Attitudinal Effects of Stimulus Co-occurrence and Stimulus Relations: Paradoxical Effects of Cognitive Load

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The reported research was supported by National Science Foundation Grant # 1649900 to Bertram Gawronski. Any opinions, findings, and conclusions or recommendations expressed in this material are those of the authors and do not necessarily reflect the views of the National Science Foundation. The author is grateful to Jan De Houwer for valuable comments on an earlier version of this article. A very special thanks to Jasmine Alli, Sophia Birk, Sarah Carr, Calvin Chan, Trey Delafoose, Jennifer Guerrero, Renee Lahav, Keoni Mahilum, Brooke Peterson, Yocelyn Rivera, Victoria Salinas, Bonnie Stice, Paul Teas, Jonathan Teplitskiy, and Megan Vernell for their help in collecting the data.

Abstract

Research suggests that evaluations of an object can be jointly influenced by (1) the mere co-occurrence of the object with a pleasant or unpleasant stimulus (e.g., mere co-occurrence of object A and negative event B) and (2) the object's specific relation to the co-occurring stimulus (e.g., object A starts vs. stops negative event B). Three experiments investigated the impact of cognitive load during learning on the effects of stimulus co-occurrence and stimulus relations. Counter to the shared prediction of competing theories suggesting that effects of stimulus relations should be reduced by cognitive load during learning, effects of stimulus relations were greater (rather than smaller) under high-load compared to low-load conditions. Effects of stimulus co-occurrence were not significantly affected by cognitive load. The results are discussed in terms of theories suggesting that cognitive load can influence behavioral outcomes via strategic shifts in resource allocation in response to task-specific affordances.

Keywords: cognitive load; dual-process theory; evaluative conditioning; propositional learning; resource allocation

Research on evaluative conditioning (EC) suggests that, when a neutral conditioned stimulus (CS) repeatedly co-occurs with a positive or negative unconditioned stimulus (US), people will show an evaluative response to the CS that matches the valence of the US (for a meta-analysis, see Hofmann, De Houwer, Perugini, Bayens, & Crombez, 2010). For a long time, EC effects have been explained in terms of associative learning mechanisms, involving the automatic formation of mental associations in memory (e.g., Baeyens, Eelen, Crombez, & Van den Bergh, 1992; Martin & Levey, 1978). More recently, associative accounts have been challenged by theories that attribute EC effects to propositional learning mechanisms, involving the non-automatic formation of mental propositions about the relation between a CS and a co-occurring US (e.g., De Houwer, 2018; De Houwer, Van Dessel, & Moran, 2020). Integrating the central ideas of both accounts, dual-process accounts suggest that EC effects can be the result of either associative or propositional learning, with their respective contributions depending on the processing conditions during the encoding of CS-US pairings (e.g., Gawronski & Bodenhausen, 2011, 2018).

The current research was inspired by the dual-process hypothesis that evaluative responses to a CS can be jointly influenced by (1) its mere co-occurrence with a positive or negative US and (2) the CS's specific relation to the co-occurring US. In cases involving assimilative relations between a CS and a co-occurring US (e.g., object A starts negative event B), the two effects influence CS evaluations in the same direction. However, in cases involving contrastive relations between a CS and a co-occurring US (e.g., object A stops negative event B), the two effects influence CS evaluations in opposite directions (e.g., Heycke & Gawronski, 2020; Hu, Gawronski, & Balas, 2017; Kukken, Hütter, & Holland, 2020; Moran & Bar-Anan, 2013).[1] According to dual-process accounts, mere co-occurrence effects are driven by an associative learning mechanism, whereas effects of stimulus relations are driven by a propositional learning mechanism. Although joint effects of CS-US co-occurrence and CS-US relations can also be explained by single-process propositional accounts (for discussions, see Heycke & Gawronski, 2020; Van Dessel, Gawronski, & De Houwer, 2019), a unique assumption of dual-process accounts is that the two effects should have distinct functional properties, due to the presumed independence of their underlying mental processes (Gawronski & Bodenhausen, 2018).

Expanding on these ideas, the current research aimed to test the dual-process hypothesis that cognitive load during learning should selectively impair the non-automatic formation of mental propositions about CS-US relations without affecting the automatic formation of associations in memory (see Gawronski & Bodenhausen, 2014, 2018). These assumptions imply that cognitive load during learning should selectively reduce effects of CS-US relations without affecting effects of CS-US co-occurrence. These predictions were tested against alternative predictions derived from single-process propositional accounts, which reject the idea of automatic association formation (see De Houwer, 2018; De Houwer et al., 2020). According to single-process propositional accounts, mere co-occurrence effects in cases involving contrastive relations do not result from the automatic formation of associations during learning. Instead, such effects can be explained as the result of incomplete retrieval of stored propositional information during the expression of an evaluative response (e.g., retrieval of *A is related to B*

[1] The distinction between assimilative and contrastive relations subsumes a wide range of dichotomous or bipolar dimensions with one end-point reflecting opposition between the co-occurring stimuli. Examples include causality (e.g., A causes vs. prevents B), similarity (e.g., A is similar vs. dissimilar to B), or sentiments (e.g., A likes vs. dislikes B). For a detailed discussion of potential relations between co-occurring stimuli and their conceptual properties, see Hughes, Ye, Van Dessel, and De Houwer (2019).

instead of *A stops B*; see Van Dessel et al., 2019). Thus, to the extent that cognitive load during learning increases the likelihood of incomplete retrieval, cognitive load during learning should reduce effects of CS-US relations and increase effects of CS-US co-occurrence. Specifically, under conditions of low cognitive load during learning, retrieval of stored propositional information should be more likely to be complete, which should increase effects CS-US relations and decrease effects of CS-US co-occurrence. In contrast, under conditions of high cognitive load during learning, retrieval of stored propositional information should be more likely to be incomplete, which should decrease effects CS-US relations and increase effects of CS-US co-occurrence.

