaving us with a final sample of 390 participants (217 women, 173 men, $M_{age} = 36.99$, $SD_{age} = 11.12$). The study used a 2 (US Valence: positive vs. negative) × 2 (CS–US Relation: causes vs. prevents) × 2 (Time During Encoding: 1000 ms vs. 5000 ms) mixed design, with first two factors being manipulated within participants and last one being manipulated between participants.

Procedure. The procedures and materials in Experiment 2b were identical to Experiment 2a, the only difference being that we reduced the presentation times in the *short* condition from 2000 ms to 1000 ms.

Results

Traditional analysis. Speeded choice data were aggregated in line with the procedures in Experiment 1. Submitted to a 2 (US Valence) × 2 (CS–US Relation) × 2 (Time During Encoding) mixed ANOVA, choice scores revealed significant main effect of US Valence, $F(1, 387) = 56.94$, $p < .001$, $\eta^2_G = .023$, and a significant main effect of the CS–US Relation, $F(1, 387) = 4.29$, $p = .039$, $\eta^2_G = .001$, which were qualified by a significant two-way interaction between US Valence and CS–US Relation, $F(1, 387) = 97.89$, $p < .001$, $\eta^2_G = .058$ (see Table 1). Replicating the pattern in Experiment 2a, post hoc tests showed that, when the CSs were described as causing the USs, CSs paired with positive USs were chosen more frequently than CSs paired with negative USs, $t(388) = 11.97$, $p < .001$, $d = 0.61$. Conversely, when the CSs were described as preventing the USs, CSs paired with positive USs were chosen less frequently than CSs paired with negative USs, $t(388) = 2.92$, $p = .004$, $d = 0.15$. Moreover, when the CSs were paired with positive USs, CSs that were described as causing the USs were chosen more frequently than CSs that were described as preventing the USs, $t(388) = 6.63$, $p < .001$, $d = 0.34$. Conversely, when the CSs were paired with negative USs, CSs that were described as causing the USs were chosen less frequently than CSs that were described as preventing the USs, $t(388) = 9.67$, $p < .001$, $d = 0.49$. The three-way interaction between US Valence, CS–US Relation, and Time During Encoding was not statistically significant, $F(1, 387) = 2.53$, $p = .113$, $\eta^2_G = .002$.

RCB model. Different from the previous two experiments, a model with six free parameters showed suboptimal fit in the current study, in that the probabilities predicted by the RCB model significantly deviated from the empirically observed probabilities, $G^2(2) = 8.63$, $p = .013$. However, because Experiment 2b turned out to be the only study in the current series of experiments in which the RCB model did not fit the data, we deemed this deviation as acceptable and nevertheless tested for differences in parameter estimates across the two experimental conditions (see Table 4). Confirming the reliability of the marginal effect in Experiment 2a, the *R* parameter was significantly smaller in the 1000 ms condition compared with the 5000 ms condition, $\Delta G^2(1) = 9.44$, $p = .002$. Moreover, replicating the finding for the *C* parameter in Experiment 2a, there was no significant effect of encoding time on the *C* parameter, $\Delta G^2(1) = 0.08$, $p = .778$. There was also no significant effect of encoding time on the *B* parameter, $\Delta G^2(1) = 0.75$, $p = .387$.

Table 4
Parameter Estimates Without Model Restrictions as a Function of Time During Encoding (1000 ms vs. 5000 ms), Experiment 2b

Parameter	Estimate	95% CI	*p*
R			
1000 ms	.12	[.10, .14]	<.001
5000 ms	.17	[.15, .20]	<.001
C			
1000 ms	.10	[.07, .13]	<.001
5000 ms	.11	[.08, .14]	<.001
B			
1000 ms	.46	[.44, .47]	<.001
5000 ms	.45	[.43, .46]	<.001

Note. The *R* parameter captures effects of relational information; the *C* parameter captures effects of co-occurrence; the *B* parameter captures general response biases. The *p* values refer to differences between parameter estimates and neutral reference points. The neutral reference point for *R* and *C* is 0; the neutral reference point for *B* is .5, with scores higher than .5 reflecting a general bias toward positive responses and scores lower than .5 reflecting a general bias toward negative responses.

Discussion

Using a stronger manipulation of time during encoding, Experiment 2b supports the conclusion that limited processing time during encoding reduces the effect of CS–US relations without affecting the effect of CS–US co-occurrence. This finding is consistent with the predictions of dual-process accounts such as the APE model (Gawronski & Bodenhausen, 2011, 2014), which suggests that effects of relational information are mediated by a resource-inefficient propositional learning process, whereas co-occurrence effects are mediated by a resource-efficient associative learning process. However, the results are inconsistent with the predictions of single-process propositional accounts such as the IPM (De Houwer, 2018), which suggest that mere co-occurrence effects are the result of incomplete retrieval of stored relational information during the expression of an evaluative response. Based on this assumption, time pressure during encoding should either (a) reduce scores on both the *R* and the *C* parameter or (b) reduce scores on the *R* parameter and increase scores on the *C* parameter.

Experiment 3

Experiment 3 investigated the impact of stimulus repetition on the effects of CS–US co-occurrence and CS–US relations. Drawing on the idea of Hebbian learning (Hebb, 1949), a central assumption of dual-process accounts is that the strength of associative links between two concepts (e.g., association between a CS and a US) increases with the frequency of their co-occurrence, which should facilitate the spread of activation from one concept to the other (e.g., Gawronski & Bodenhausen, 2018; Rydell & McConnell, 2006; Smith & DeCoster, 2000). Although propositional learning has been claimed to be less dependent on repetition than associative learning (e.g., Rydell, McConnell, Strain, Claypool, & Hugenberg, 2007), a similar assumption could be made for the retrieval of representations that capture the relation between two concepts, which should be facilitated by frequent repetition of relational information during encoding (see Gawronski & Bodenhausen, 2018). Hence, from a dual-process perspective, repetition of CS–US pairings should increase scores on the *C* parameter via the strengthening of associative links between co-occurring stimuli. Moreover, dual-process accounts would be consistent with a simultaneous increase of scores on the *R* parameter to the extent that repetition of relational information during encoding facilitates its retrieval during the expression of an evaluative response.

For single-process propositional accounts, there are again two sets of potential hypotheses with different implications for the influence of stimulus repetition on mere co-occurrence effects. First, frequent repetition may support the *generation* of mental propositions about observed CS–US relations, which should lead to higher scores on the *R* parameter. Moreover, given that effects of co-occurrence inherently depend on the generation of mental propositions about CS–US relations during encoding, frequent repetition should also lead to higher scores on the *C* parameter. Second, frequent repetition may support the *storage* of generated mental propositions about CS–US relations in long-term memory, which may counteract incomplete retrieval of the stored relational information during the expression of an evaluative response. In this case, frequent repetition should have complementary effects by increasing scores on the *R* parameter and decreasing scores on the *C* parameter. Experiment 3 tested these predictions by investigating the impact of stimulus repetition during encoding on the effects of CS–US co-occurrence and CS–US relations.

Method

Participants and design. Participants were recruited for a study entitled "How Do We Form Impressions of Novel Objects?" via Amazon's MTurk. Eligibility for participation was limited to MTurk workers in the United States with a HIT approval rate of at least 95% who did not participate in prior studies from our lab using the same paradigm. Participants received compensation of $2.00 for completing the study. Of the 216 MTurk workers who initially began the study, 201 completed all measures. Four participants failed to pass the instructional manipulation check, three participants reported not paying attention to the images or not taking their responses seriously, and seven participants did not respond to any products within the response window for at least one of the four experimental cells. Data from these participants were excluded from the statistical analyses, leaving us with a final sample of 187 participants (80 women, 107 men, $M_{\text{age}} = 34.43$, $SD_{\text{age}} = 10.02$). The study used a 2 (US Valence: positive vs. negative) × 2 (CS–US Relation: causes vs. prevents) × 2 (Repetition: 4 vs. 24) within-subjects design.

Procedure. Experiment 3 used the learning and choice tasks of Experiment 1, the only two differences being that (a) the number of presentations in the learning task was manipulated as an additional within-subjects factor and (b) the number of CSs was reduced from 12 to 8, such that there was one CS for each within-subjects condition. Specifically, four of the CSs were presented four times, whereas the other four were presented 24 times. As in Experiment 1, the learning task was divided into four blocks, with each CS–US-qualifier combination being presented once or six times within each block. The assignment of a given CS to the 8 within-subjects conditions was counterbalanced by means of a Latin square.

Results

Traditional analysis. Speeded choice data were aggregated in line with the procedures in Experiment 1. A 2 (US Valence) × 2 (CS–US Relation) × 2 (Repetition) ANOVA for repeated measures revealed a significant main effect of US Valence, $F(1, 185) = 24.67$, $p < .001$, $\eta^2_G = .014$, a significant main effect of repetition, $F(1, 185) = 5.53$, $p = .020$, $\eta^2_G = .003$, and a significant two-way interaction between US Valence and CS–US Relation, $F(1, 185) = 76.86$, $p < .001$, $\eta^2_G = .083$, which were qualified by a significant three-way interaction between US Valence, CS–US Relation, and Repetition, $F(1, 185) = 6.56$, $p = .011$, $\eta^2_G = .003$ (see Table 1). To decompose the three-way interaction, we conducted separate 2 (US Valence) × 2 (CS–US Relation) ANOVAs for the each of the two repetition conditions.

The ANOVAs revealed a significant main effect of US Valence in the condition with 4 repetitions, $F(1, 185) = 10.13$, $p = .002$, $\eta^2_G = .009$, as well as in the condition with 24 repetitions, $F(1, 185) = 16.46$, $p < .001$, $\eta^2_G = .019$. These main effects were qualified by a significant two-way interaction between US Valence and CS–US Relation in the condition with 4 repetitions, $F(1, 185) = 37.43$, $p < .001$, $\eta^2_G = .056$, as well as in the condition with 24 repetitions, $F(1, 185) = 71.32$, $p < .001$, $\eta^2_G = .113$. The primary difference driving the significant three-way interaction in the omnibus ANOVA was that the effect size for the two-way interaction was twice as large in the 24-repetition condition compared with the four-repetition condition.

Further analyses revealed that CSs that caused a positive US were preferred over CSs that caused a negative US in the four-repetition condition, $t(185) = 6.56$, $p < .001$, $d = 0.48$, as well as the 24-repetition condition, $t(185) = 8.97$, $p < .001$, $d = 0.66$. Conversely, CSs that prevented a negative US were preferred over CSs that prevented a positive US in the four-repetition condition, $t(185) = 2.99$, $p = .003$, $d = 0.22$, as well as the 24-repetition condition, $t(185) = 4.03$, $p < .001$, $d = 0.30$. Moreover, CSs that caused a positive US were preferred over CSs that prevented a positive US in the four-repetition condition, $t(185) = 5.72$, $p < .001$, $d = 0.42$, as well as the 24-repetition condition, $t(185) = 6.64$, $p < .001$, $d = 0.49$. Conversely, CSs that prevented a negative US were preferred over CSs that caused a negative US in the four-repetition condition, $t(185) = 3.83$, $p < .001$, $d = 0.28$, as well as the 24-repetition condition, $t(185) = 7.33$, $p < .001$, $d = 0.54$.

RCB model. Overall, the RCB model fit the data well with six free parameters, $G^2(2) = 3.90$, $p = .142$ (see Table 5). The *R* parameter was significantly larger in the 24-repetition condition compared with the 4-repetition condition, $\Delta G^2(1) = 7.46$, $p = .006$, indicating that the impact of relational information on choices increased with increasing repetitions. There was no significant effect of the number of repetitions on the *C* parameter, $\Delta G^2(1) = 1.42$, $p = .233$, suggesting that the effect of CS–US co-occurrence was unaffected by the number of repetitions. The *B* parameter was larger in the 24-repetition condition compared with

Table 5
Parameter Estimates Without Model Restrictions as a Function of Stimulus Repetition During Encoding (4 vs. 24), Experiment 3

Parameter	Estimate	95% CI	*p*
R			
4 repetitions	.19	[.14, .23]	<.001
24 repetitions	.27	[.23, .31]	<.001
C			
4 repetitions	.09	[.04, .15]	.001
24 repetitions	.14	[.08, .20]	<.001
B			
4 repetitions	.43	[.40, .46]	<.001
24 repetitions	.48	[.45, .52]	.330

Note. The *R* parameter captures effects of relational information; the *C* parameter captures effects of co-occurrence; the *B* parameter captures general response biases. The *p* values refer to differences between parameter estimates and neutral reference points. The neutral reference point for *R* and *C* is 0; the neutral reference point for *B* is .5, with scores higher than .5 reflecting a general bias toward positive responses and scores lower than .5 reflecting a general bias toward negative responses.

the 4-repetition condition, $\Delta G^2(1) = 5.03$, $p = .025$, indicating that participants were less likely to reject the presented products when the products were presented 24 times than when they were presented 4 times.