Counter to the shared prediction of the two accounts regarding the impact of cognitive load on the effect of CS-US relations, three experiments found that effects of CS-US relations were greater (rather than smaller) under conditions of high cognitive load compared to conditions of low cognitive load. Because the finding contradicts dominant theorizing about cognitive-load effects and the processing of stimulus relations, we discuss this finding in terms of its implications for how cognitive load may influence the strategic allocation of mental resources instead of treating it as evidence for or against a particular mental-process theory of EC. Yet, to provide sufficient background for the reported experiments, we will briefly review the available evidence regarding effects of cognitive load on EC and the impact of CS-US co-occurrence and CS-US relations.

Cognitive-Load Effects on EC

Challenging long-standing assumptions about the automaticity of EC effects, several studies showed that cognitive load during the encoding of simple CS-US pairings reduces the overall size of EC effects (e.g., Davies, El-Deredy, Zandstra, & Blanchette, 2012; Dedonder, Corneille, Yzerbyt, & Kuppens, 2010; Mierop, Hütter, & Corneille, 2017; Pleyers, Corneille, Yzerbyt, & Luminet, 2009; for a review, see Corneille & Stahl, 2019). These results are consistent with the hypothesis that EC effects can be product of a resource-dependent propositional learning mechanism (e.g., De Houwer, 2018; De Houwer et al., 2020). However, the obtained reduction of EC effects under cognitive load does not necessarily contradict the dual-process hypothesis that EC effects can also be the product of a resource-independent associative learning mechanism (e.g., Gawronski & Bodenhausen, 2011, 2018). As noted by Gawronski and Bodenhausen (2018), while significant differences across cognitive-load conditions are informative about the contribution of resource-dependent learning mechanisms, claims about the operation of additional resource-independent learning mechanisms have to be evaluated based on whether there is a residual EC effect under cognitive load. Moreover, to the extent that EC effects resulting from resource-independent mechanisms are relatively small, lack of a significant EC effect under cognitive load could be due to low statistical power rather than genuine non-existence of resource-independent EC. Consistent with these arguments, Mierop et al. (2017) report non-significant residual EC effects under cognitive-load conditions in three relatively low powered studies (Ns = 34, 41, 61, respectively), while an integrative analysis of the combined data from the three studies (N = 136) did obtain a significant residual EC effect under cognitive load. Thus, although there is clear evidence for the contribution of resource-dependent processes to EC (e.g., non-automatic generation of mental propositions), the available evidence against the role of resource-independent processes (e.g., automatic formation of mental associations) is still inconclusive.

CS-US Co-Occurrence and CS-US Relations

Expanding the focus from *operating conditions* (i.e., automatic vs. non-automatic) to *operating principles* (i.e., associative vs. propositional), some studies aimed to provide deeper insights into the mental processes underlying EC effects by investigating effects of CS-US co-occurrence and CS-US relations. Early studies suggested that joint effects of CS-US co-occurrence and CS-US relations could potentially be identified via dissociations on explicit and implicit measures (for a review of implicit measures, see Gawronski & De Houwer, 2014). Whereas evaluations captured by explicit measures (e.g., self-reported evaluative ratings) have been found to reliably reflect effects of CS-US relations, some studies found that evaluations captured by implicit measures (e.g., evaluative priming effects) reflected unqualified effects of mere CS-US co-occurrences (e.g., Hu et al., 2017, Experiments 1 and 2; Moran & Bar-Anan, 2013). For example, when participants were repeatedly presented with information that a pharmaceutical product prevents a negative health condition, they subsequently showed a positive response to the product on an explicit measure (reflecting the product's relation to the negative health condition) and a negative response on an implicit measures (reflecting the product's co-occurrence with the negative health condition; see Hu et al., 2017, Experiments 1 and 2).

While the described dissociation is consistent with the predictions of dual-process accounts (e.g., Gawronski & Bodenhausen, 2006, 2011, 2018), the full body of evidence regarding mere co-occurrence effects on implicit measures is rather mixed and inconclusive (for a review, see Kurdi & Dunham, 2020). While some studies found unqualified co-occurrence effects on implicit measures (e.g., Moran & Bar-Anan, 2013), other studies found attenuated co-occurrence effects on implicit measures in cases involving contrastive CS-US relations (e.g., Zanon, De Houwer, & Gast, 2012), while others found strong effects of CS-US relations and no effects of mere co-occurrence (e.g., Gawronski, Walther, & Blank, 2005, Experiment 1). In addition to the mixed evidence, a major interpretational problem in this line of research is that dissociations between explicit and implicit measures could be driven by processing differences during the measurement of evaluative responses rather than two distinct learning mechanisms (see Van Dessel et al., 2019). For example, based on the idea that implicit measures capture relatively fast responses and explicit measures typically provide more time for evaluations of a target object, dissociations between implicit and explicit measures may reflect differences in the retrieval of stored propositional information rather than two distinct learning mechanisms. Thus, in line with the assumptions of single-process propositional accounts (e.g., De Houwer, 2018; De Houwer et al., 2020), slow evaluations captured by explicit measures may be shaped by completely retrieved propositions (e.g., *A prevents B*), while fast evaluations captured by implicit measures may be driven by incompletely retrieved propositions (e.g., *A is related to B*).