Discussion

The results of Experiment 3 suggest that effects of CS–US relations increase as a function of stimulus repetition during encoding. This result is consistent with both dual-process and single-process propositional accounts. However, the two accounts are difficult to reconcile with the finding that effects of CS–US co-occurrence were unaffected by stimulus repetition during encoding. From a dual-process view, repetition of CS–US pairings should increase scores on the *C* parameter via the strengthening of associative links between co-occurring stimuli. From a single-process propositional view, repetition of CS–US pairings should either (a) increase scores on the *C* parameter by supporting the *generation* of mental propositions about observed CS–US relations or (b) reduce scores on the *C* parameter by supporting the *storage* of generated mental propositions about CS–US relations. Thus, the finding that co-occurrence effects on the *C* parameter remained unaffected by stimulus repetition during encoding poses an explanatory challenge to both dual-process and single-process propositional accounts.

Experiment 4

Expanding on the argument that differential effects of co-occurrence and relational information may be the result of response-related rather than learning-related processes (De Houwer, 2014b, 2018), the final two studies explored the impact of processing conditions during the expression of an evaluative response. Experiment 4 tested the impact of time during judgment on the effects of CS–US co-occurrence and CS–US relations. Toward this end, half of the participants in Experiment 4 had 750 ms to provide their response in the choice task, whereas the remaining half had 2500 ms to respond (compared with 1000 ms in the previous experiments). According to single-process propositional accounts such as the IPM (De Houwer, 2018), differential effects of CS–US relations versus CS–US co-occurrence depend on the complete retrieval of stored relational information during the expression of an evaluative response (e.g., complete retrieval of *X prevents something negative* instead of *X is related to something negative*). Thus, to the extent that time pressure during judgment increases the likelihood of incomplete retrieval of stored relational information, it should influence scores on the *R* and the *C* parameter in a compensatory fashion. That is, less (vs. more) time during judgment should decrease scores on the *R* parameter and increase scores on the *C* parameter—corresponding to the differential sensitivity of implicit and explicit measures to co-occurrence and relational information (e.g., Hu et al., 2017; Moran & Bar-Anan, 2013). A similar prediction can be derived from dual-process accounts such as the APE model (Gawronski & Bodenhausen, 2011, 2014, 2018), suggesting that effects of activated associations on judgments and behavior should be reduced when deliberate propositional reasoning leads to a rejection of the spontaneous evaluative response elicited by automatically activated associations. From this perspective, less (vs. more) time during judgment should reduce scores on the *R* parameter and increase scores on the *C* parameter—corresponding to the differential sensitivity of implicit and explicit measures to co-occurrence and relational information (e.g., Hu et al., 2017; Moran & Bar-Anan, 2013). The main goal of Experiment 4 was to test these predictions.

Method

Participants and design. Participants were recruited for a study entitled "How Do We Form Impressions of Novel Objects?" via Amazon's MTurk. Eligibility for participation was limited to MTurk workers in the United States with a HIT approval rate of at least 95% who did not participate in prior studies from our lab using the same paradigm. Participants received compensation of $2.00 for completing the study. Of the 441 MTurk workers who initially began the study, 401 completed all measures.[10] Eight participants failed to pass the instructional manipulation check, three participants reported not paying attention to the images or not taking their responses seriously, and four participants had no data for at least one of the four experimental cells. Data from these participants were excluded, leaving us with a sample of 386 participants (209 women, 176 men, 1 other, $M_{age} = 38.40$, $SD_{age} = 12.66$). The study used a 2 (US Valence: positive vs. negative) × 2 (CS–US Relation: causes vs. prevents) × 2 (Time During Judgment: 750 ms vs. 2500 ms) mixed design, with the first two factors being manipulated within participants and last one being manipulated between participants.

Procedure. Experiment 4 used the learning and choice tasks of Experiment 1, the only difference being that the speeded choice task included either a short (750 ms) or long (2500 ms) response window.

Results

Traditional analysis. Speeded choice data were aggregated in line with the procedures in Experiment 1. A 2 (US Valence) × 2 (CS–US Relation) × 2 (Time During Judgment) mixed ANOVA revealed a significant main effect of US Valence, $F(1, 384) = 46.49$, $p < .001$, $\eta^2_G = .018$, a significant two-way interaction between US Valence and Time During Judgment, $F(1, 384) = 7.47$, $p = .007$, $\eta^2_G = .003$, and a significant two-way interaction between US Valence and CS–US Relation, $F(1, 384) = 138.08$, $p < .001$, $\eta^2_G = .104$. These effects were qualified by a significant three-way interaction between US Valence, CS–US Relation, and Time During Judgment, $F(1, 384) = 32.87$, $p < .001$, $\eta^2_G = .027$ (see Table 1). To decompose this interaction, we conducted separate 2 (US Valence) × 2 (CS–US Relation) ANOVAs for the each of the two response-window conditions.

The ANOVAs revealed a significant main effect of US Valence in the short response-window condition, $F(1, 196) = 9.07$, $p = .003$, $\eta^2_G = .007$, as well as the in the long response-window condition, $F(1, 188) = 42.06$, $p < .001$, $\eta^2_G = .033$. These main effects were qualified by a significant two-way interaction between US Valence and CS–US Relation in the short response-window condition, $F(1, 196) = 26.58$, $p < .001$, $\eta^2_G = .031$, as well as the long response-window condition, $F(1, 188) = 113.67$,

[10] One participant completed the study twice. The second data set of this participant was not included in the analysis.

$p < .001$, $\eta^2_G = .195$. The primary difference driving the significant three-way interaction in the omnibus ANOVA was that the effect size for the two-way interaction was six times larger in the long response-window condition compared with the short response-window condition.