To address these ambiguities, some researchers have utilized multinomial modeling (see Hütter & Klauer, 2016) to quantify the contributions of CS-US co-occurrence and CS-US relations to responses on a single task (e.g., Gawronski & Brannon, in press; Heycke & Gawronski, 2020; Kukken et al., 2020). Consistent with the idea that CS-US co-occurrence and CS-US relations can jointly influence evaluative responses, research using a multinomial modeling approach has found scores that reliably differed from a neutral baseline for both a model parameter capturing effects of CS-US co-occurrence and a model parameter capturing effects of CS-US relations. Expanding on these findings, several studies aimed to provide deeper insights into the contribution of learning-related and judgment-related processes to the effects of CS-US co-occurrence and CS-US relations by separately manipulating processing conditions during learning and the measurement of evaluative responses (e.g., Gawronski & Brannon, in press; Heycke & Gawronski, 2020).

The Current Research

An interesting question linking *operating conditions* (i.e., automatic vs. non-automatic) to *operating principles* (i.e., associative vs. propositional) is how cognitive load during learning affects the impact of CS-US co-occurrence and CS-US relations. According to dual-process accounts (e.g., Gawronski & Bodenhausen, 2018), effects of CS-US co-occurrence are driven by an associative learning mechanism involving the automatic formation of mental associations, while effects of CS-US relations are driven by a propositional learning mechanism involving the non-automatic formation of mental propositions about CS-US relations. From this perspective, cognitive load during learning should reduce the impact of CS-US relations without affecting the impact of CS-US co-occurrence. Single-process propositional accounts (e.g., De Houwer, 2018) reject the idea of automatic association formation and instead suggest that mere co-occurrence effects can result from incomplete retrieval of stored propositional information during the expression of evaluative responses (e.g., retrieval of *A is related to B* instead of *A prevents B*). Thus, to the extent that cognitive load during learning increases the likelihood of incomplete retrieval by impairing the storage of propositional information, cognitive load during learning should reduce the impact of CS-US relations and increase the impact of CS-US co-occurrence. The main goal of the current research was to test these competing predictions.

To this end, three experiments used a learning paradigm by Hu et al. (2017, Experiment 3) and Heycke and Gawronski's (2020) RCB model, a multinomial model to quantify effects of CS-US co-occurrence and CS-US relations on evaluative responses (see also Kukken et al., 2020). Participants were presented with pairings of pharmaceutical products (CS) and images of positive or negative health conditions (US). For half of the pairings, participants received information that the pharmaceutical product causes the depicted health condition. For the remaining half, participants received information that the pharmaceutical product prevents the depicted health condition. Participants' task was to form an impression of the pharmaceutical products based on the presented information. Afterwards, participants were presented with the pharmaceutical products one-by-one and asked to indicate whether or not they would choose the product (*yes* vs. *no*).

Applied to Hu et al.'s (2017) learning paradigm, Heycke and Gawronski's (2020) RCB model captures patterns of evaluative responses to four kinds of stimuli: (1) pharmaceutical products that cause positive health outcomes, (2) pharmaceutical products that cause negative health outcomes, (3) pharmaceutical products that counteract positive health outcomes, and (4) pharmaceutical products that counteract negative health outcomes (see Figure 1). Based on the observed responses to the four kinds of stimuli, the RCB model provides numerical estimates for the probabilities that (1) responses to the pharmaceutical products are driven by their relation to the depicted health outcomes (labeled *R*), (2) responses to the pharmaceutical products are driven by their mere co-occurrence with the depicted health outcomes (labeled *C*), and (3) responses to the pharmaceutical products reflect a general positivity or negativity bias regardless of their relation and co-occurrence with particular health outcomes (labeled *B*).

To investigate the impact of cognitive load on the effects of CS-US co-occurrence (captured by the RCB model's *C* parameter) and CS-US relations (captured by the RCB model's *R* parameter), participants were instructed to memorize a digit-string prior to the learning task, keep it in mind during the learning task, and reproduce it after the learning task. For one group of participants, the digit-string included a meaningless combination of eight letters, numbers, and symbols, rendering the memory task relatively difficult (i.e., high load). For a second group of participants, the digit-string included a simple combination of one letter and one number, rendering the memory task relatively easy (i.e., low load). Experiment 3 additionally included a third condition in which participants completed the learning task without having to memorize a digit-string.[2]