Further analyses revealed that CSs that caused a positive US were preferred over CSs that caused a negative US in the short response-window condition, $t(196) = 5.36$, $p < .001$, $d = 0.38$, as well as the long response-window condition, $t(188) = 12.47$, $p < .001$, $d = 0.91$. Conversely, CSs that prevented a negative US were preferred over CSs that prevented a positive US in the short response-window condition, $t(196) = 2.42$, $p = .017$, $d = 0.17$, as well as the long response-window condition, $t(188) = 5.66$, $p < .001$, $d = 0.41$. Moreover, CSs that caused a positive US were preferred over CSs that prevented a positive US in the short response-window condition, $t(196) = 4.42$, $p < .001$, $d = 0.32$, as well as the long response-window condition, $t(188) = 8.73$, $p < .001$, $d = 0.63$. Conversely, CSs that prevented a negative US were preferred over CSs that caused a negative US in the short response-window condition, $t(196) = 3.64$, $p < .001$, $d = 0.26$, as well as the long response-window condition, $t(188) = 9.93$, $p < .001$, $d = 0.72$.

RCB model. Overall, the RCB model fit the data well with six free parameters, $G^2(2) = 2.95$, $p = .229$ (see Table 6). There was a significant effect of Time During Judgment on the *C* parameter, $\Delta G^2(1) = 18.66$, $p < .001$, indicating that mere co-occurrence of a CS and a US had a weaker impact on participants' choices when they had less time to respond than when they had more time to respond. A corresponding effect emerged for the *R* parameter, $\Delta G^2(1) = 115.60$, $p < .001$, indicating that relational information had a weaker impact on choices when they had less time to respond than when they had more time to respond. There was no significant effect of the response-window manipulation on the *B* parameter, $\Delta G^2(1) = 0.41$, $p = .523$.

Discussion

Experiment 4 tested the impact of time during judgment on the effects of CS–US co-occurrence and CS–US relations. Consistent with the predictions of both dual-process and single-process propositional accounts, effects of relational information captured by the *R* parameter were stronger when participants had more time to respond than when they had less time to respond. However, in contrast to the predictions of either account, the *C* parameter showed the same pattern, in that mere co-occurrence effects were stronger when participants had more time to respond than when they had less time to respond.[11] This finding stands in contrast to the shared prediction that effects of CS–US co-occurrence should be weaker when participants have more time to respond than when they have less time to respond.

Table 6
Parameter Estimates Without Model Restrictions as a Function of Time During Judgment (750 ms vs. 2500 ms), Experiment 4

Parameter	Estimate	95% CI	*p*
R			
750 ms	.10	[.08, .13]	<.001
2500 ms	.29	[.26, .31]	<.001
C			
750 ms	.06	[.03, .09]	<.001
2500 ms	.15	[.12, .18]	<.001
B			
750 ms	.45	[.43, .46]	<.001
2500 ms	.44	[.42, .46]	<.001

Note. The *R* parameter captures effects of relational information; the *C* parameter captures effects of co-occurrence; the *B* parameter captures general response biases. The *p* values refer to differences between parameter estimates and neutral reference points. The neutral reference point for *R* and *C* is 0; the neutral reference point for *B* is .5, with scores higher than .5 reflecting a general bias toward positive responses and scores lower than .5 reflecting a general bias toward negative responses.

Experiment 5

Experiment 5 investigated the effect of another response-related factor: the impact of time delay between encoding and judgment. Toward this end, participants completed the choice task immediately after the learning task, and then again two days after completion of the first session. According to single-process propositional accounts such as the IPM (De Houwer, 2018), differential effects of CS–US relations versus CS–US co-occurrence depend on the complete retrieval of stored relational information during the expression of an evaluative response (e.g., complete retrieval of *X prevents something negative* instead of *X is related to something negative*). Thus, to the extent that delay between encoding and judgment increases the likelihood of incomplete retrieval of stored relational information, it should decrease scores on the *R* parameter and increase scores on the *C* parameter. In contrast, dual-process accounts such as the APE model (Gawronski & Bodenhausen, 2011, 2014, 2018) suggest that mental representations of CS–US relations involve multiple layers of representation in which activated concepts at higher levels specify the relation between activated concepts at lower levels (see Gawronski & Bodenhausen, 2018; Gawronski et al., 2017). Thus, to the extent that hierarchical representations involving multiple layers of associative links are more likely affected by memory decay compared with direct associative links between two concepts, effects of CS–US co-occurrence should be more stable over time compared with effects of CS–US relations. From this perspective, temporal delays between encoding and judgment should reduce scores on the *R* parameter, but not the *C* parameter. Experiment 5 tested these predictions using the RCB model.

Method

Procedure and design. At Time 1, participants completed the learning and choice tasks of Experiment 1. At Time 2, participants completed the choice task a second time. With the combined data from the two sessions, the study used a 2 (US Valence: positive vs.

[11] Prior to Experiments 2–5, we conducted a pilot study ($N = 189$) using the same materials of Experiment 4. In this study, the RCB model fit the data well with six free parameters, $G^2(2) = 0.09$, $p = .96$. Time During Judgment had a significant effect on the *R* parameter, $\Delta G^2(1) = 43.62$, $p < .001$, indicating that relational information had a weaker impact on choices when participants had less time to respond than when they had more time to respond. There was also a non-significant trend on the *C* parameter, $\Delta G^2(1) = 2.87$, $p = .09$, indicating that mere co-occurrence of a CS and a US tended to have a weaker impact on participants' choices when they had less time to respond than when they had more time to respond.

negative) × 2 (CS–US Relation: causes vs. prevents) × 2 (Measurement Time: immediate vs. 2-day delay) within-subjects design.

Time 1. Participants were recruited for a two-session study entitled "How Do We Form Impressions of Novel Objects?" via Amazon's MTurk. Eligibility for participation was limited to MTurk workers in the United States with a HIT approval rate of at least 95% who did not participate in prior studies from our lab using the same paradigm. Participants received compensation of $2.00 for completing the first part of the study. Of the 326 MTurk workers who initially began the study, 300 submitted a request for payment. Two of these participants submitted the correct payment code on Amazon Mturk, but did not complete the study; four participants had incomplete data (presumably because of slow internet connections or server outages); 25 participants failed an instructional manipulation check (see below), and one participant reported not paying attention to the images or not taking their responses seriously. These participants were not invited for the second part of the study, leaving us with a potential sample of 268 participants (131 women, 136 men, 1 other, $M_{\text{age}} = 36.29$, $SD_{\text{age}} = 11.59$).