To investigate the impact of cognitive load on the effects of CS-US co-occurrence and CS-US relations, we conducted three experiments. Experiment 1 aimed to test the competing predictions of dual-process and single-process propositional accounts. Based on the unexpected results of Experiment 1, Experiment 2 served as a direct replication of Experiment 1. Experiment 3 aimed to replicate and extent the findings of the first two studies. In line with concerns about selective reporting of statistically significant effects (Ioannidis, Munafo, Fusar-Poli, Nosek, & David, 2014), we report the results of all three experiments regardless of their outcomes. For Experiments 1 and 2, we aimed to recruit 480 participants (i.e., 240 participants in each of two cognitive-load conditions), which provides a power of 80% in detecting a small difference of d = 0.26 between cognitive-load conditions in a traditional t-test for two independent groups (two-tailed).[3] For Experiment 3, we initially aimed to recruit 750 participants (i.e., 250 participants in each of three cognitive-load conditions), which provides a power of 80% in detecting a small effect of f = 0.11 in a one-way ANOVA with three independent groups (two-tailed). However, due to the Covid-19 pandemic, the lab in which the data were collected had to close on March

[2] Although control conditions without mental load are rather common in the literature, they are suboptimal for inferences regarding resource-dependence because they confound resource-dependence with goal-dependence (see Gast, Gawronski, & De Houwer, 2012). A superior approach that does not suffer from this ambiguity is to compare conditions of high vs. low cognitive load (e.g., rehearsal of two-digit vs. eight-digit string), as in the current studies (see also Gawronski, Armstrong, Conway, Friesdorf, & Hütter, 2017; Yzerbyt, Coull, & Rocher, 1999).
[3] Because power analyses within multinomial modeling require simulations with expected population values for the three parameters and any specific expectations in this regard would be arbitrary, we made our a priori sample-size decision in a heuristic fashion based on simple comparisons of mean values.

13, 2020 and the data collection for Experiment 3 had to be terminated early after the recruitment of 687 participants and valid data from 668 participants (see below). The final sample of 668 participants in Experiment 3 provides a power of 80% in detecting a small effect of $f = 0.12$ in a one-way ANOVA with three independent groups (two-tailed). 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/yx67j/?view_only=9e35ec87b7ab425ea8abf7d763a7c9c2. The studies were not formally preregistered.

Methods

Participants and Design

Experiments 1 and 2 included the same 2 (US Valence: positive vs. negative) × 2 (CS-US Relation: causes vs. prevents) × 2 (Cognitive Load: low vs. high) mixed design with the first two variables being manipulated within-subjects and the last one being manipulated between-subjects. Experiment 3 included an additional no-load condition in a 2 (US Valence: positive vs. negative) × 2 (CS-US Relation: causes vs. prevents) × 3 (Cognitive Load: no vs. low vs. high) mixed design with the first two variables being manipulated within-subjects and the last one being manipulated between-subjects.

For Experiment 1, we recruited 493 psychology undergraduates for a one-hour battery entitled "First Impressions" that included the current study and one unrelated study.[4] The current study was always completed as the second one in the battery. Participants received credit for a research participation requirement. Data from two participants were lost due to experimenter error, data from two participants were lost due to computer malfunctions, and data from three

[4] Due to excessive sign-ups at the end of the academic term, the sample size was slightly larger than the desired sample size of 480 participants.

participants who did not respond within the 1000 ms response deadline on more than 50% of the trials in the choice task were excluded from analyses, leaving us with valid data from 486 participants (356 women, 130 men).

For Experiment 2, we recruited 494 psychology undergraduates for a one-hour battery entitled "First Impressions" that included the current study and one unrelated study.[5] The current study was always completed as the second one in the battery. Participants received credit for a research participation requirement. Data from one participant were lost due to experimenter error, data from four participants were lost due to computer malfunctions, and data from one participant who did not respond within the 1000 ms response deadline on more than 50% of the trials in the choice task were excluded from analyses, leaving us with valid data from 488 participants (348 women, 140 men).

For Experiment 3, we recruited 687 psychology undergraduates for a one-hour battery entitled "Moral Judgment and Impression Formation" that included the current study and one unrelated study. The current study was always completed as the second one in the battery. Participants received credit for a research participation requirement. Data from eight participants were lost due to computer malfunctions, three participants left the lab before completing the study, and data from eight participants who did not respond within the 1000 ms response deadline on more than 50% of the trials in the choice task were excluded from analyses, leaving us with valid data from 668 participants (454 women, 214 men).

Learning Task

Participants in all three experiments completed the same learning task, which was directly adapted from Heycke and Gawronski (2020). The task included information about

[5] Due to excessive sign-ups at the end of the academic term, the sample size was slightly larger than the desired sample size of 480 participants.

whether pharmaceutical products cause or prevent either healthy or unhealthy physical conditions. The stimuli in the task included 12 images of hypothetical pharmaceutical products, 6 images of healthy physical conditions (e.g., voluminous hair), and 6 images of unhealthy physical conditions (e.g., tooth decay). On each trial of the task, an image of a pharmaceutical product (CS) was presented on the left and an image of a healthy or unhealthy physical condition (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 inter-trial interval of 1000 ms. Three CSs were presented with a positive US and the relational qualifier *causes*; three CSs were presented with a negative US and the relational qualifier *causes*; three CSs were presented with a positive US and the relational qualifier *prevents*; and three CSs were presented with a negative US and the relational qualifier *prevents*. The use of a given CS for pairings with positive versus negative USs and the relational qualifiers *causes* versus *prevents* was counterbalanced by means of a Latin square. The learning phase consisted of 4 blocks with self-paced breaks between blocks. Within each block, each CS-US-qualifier combination was presented twice, summing up to 8 presentations of each stimulus combination over the four blocks. For each participant, a given CS was always presented together with the same US. With 12 unique CS-US-qualifier combinations and 8 presentations of each CS-US-qualifier combination, the learning task included a total of 96 trials. Following Heycke and Gawronski (2020), participants received the following instructions for the learning task:

> *The next part of this study is concerned with how people process information about consumer products. For this purpose, 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.

Cognitive Load Manipulation

After the basic instructions for the learning task, participants were provided with the following instructions for the manipulation of cognitive load:

In the current study, we are interested in how the processing of such information is influenced by mental distraction. Toward this end, you will be asked to memorize a digit-string and rehearse it during the presentation of the information about the consumer products. Please memorize the following string of digits. You will be asked to repeat it at the end of this task. It is VERY IMPORTANT that you keep this string in mind throughout the entire task until you are asked to report it.

Participants in the low-load condition were then presented with a simple two-digit string that included one letter and one number (*h7*). Participants in the high-load condition were presented with a complex eight-digit string that included a meaningless combination of letters, numbers, and symbols (*h7%r5K$3*). After the presentation of the digit-string. Participants were instructed to rehearse it, keep it in mind during the impression formation task, and to think of the presented image pairs in terms of the relation presented on the screen. After completion of the learning task, participants were asked to type the digit-string they were asked to memorize into a text box. Participants in the no-load condition of Experiment 3 received only the basic instructions for the learning task without being instructed to memorize a digit-string.

Measures

Choice task. After the learning task, participants in all three experiments completed a speeded choice task in which they were asked to indicate whether they would choose a given product (see Heycke & Gawronski, 2020). 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. Participants were asked to press a left-hand key (*A*) if their answer was *no* and a right-hand key (*Numpad 5*) if their answer was *yes*. If participants did not respond within the 1000 ms response window, they were presented with the message *Please try to respond faster!* for 1000 ms. Only valid responses within the 1000 ms response window were used in the analysis. Each trial started with a blank screen for 500 ms, followed by a fixation cross for 500 ms. During the 1000 ms presentation of a given CS, labels for the two response options (*no* vs. *yes*) were displayed on the bottom-left side and the bottom-right side 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.

Manipulation check. To test the effectiveness of the cognitive-load manipulation, participants in the high-load and the low-load conditions were asked to indicate how difficult it was to keep the digit-string in mind during the learning task. Responses were recorded with a 7-point rating scale ranging from 1 (*very easy*) to 7 (*very difficult*).

RCB Model

Because the mathematical underpinnings of the RCB model are explained in detail by Heycke and Gawronski (2020), we will only summarize the basic steps in analyzing data with the model. Based on the processing tree depicted in Figure 1, the RCB model provides four non-redundant mathematical equations to estimate numerical values for the three model parameters (*R*, *C*, *B*) based on the empirically observed probabilities of a *positive* (i.e., *yes*) versus *negative* (i.e., *no*) response to the four types of stimuli (see Appendix). These equations include the three model parameters as unknowns and the empirically observed probabilities of *positive* versus *negative* responses to the four types of stimuli as known numerical values. Using maximum likelihood statistics, multinomial modeling generates parameter estimates for the three unknowns that minimize the difference between the empirically observed probabilities of positive versus negative responses to the four types of stimuli and the probabilities of positive versus negative responses predicted by the model equations using the generated parameter estimates. The adequacy of the model in describing the data can be evaluated by means of goodness-of-fit statistics, such that poor model fit would be reflected in a statistically significant discrepancy between the empirically observed probabilities in a given data set and the probabilities predicted by the model for this data set. Differences in parameter estimates across groups 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. RCB model analyses were conducted with the free software multiTree v0.43 (Moshagen, 2010) and the multiTree template files for RCB model analyses provided by Heycke and Gawronski (2020).

Results

Manipulation Checks

Supporting the intended effect of the cognitive-load manipulation, participants in the high-load condition found it more difficult to keep the digit-string in mind than participants in the low-load condition. This difference was reflected in a significant effect of cognitive load in Experiment 1 (Ms = 2.15 vs. 3.48, respectively), $t(472.92) = 8.71$, $p < .001$, $d = 0.790$, Experiment 2, (Ms = 2.01 vs. 3.31, respectively), $t(465.43) = 9.21$, $p < .001$, $d = 0.835$, and Experiment 3 (Ms = 1.91 vs. 3.31, respectively), $t(398.31) = 9.29$, $p < .001$, $d = 0.875$.

RCB Model

Mean proportions and 95% confidence intervals of *yes* vs. *no* responses to the four kinds of stimuli as a function of cognitive-load conditions are presented in Table 1. The RCB model was fit to the data of each experiment with the three model parameters varying freely across cognitive-load conditions. Although the RCB model has shown adequate fit in numerous prior studies using the same learning and choice tasks (e.g., Gawronski & Brannon, in press; Heycke & Gawronski, 2020), model fit was acceptable only in Experiment 3, $G^2(3) = 6.21$, $p = .102$, $w = .017$, but suboptimal in Experiment 1, $G^2(2) = 5.92$, $p = .052$, $w = .019$, and Experiment 2, $G^2(2) = 6.59$, $p = .037$, $w = .020$. Because large sample sizes increase the likelihood of significant discrepancies between actual and predicted response probabilities, and the effect sizes of the observed discrepancies all fell far below Cohen's (1988) benchmark for a small effect (w = .10), we nevertheless tested whether the obtained estimates for the three parameters were significantly different across conditions. Parameter estimates obtained in the three experiments are presented in Table 2.