Time 2. Two days after completion of the first part, we invited the 268 participants who were not excluded at Time 1 to complete the second part of the study. Our goal was to obtain data from 200 participants for both time points, and the study posting on MTurk was set to expire once 200 participants completed the second part. Participants received an additional compensation of $2.00 for completing the second part of the study. The average lag time between the first and second part was 52.05 hr ($Min = 40.91$, $Max = 87.45$, $SD = 8.22$, $Median = 48.76$). Based on this procedure, we obtained complete data from 199 participants, all of whom passed the instructional manipulation check.[12] Two participants reported that they did not pay attention to the images; for one participant the MTurk ID provided at Time 2 did not match any of the MTurk IDs at Time 1; and one participant failed to respond within the 1000 ms response window on all 36 trials. Data from these participants were excluded, leaving us with a final sample of 195 participants with valid data from both Part 1 and Part 2 (96 women, 98 men, 1 other, $M_{\text{age}} = 37.41$, $SD_{\text{age}} = 11.47$).

Attention check. As an instructional manipulation check, participants were presented with the following instructions at the end of the first session:

> To facilitate our research on decision-making we are interested in knowing certain factors about you, the decision maker. Specifically, we are interested in whether you actually take the time to read the directions; if not, then some of our manipulations that rely on changes in the instructions will be ineffective. So, to demonstrate that you have read the instructions, please ignore the product name items below. Instead, simply continue on to the next page after the options. Thank you very much.

The following question and response options were displayed below the instructions:

> Which of these product names were shown in the previous task? (check all that apply)

> Metalene, Argozine, Tyrobutol, Legarol, Rangazine, Lemitol, Mirvanol

By default, we excluded all participants who did not follow the instruction to ignore the response options on this item. At the end of the second session, participants were presented with the same instructional manipulation check used in the previous studies.

Results

Traditional analysis. Speeded choice data were aggregated in line with the procedures in Experiment 1. A 2 (US Valence) × 2 (CS–US Relation) × 2 (Measurement Time) ANOVA for repeated measures revealed a significant main effect of US Valence, $F(1, 194) = 34.64$, $p < .001$, $\eta_G^2 = .018$, a significant main effect of Measurement Time, $F(1, 194) = 10.95$, $p = .001$, $\eta_G^2 = .004$, and a significant two-way interaction between US Valence and CS–US Relation, $F(1, 194) = 72.56$, $p < .001$, $\eta_G^2 = .062$. These effects were qualified by a significant three-way interaction between US Valence, CS–US Relation, and Measurement Time, $F(1, 194) = 4.55$, $p = .034$, $\eta_G^2 = .001$ (see Table 1).

To decompose this interaction, we conducted separate 2 (US Valence) × 2 (CS–US Relation) ANOVAs for each of the two Measurement Time conditions. The ANOVA revealed a significant main effect of US Valence in both the immediate condition, $F(1, 194) = 29.03$, $p < .001$, $\eta_G^2 = .022$, as well as the 2-day delay condition, $F(1, 194) = 18.91$, $p < .001$, $\eta_G^2 = .014$. These main effects were qualified by a significant two-way interaction between US Valence and CS–US Relation in the immediate measurement condition, $F(1, 194) = 62.59$, $p < .001$, $\eta_G^2 = .076$, as well as the 2-day delay condition, $F(1, 194) = 53.36$, $p < .001$, $\eta_G^2 = .050$. The primary difference driving the significant three-way interaction in the omnibus ANOVA was that the effect size for two-way interaction in the immediate condition was somewhat larger compared with the 2-day delay condition.

Further analyses revealed that CSs that caused a positive US were preferred over CSs that caused a negative US in the immediate measurement condition, $t(194) = 9.29$, $p < .001$, $d = 0.67$, as well as the delayed measurement condition, $t(194) = 8.00$, $p < .001$, $d = 0.57$. Conversely, CSs that prevented a negative US were preferred over CSs that prevented a positive US in the immediate measurement condition, $t(194) = 3.11$, $p = .002$, $d = 0.22$, as well as the delayed measurement condition, $t(194) = 2.82$, $p = .005$, $d = 0.20$. Moreover, CSs that caused a positive US were preferred over CSs that prevented a positive US in the immediate measurement condition, $t(194) = 6.09$, $p < .001$, $d = 0.44$, as well as the delayed measurement condition, $t(194) = 4.73$, $p < .001$, $d = 0.34$. Conversely, CSs that prevented a negative US were preferred over CSs that caused a negative US in the immediate measurement condition, $t(194) = 6.97$, $p < .001$, $d = 0.50$, as well as the delayed measurement condition, $t(194) = 7.02$, $p < .001$, $d = 0.50$.

RCB model. Overall, the RCB model fit the data well with six free parameters, $G^2(2) = 2.98$, $p = .225$ (see Table 7). Measurement Time showed a significant effect on the R parameter, $\Delta G^2(1) = 5.73$, $p = .017$, indicating that the impact of relational information was weaker when choices were measured with a delay of 2 days than when they were measured immediately after encoding. There was no significant effect of Measurement Time on

[12] One participant completed Part 2 twice. The second data set of this participant was not included in the analysis.

Table 7
Parameter Estimates Without Model Restrictions as a Function of Measurement Delay Between Encoding and Judgment (Immediate Versus 2-Day Delay), Experiment 5

Parameter	Estimate	95% CI	*p*
R			
Immediate	.17	[.15, .20]	<.001
2-day delay	.13	[.11, .15]	<.001
C			
Immediate	.11	[.08, .14]	<.001
2-day delay	.08	[.05, .11]	<.001
B			
Immediate	.46	[.45, .48]	<.001
2-day delay	.42	[.40, .43]	<.001

Note. The *R* parameter captures effects of relational information; the *C* parameter captures effects of co-occurrence; the *B* parameter captures general response biases. The *p* values refer to differences between parameter estimates and neutral reference points. The neutral reference point for *R* and *C* is 0; the neutral reference point for *B* is .5, with scores higher than .5 reflecting a general bias toward positive responses and scores lower than .5 reflecting a general bias toward negative responses.

the *C* parameter, $\Delta G^2(1) = 2.03$, $p = .154$. Scores on the *B* parameter significantly differed as a function of Measurement Time, $\Delta G^2(1) = 16.20$, $p < .001$, in that participants showed a higher likelihood of choosing the products when choices were measured immediately after encoding than when they were measured with a delay of 2 days.