In Experiment 1, analyses revealed a significant effect of Cognitive Load on the *R* parameter, $\Delta G^2(1) = 7.89$, $p = .005$, $w = .022$, indicating that CS-US relations had a greater impact on participants' choices in the high-load condition compared to the low-load condition. There was no significant effect of Cognitive Load on the *C* parameter, $\Delta G^2(1) = 0.39$, $p = .535$, $w = .005$, and the *B* parameter, $\Delta G^2(1) < 0.01$, $p = .983$, $w < .001$.

In Experiment 2, analyses revealed a marginal effect of Cognitive Load on the *R* parameter, $\Delta G^2(1) = 3.75$, $p = .053$, $w = .015$, indicating that CS-US relations tended to have a greater impact on participants' choices in the high-load condition compared to the low-load condition. There was also a significant effect of Cognitive Load on the *B* parameter, $\Delta G^2(1) = 4.41$, $p = .036$, $w = .017$, indicating a greater tendency to reject all products in the high-load condition compared to the low-load condition. There was no significant effect of Cognitive Load on the *C* parameter, $\Delta G^2(1) < 0.01$, $p = .966$, $w < .001$.

In Experiment 3, Cognitive Load showed a significant effect on the *R* parameter, $\Delta G^2(2) = 18.30$, $p < .001$, $w = .030$, but not the *C* parameter, $\Delta G^2(2) = 4.62$, $p = .099$, $w = .015$, and the *B* parameter, $\Delta G^2(2) = 2.23$, $p = .327$, $w = .010$. Further analyses with the *R* parameter revealed that CS-US relations had a weaker impact in the low-load condition compared to both the high-load condition, $\Delta G^2(1) = 15.42$, $p < .001$, $w = .027$, and the no-load condition, $\Delta G^2(1) = 11.77$, $p < .001$, $w = .024$. The impact of CS-US relations did not significantly differ across high-load and no-load conditions, $\Delta G^2(1) = 0.20$, $p = .651$, $w = .003$.

Integrative Data Analysis

Although the sample sizes in the three individual studies were relatively large and the unexpected effect of cognitive load on the *R* parameter replicated across studies, a potential question is whether this unexpected effect remains reliable in an integrative analysis that includes the data from all three experiments (see Curran & Hussong, 2009). A related question is whether the sample sizes in three individual studies were insufficient to detect small effects of cognitive load that might be detected in a larger sample (e.g., a small effect on the *C* parameter). To address these questions, we combined the data of the low-load and high-load conditions from the three experiments ($N = 1424$) and investigated differences in the three RCB parameters across conditions in the combined sample.

Although model fit was suboptimal in the combined sample, $G^2(2) = 16.19$, $p < .001$, $w = .019$, the effect size of the observed discrepancy again fell far below Cohen's (1988) benchmark for a small effect ($w = .10$). We therefore moved on to test whether the obtained estimates for the RCB model parameters were significantly different across conditions (see Figure 2). Consistent with the main finding of the three individual experiments, Cognitive Load showed a significant effect on the *R* parameter, $\Delta G^2(1) = 24.28$, $p < .001$, $w = .023$, indicating that CS-US relations had a greater impact under high-load compared to low-load conditions. Despite the greater statistical power for the detection of very small effects, there was no significant effect of Cognitive Load on the *C* parameter, $\Delta G^2(1) = 0.49$, $p = .482$, $w = .003$, and the *B* parameter, $\Delta G^2(1) = 1.97$, $p = .161$, $w = .007$.

A potential concern about the reported findings is that the RCB model showed suboptimal fit in two of the three experiments as well as the integrative data analysis, raising questions about the interpretability of the obtained effect of Cognitive Load on the *R* parameter.

A related concern is that, like any multinomial model, the RCB model is based on a number of background assumptions and violations of these assumptions could potentially undermine the interpretation of findings obtained with the RCB model (see Hütter & Klauer, 2016). To address these concerns, we also analyzed the combined data by submitting the proportions of *yes* (vs. *no*) responses to a 2 (US Valence: positive vs. negative) × 2 (CS-US Relation: causes vs. prevents) × 2 (Cognitive Load: low vs. high) mixed ANOVA with the first two variables as within-subjects factors and the last one as a between-subjects factor. Although not statistically identical, the *R* parameter of the RCB model conceptually corresponds to the two-way interaction between US Valence and CS-US Relation in the ANOVA, reflecting a response pattern consistent with the presumed impact of CS-US relations (see first row in Figure 1). Thus, if the obtained effect of Cognitive Load on the *R* parameter reflects a reliable difference in the impact of CS-US relations, the ANOVA should reveal a significant three-way interaction between US Valence, CS-US Relation, and Cognitive Load, such that the two-way interaction between US Valence and CS-US Relation is more pronounced under high-load compared to low-load conditions.

The ANOVA revealed a significant main effect of US Valence, $F(1, 1422) = 325.11$, $p < .001$, $\eta_p^2 = .186$, a significant main effect of CS-US Relation, $F(1, 1422) = 12.38$, $p < .001$, $\eta_p^2 = .009$, and a significant two-way interaction between US Valence and CS-US Relation, $F(1, 1422) = 1151.41$, $p < .001$, $\eta_p^2 = .447$, which were qualified by the predicted three-way interaction between US Valence, CS-US Relation, and Cognitive Load, $F(1, 1422) = 6.05$, $p = .014$, $\eta_p^2 = .004$. Further analyses revealed that the two-way interaction between US Valence and CS-US Relation was more pronounced under high-load conditions, $F(1, 711) = 619.32$, $p < .001$, $\eta_p^2 = .466$, compared to low-load conditions, $F(1, 711) = 532.10$, $p < .001$, $\eta_p^2 = .428$ (see Figure 3).

These results support the conclusion that Cognitive Load increased (rather than decreased) the impact of CS-US relations.

Discussion

Counter to the shared prediction of dual-process and single-process propositional accounts that cognitive load during learning should reduce the impact of CS-US relations on CS evaluations, effects of CS-US relations were greater (rather than smaller) under conditions of high cognitive load compared to conditions of low cognitive load. Although this unexpected effect was relatively small overall, it replicated across three individual studies and in an integrative analysis of the data from all three studies. The effect also emerged regardless of whether effects of CS-US relations were quantified via multinomial modeling or analyzed using standard ANOVA.

Potential Explanations

One potential conclusion from this unexpected effect is that the propositional learning mechanism claimed to underlie effects of CS-US relations is highly efficient, questioning the common assumption that propositional learning depends on the amount of available cognitive resources. However, it is worth noting that such a conclusion would suggest a null effect of cognitive load on the impact of CS-US relations. It does not explain why effects of CS-US relations were greater under high-load compared to low-load conditions. This difference cannot be explained with the simple assumption of resource-independence (which implies no difference between load conditions), but instead requires additional assumptions about how greater cognitive load can increase the impact of CS-US relations.

A more plausible explanation could be derived from recent theories suggesting that experimental procedures that have traditionally been interpreted as direct manipulations of mental resources may influence behavioral outcomes via strategic shifts in the allocation of mental resources (e.g., Inzlicht, Schmeichel, & Macrae, 2014). Applied to the current manipulation of cognitive load, the requirement to memorize a highly complex digit-string may influence effects of CS-US relations, not by influencing the amount of residual resources for the encoding of CS-US relations, but by leading to a strategic shift in the allocation of mental resources to the encoding of CS-US relations. Based on the meta-cognitive assumption that having to memorize a highly complex digit-string might interfere with the focal task of forming impressions based on CS-US relations, participants may decide to allocate greater mental effort to the encoding of CS-US relations to compensate for the presumed processing impairments. In contrast, participants asked to memorize a relatively simple digit-string may not assume any such impairments, and therefore not increase the allocation of resources for the focal task. Consistent with this interpretation, effects of CS-US relations were significantly weaker under conditions of low cognitive load where participants had to memorize a relatively simple two-digit string compared to both (1) conditions of high load where participants had to memorize a relatively complex eight-digit string and (2) conditions of no load where participants were not asked to memorize to memorize any digit-string. Although interpretations of differences between load and no-load conditions are somewhat difficult due to the confound between cognitive load and processing goals (see Gast et al., 2012), the obtained pattern in Experiment 3 is consistent with the idea that (1) having to memorize a simple two-digit string impaired the processing of CS-US relations in the low-load condition compared to the no-load condition and (2) participants did not assume any such impairments and therefore did not allocate greater effort to the focal task of encoding CS-US relations. Moreover, the outcome in the high-load condition can be explained by the assumptions that (3) having to memorize a complex eight-digit string impaired the processing of CS-US relations in the high-load condition and (4) participants overcompensated for these impairments by allocating greater effort to the focal task of encoding CS-US relations. Together, these assumptions imply that effects of CS-US relations should be weaker under low-load compared to both high-load and no-load conditions, as found in Experiment 3. Although these assumptions are admittedly post-hoc, future research may help to provide deeper insights into the processes underlying the obtained results by independently manipulating (1) incremental levels of secondary task demands and (2) strategic allocation of mental resources (e.g., via instructions and performance incentives). Combined with (1) a measure of meta-cognitive beliefs about processing demands and (2) a measure of resource allocation that is independent of participants' performance on the focal task, such research may provide further evidence for the interactive role of processing resources and resource allocation in dual-task paradigms.[6]

Implications

The proposed explanation in terms of strategic resource allocation not only accounts for the unexpected effect of cognitive load on the impact of CS-US relations; it also reconciles this finding with the core assumptions of dual-process and single-process propositional accounts. Both accounts assume that effects of CS-US relations are driven by a propositional learning mechanism, involving the non-automatic formation of mental propositions about the relation between a CS and a co-occurring US. This shared hypothesis led to the prediction that cognitive load should reduce effects of CS-US relations. However, to the extent that high cognitive load