Discussion

Experiment 5 tested the impact of time delay between encoding and judgment on the effects of CS–US co-occurrence and CS–US relations. Consistent with the shared prediction of dual-process and single-process propositional accounts, effects of CS–US relations captured by the *R* parameter decreased with increasing delays between encoding and judgment. In contrast, effects of CS–US co-occurrence captured by the *C* parameter were unaffected by the delay between encoding and judgment. The latter finding is consistent with the predictions of dual-process accounts such as the APE Model (Gawronski & Bodenhausen, 2011, 2014, 2018), suggesting that direct associative links between concepts are less affected by memory decay compared with representations of relational information involving multiple layers of associative links (see Gawronski & Bodenhausen, 2018; Gawronski et al., 2017). However, the observed stability of the *C* parameter over time is inconsistent with the predictions of single-process propositional accounts such as the IPM (De Houwer, 2018), suggesting that delay between encoding and judgment increases the likelihood of incomplete retrieval of stored relational information. According to this hypothesis, temporal delay should decrease scores on the *R* parameter and increase scores on the *C* parameter. Although Experiment 5 revealed the predicted reduction on the *R* parameter, there was no significant increase on the *C* parameter.

General Discussion

Expanding on theoretical controversies regarding the effects of CS–US co-occurrence and CS–US relations on evaluative responses, the current work aimed to provide a deeper understanding of their underlying processes using a multinomial modeling approach. According to dual-process theories that propose two functionally distinct learning mechanisms (e.g., Gawronski & Bodenhausen, 2011, 2014, 2018), co-occurrence effects are mediated by an associative learning process involving the formation of unqualified links between co-occurring stimuli, whereas effects of relational information are mediated by a propositional learning process involving the generation and truth assessment of propositional beliefs about the relation between stimuli. Single-process propositional accounts reject the idea of associative link formation, and instead suggest that both effects are mediated by the generation and truth assessment of mental propositions about the relation between stimuli (e.g., De Houwer, 2009, 2014a, 2018; Mitchell et al., 2009). A central issue in this debate is whether differential effects of co-occurrence and relational information on implicit and explicit measures (e.g., Hu et al., 2017; Moran & Bar-Anan, 2013) are attributable to (a) processes during the formation of evaluative representations or (b) processes during the expression of an evaluative response (see De Houwer, 2018; Gast et al., 2012; Gawronski et al., 2016). Compared with the dominant focus on measurement-related dissociations in earlier research on this question (e.g., Gawronski et al., 2005; Hu et al., 2017; Moran & Bar-Anan, 2013; Zanon et al., 2014), a major advantage of the RCB model is that it quantifies simultaneous effects of co-occurrence and relational information on a single measure of evaluative responses. This aspect resolves confounds between learning-related and response-related processes in the interpretation of measurement-related dissociations, providing a valuable tool for more stringent investigations of the effects of CS–US co-occurrence and CS–US relations. Moreover, the roles of learning-related and response-related processes can be studied more directly by independently manipulating processing conditions during the learning of evaluative information and the expression of an evaluative response (see De Houwer, 2018; Gast et al., 2012; Gawronski et al., 2016).

The first noteworthy finding obtained with the RCB model is that both CS–US co-occurrence and CS–US relations influenced responses in a binary choice task. Expanding on this finding, we conducted a series of follow-up studies to investigate the functional properties of the two kinds of effects by separately manipulating processing conditions during encoding and judgment. The findings for the effect of CS–US relations were generally consistent with the predictions of the two competing theories. However, the two theories fared less well in terms of their predictions for the effect of CS–US co-occurrence.

Consistent with the predictions of the two competing accounts, effects of CS–US relations captured by the model's *R* parameter were larger when the presentation times during encoding were long rather than short (Experiments 2a and 2b), when the stimuli were presented more frequently rather than less frequently (Experiment 3), when participants had more time to make a choice than when they had less time to make a choice (Experiment 4), and when participants made their choices immediately after encoding than when they made their choices after a 2-day delay (Experiment 5). In contrast, effects of CS–US co-occurrence captured by the model's *C* parameter were unaffected by presentation times during encoding (Experiments 2a and 2b), the relative frequency of stimulus repetition (Experiment 3), and the delay between encoding and judgment (Experiment 5). The only significant effect on the *C* parameter occurred for the manipulation of time during judgment, in that effects of CS–US co-occurrence were larger when participants had more time to make a choice than when they had less time to make a choice (Experiment 4). These effects stand in contrast to the predictions of single-process propositional accounts, which suggest that co-occurrence effects should be reduced or increased by shorter presentation times during encoding, reduced or increased by stimulus repetition, increased by time pressure during judgment, and increased by increasing delays between encoding and judgment. Dual-process theories fare somewhat better, in that they are consistent with the obtained resistance of co-occurrence effects against presentation times during encoding and delays between encoding and judgment. However, dual-process theories are inconsistent with the obtained effects of stimulus repetition and time during judgment. From a dual-process view, effects of CS–US co-occurrence should become stronger with frequent repetition and greater time pressure during judgment.

Implications for Dual-Process Accounts

The challenge for dual-process accounts is to provide theoretically plausible interpretations for the findings that (a) effects of CS–US co-occurrence were unaffected by stimulus repetition during encoding (Experiment 3) and (b) effects of CS–US co-occurrence were larger when participants had more time to make a choice than when they had less time to make a choice (Experiment 4).

A potential interpretation of the first unpredicted finding (i.e., effects of CS–US co-occurrence were unaffected by stimulus repetition during encoding) is that stimulus repetition influences mere co-occurrence effects in multiple ways that have compensatory behavioral outcomes. On the one hand, repetition of CS–US pairings may strengthen associations between the co-occurring stimuli, as suggested by the idea of associative learning. On the other hand, repetition of information about CS–US relations may strengthen links across multiple layers of representation in which activated concepts at higher levels specify the relation between activated concepts at lower levels (see Gawronski & Bodenhausen, 2018; Gawronski et al., 2017). Thus, although the latter effect should increase the impact of relational information on the *R* parameter, it may also compensate for the effect of strengthened CS–US associations on the *C* parameter. That is, strengthened unqualified associations between co-occurring stimuli facilitate the spread of activation from the CS to the US, but the effect of their coactivation on evaluative responses is compensated by the facilitated spread of activation to higher levels of representation that specify the relation between the two stimuli. Because effects of activated associations on judgments and behavior should be reduced when propositional reasoning leads to a rejection of the spontaneous evaluative response elicited by automatically activated associations (Gawronski & Bodenhausen, 2006, 2011), the two effects at the representational level should compensate each other at the behavioral level, leading to a null effect of stimulus repetition on the *C* parameter.

A potential interpretation of the second unpredicted finding (i.e., effects of CS–US co-occurrence were larger when participants had more time to make a choice than when they had less time to make a choice) is that expression-related processes differ in terms of their relative efficiency instead of representing two categories of processes that are either resource-dependent or resource-independent. Put differently, even processes that qualify as relatively efficient may be disrupted when processing resources are low, and such disruptions may simply be more pronounced more for processes that qualify as inefficient. Thus, it is possible that the processing constraints imposed by our manipulation of time during judgment were too extreme, in that they undermined not only the retrieval of relational information, but also the spread of activation along unqualified associative links. In this case, extreme time pressure during judgment should reduce effects of CS–US relations on the *R* parameter as well as effects of CS–US co-occurrence on the *C* parameter, as we found in Experiment 4. However, any such interpretation would have to be reconciled with previous research showing effects of CS–US co-occurrence on responses in an evaluative priming task (Hu et al., 2017, Experiments 1 and 2). Because average response times in the priming task were similar to the response window employed in the current research, it is unclear why co-occurrence effects were reduced by time pressure in the current research and enhanced in previous research using implicit measures. Hence, it remains unclear how dual-process accounts would explain the finding that time pressure during judgment reduced the effect of CS–US co-occurrence on the *C* parameter.

Implications for Single-Process Accounts

The challenge for single-process propositional accounts is to provide theoretically plausible interpretations for the findings that effects of CS–US co-occurrence were (a) unaffected by time during encoding (Experiments 2a and 2b), (b) unaffected by stimulus repetition during encoding (Experiment 3), (c) larger when participants had more time to make a choice than when they had less time to make a choice (Experiment 4), and (d) unaffected by the delay between encoding and judgment (Experiment 5).

A potential interpretation of the first unpredicted finding (i.e., effects of CS–US co-occurrence were unaffected by time during encoding) is that limited time during encoding interferes with both (a) the *generation* of mental propositions about observed CS–US relations and (b) the *storage* of generated mental propositions in long-term memory. Whereas the former interference effect was predicted to reduce co-occurrence effects on the *C* parameter, the latter interference effect was predicted to increase co-occurrence effects on the *C* parameter. Thus, to the extent that time during encoding influences both the generation of mental propositions and their storage in long-term memory, the two effects should influence mere-occurrence effects in a compensatory fashion, leading to a null effect of time during encoding on the *C* parameter.

A similar argument may account for the second unpredicted finding (i.e., effects of CS–US co-occurrence were unaffected by stimulus repetition during encoding), in that stimulus repetition may influence mere co-occurrence effects in two ways that have compensatory outcomes. That is, stimulus repetition may support both (a) the *generation* of mental propositions about observed CS–US relations and (b) the *storage* of generated mental propositions in long-term memory. Whereas the former kind of influence was predicted to increase co-occurrence effects on the *C* parameter, the latter kind of influence was predicted to decrease co-occurrence effects on the *C* parameter. Thus, to the extent that stimulus repetition influences both the generation of mental propositions and their storage in long-term memory, the two effects should influence mere co-occurrence effects in a compensatory fashion, leading to a null effect of stimulus repetition on the *C* parameter.

A potential interpretation for the third unpredicted finding (i.e., effects of CS–US co-occurrence were larger when participants had more time to make a choice than when they had less time to make a choice) is that the retrieval of stored information requires a minimum amount of mental resources regardless of whether retrieval is complete or incomplete. From this perspective, time pressure during judgment should reduce scores on both the *R* parameter and the *C* parameter, as we found in Experiment 4. However, similar to the suggested post hoc interpretation for dual-process accounts, any such interpretation would have to be reconciled with studies showing reduced effects of CS–US relations and enhanced effects of CS–US co-occurrence in an evaluative priming task (Hu et al., 2017, Experiments 1 and 2). If retrieval of stored information requires a minimum amount of mental resources regardless of whether retrieval is complete or incomplete, implicit measures should show reduced effects of both CS–US co-occurrence and CS–US relations. Because the response window employed in the current research was similar to the average response times in earlier research using evaluative priming, it is unclear why co-occurrence effects were reduced by time pressure in the current research and enhanced in previous research using implicit measures.

A potential interpretation of the fourth unpredicted finding (i.e., effects of CS–US co-occurrence were unaffected by the delay between encoding and judgment) is that longer delays between encoding and judgment influence mere co-occurrence effects in multiple ways that have compensatory outcomes. On the one hand, longer delays may increase the likelihood of incomplete retrieval of stored relational information, which should decrease scores on the *R* parameter and increase scores on the *C* parameter. On the other hand, longer delays may lead to a general decay of stored information, which should decrease scores on both the *R* and the *C* parameter. Thus, although the two kinds of memory-decay effects should reduce the *R* parameter in a synergistic fashion, the two effects should compensate each other on the *C* parameter, leading to an overall null effect of temporal delay.

Together, these ad hoc assumptions reconcile single-process propositional accounts with the unpredicted effects of time during encoding, stimulus repetition, and delay between encoding and judgment. However, single-process propositional accounts seem difficult to reconcile with the obtained effect of time during judgment, thereby facing the same explanatory challenge as dual-process accounts.

Caveats

Although we consider the RCB model superior compared with earlier approaches that investigated effects of CS–US co-occurrence and CS–US relations on implicit and explicit measures (e.g., Gawronski et al., 2005; Hu et al., 2017; Moran & Bar-Anan, 2013; Zanon et al., 2014), we acknowledge some limitations of the current work. One important issue concerns the interpretation of significant and nonsignificant effects in the current series of studies. Although we deem the sample sizes in our studies sufficiently large to interpret null effects in traditional data analyses (see power analysis in the Introduction), we acknowledge the well-known asymmetry between significant and nonsignificant effects in null-hypothesis testing. A superior approach that does not suffer from this limitation is Bayesian analysis (Gelman et al., 2014), but there is currently no simple way of applying Bayesian analysis to comparisons of parameter estimates obtained with multinomial modeling. That being said, it is worth noting that the greatest challenge to both dual-process and single-process propositional theories is the statistically significant effect of time during judgment on the *C* parameter (Experiment 4). Different from the shared prediction that co-occurrence effects should decrease as a function of increasing time during judgment, co-occurrence effects were greater when participants had more time to respond than when they had less time to respond. Thus, even if the null effects of time during encoding, stimulus repetition, and temporal delay on the *C* parameter turn out to be false negatives, both accounts still have to explain why mere co-occurrence effects increased (rather than decreased) as a function of increasing time during judgment (assuming that the obtained effect of time during judgment is not a false positive).

Another important caveat is that multinomial models are not simply alternative tools to analyze data. Different from general-purpose tools that are agnostic about the content of the analyzed data (e.g., ANOVA), multinomial models combine statistical analyses with content-related theoretical assumptions about (a) the causes of observed response patterns and (b) the hierarchical structure of the proposed causes (Klauer, 2015). With regard to the causes of observed response patterns, the RCB model assumes that responses to the CSs are fully determined by (a) the valence of the US a given CS had been paired with (captured by the *C* parameter), (b) the valence implied by the observed relation to that US (captured by the *R* parameter), and (c) general response biases (captured by the *B* parameter). The RCB model does not capture the possibility that relational information influences responses independent of US valence. That is, information that a CS causes or prevents something is assumed to have no effect on evaluative responses to the CS over and above the valence inferred from the combination of relational information and US valence. Although this assumption seems plausible for the learning task in the current studies, it may not hold for other kinds of relational information. For example, relational information that a person likes or dislikes someone else has been found to influence evaluative responses, in that people who like others are preferred over people who dislike others independent of the valence of the liked or disliked individuals (Gawronski & Walther, 2008). Similarly, relational information that a person is liked or disliked by someone else has been found to influence evaluative responses, in that people who are liked are preferred over people who disliked independent of the valence of the individuals who like or dislike the target person (Gawronski et al., 2005). Thus, although the RCB model is well suited for studies on the effects of CS–US co-occurrence and CS–US relations when the relational information does not have an evaluative connotation by itself, the model is not suitable for cases in which the relational information may directly influence evaluative responses independent of the valence of the US.

With regard to the hierarchical structure of the proposed causes, the RCB model assumes that effects of CS–US relations dominate over the effects of CS–US co-occurrence. That is, CS–US co-occurrence is assumed to influence evaluative responses only when CS–US relations do not influence evaluative responses (see Figure 1). This assumption is based on the idea that, in the presence of information about causal relations between a CS and US, mere co-occurrence of a CS and a US loses informational value about the valence of the CS. Although this assumption may seem plausible, it is a theoretical hypothesis that is central to the hierarchical structure of the RCB model. Yet, there is no straightforward way to test this hypothesis, because the goodness-of-fit of the current model is identical the one of a model in which the relative positions of *R* and *C* are reversed. All combinatorically possible models have the same degrees of freedom and impose the same equality restrictions on the probabilities for showing a particular response on the four kinds of stimulus combinations. However, identical model fit does not imply that the estimated parameter values are equal, implying the possibility that a given manipulation can have different effects as a function of different model specifications. To investigate potential differences in the observed effects of processing conditions, we reran the reported analyses with a model in which the positions of the *R* and the *C* parameter were reversed. All of the reported effects replicated with the reversed model. Thus, although there is no straightforward way to test the hierarchical structure of parameters proposed by the RCB model, the reported findings are independent of the proposed structure.

Conclusion

Expanding on theoretical controversies regarding the effects of CS–US co-occurrence and CS–US relations on evaluative responses, the current work aimed to provide a deeper understanding of their underlying processes using a multinomial modeling approach. The findings obtained with the proposed RCB model suggest that (a) CS–US co-occurrence and CS–US relations jointly influence evaluative responses and (b) their respective effects have distinct functional properties. Although the predictions of dual-process and single-process theories are consistent with the moderating effects obtained for CS–US relations, both theories face challenges in predicting and explaining the impact of various processing conditions on the effect of CS–US co-occurrence. Future research using the RCB model may provide deeper insights into the mechanisms underlying effects of CS–US co-occurrence and CS–US relations by testing other a priori predictions of competing theories.

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Appendix

RCB Model Equations

Equations of the RCB model for the estimation of effects of CS–US relations (R), CS–US co-occurrence (C), and general response bias (B) in choice responses for CSs that cause or prevent a positive or negative US.

$$p(\text{yes}|\text{cause, positive}) = R + [(1-R) \times C] + [(1-R) \times (1-C) \times B]$$

$$p(\text{yes}|\text{cause, negative}) = (1-R) \times (1-C) \times B$$

$$p(\text{yes}|\text{prevent, positive}) = [(1-R) \times C] + [(1-R) \times (1-C) \times B]$$

$$p(\text{yes}|\text{prevent, negative}) = R + [(1-R) \times (1-C) \times B]$$

$$p(\text{no}|\text{cause, positive}) = (1-R) \times (1-C) \times (1-B)$$

$$p(\text{no}|\text{cause, negative}) = R + [(1-R) \times C] + [(1-R) \times (1-C) \times (1-B)]$$

$$p(\text{no}|\text{prevent, positive}) = R + [(1-R) \times (1-C) \times (1-B)]$$

$$p(\text{no}|\text{prevent, negative}) = [(1-R) \times C] + [(1-R) \times (1-C) \times (1-B)]$$

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