[6] A potential alternative explanation is that participants in the high-load condition may have tried to simplify the focal task by drawing abstract evaluative inferences about the products (e.g., X is good) instead of learning specific information about the products (e.g., X prevents something bad). We deem this interpretation insufficient for two reasons. First, it presupposes that drawing abstract evaluative inferences requires less mental resources than memorizing the specific information, which conflicts with recent evidence suggesting the opposite (Gawronski, Luke, & Ng, 2021). Second, given that abstract evaluative inferences seems to require more (rather than less) resources than memorizing specific information (see Gawronski et al., 2021), one would have to make additional assumptions to explain why effects of CS-US relations were greater under high-load compared to low-load conditions (e.g., overcompensation via strategic shifts in resource allocation).

leads to a strategic shift in resource allocation (see above), both theories would suggest that effects of CS-US relations should be greater under high load compared to low load, reconciling the two accounts with the unexpected finding in the current studies. Nevertheless, the two accounts still lead to different predictions about how strategic shifts in resource allocation should influence effects of CS-US co-occurrence. Dual-process accounts assume that mere co-occurrence effects result from an associative learning mechanism of automatic link formation, which is claimed to be independent of mental resources. From this perspective, effects of CS-US co-occurrence should be unaffected by strategic resource allocation, consistent with the obtained null effect of cognitive load on the impact of CS-US co-occurrence. In contrast, single-process propositional accounts suggest that mere co-occurrence effects can result from incomplete retrieval of stored propositional information, which should become less likely with increasing amounts of resources allocated during learning. Thus, if the unexpected impact of cognitive load of the effects of CS-US relations is driven by strategic shifts in resource allocation, effects of CS-US co-occurrence should be smaller under high-load compared to low-load conditions, which is inconsistent with the obtained null effect of cognitive load on the impact of CS-US co-occurrence. Together, these considerations suggest that, although the unexpected effect of cognitive load on the impact of CS-US relations can be reconciled with the two accounts via additional assumptions about strategic resource allocation, the pattern of obtained for CS-US co-occurrence is easier to reconcile with dual-process compared to single-process propositional accounts.

Although the current research focused exclusively on effects of CS-US co-occurrence and CS-US relations, the unexpected effect of cognitive load has important implications for research on the resource-dependence of mental processes more broadly (see Bargh, 1994; Moors

& De Houwer, 2006). To the extent that the above interpretation in terms of strategic resource allocation is correct, it suggests a fundamental ambiguity in the interpretation of data patterns in studies on the efficiency of mental processes. Specifically, it suggests that null effects of traditional manipulations of cognitive load (e.g., dual-task paradigms) should not be interpreted as evidence for the resource-independence of the process underlying a focal effect. After all, it is possible that the process does require a considerable amount of resources, but the processing impairments resulting from the manipulation are compensated by a strategic shift in the allocation of mental resources. In this case, it would be ill-founded to infer that the underlying process is automatic in terms of the efficiency criterion (see Bargh, 1994; Moors & De Houwer, 2006). Thus, although significant effects of traditional manipulations of cognitive load can help to demonstrate resource-dependence, null effects of such manipulations are insufficient to demonstrate resource-independence, even if the reliability of such null effects is confirmed by studies with large sample sizes and advanced statistical tools, such as Bayesian analyses (Morey & Rouder, 2011) or equivalence tests (Lakens, Scheel, & Isager, 2018).

At the theoretical level, these considerations echo broader concerns that experimental manipulations should be described in terms of operational differences in environmental conditions rather than mental constructs (De Houwer, 2011; De Houwer, Gawronski, & Barnes-Holmes, 2013; Gawronski & Bodenhausen, 2015). Following the modal practice in the field, the current research was based on the assumption that dual-task manipulations such as the concurrent memory task in the current studies influence the amount of residual resources that are available for a focal task. Although the terms *no load*, *low load* and *high load* could be interpreted as referring to environmental task affordances, these term are often interpreted at the mental level, suggesting that they reflect differences in the amounts of residual resources (for a discussion, see De Houwer & Moors, 2012). However, differences in residual resources may be just one mental factor that mediates effects of environmental task affordances on behavioral outcomes. Another important factor might be strategic resource allocation, which is ignored when dual-task manipulations are described in terms a specific mental construct (e.g., available residual resources) instead of environmental conditions (e.g., environmental task affordances).

Limitations

Although the unexpected effect of cognitive load replicated across studies and data analytic approaches, it is worth noting that the effect was very small overall. Moreover, although mean difficulty ratings in the manipulation checks were significantly different across cognitive-load conditions, the observed scores suggest that participants in the high-load condition found the memory-task only moderately difficult. Hence, it is possible that strategic shifts in resource allocation can compensate for performance impairments caused by concurrent tasks only to a level of moderate load, with higher levels of load showing the typically expected performance impairments. These considerations suggest potential limits in the generality of the obtained results, in that they may not replicate with higher levels of cognitive load. Similar caveats seem in order for generalizations across populations, in that the current studies were conducted with psychology undergraduates, who might have larger working memory capacity compared to other groups of potential participants (see Wilhelm, Hildebrandt, & Oberauer, 2013). To the extent that compensatory effects of strategic resource allocation require a minimum amount of working memory capacity, the obtained results may not replicate in populations with lower working memory capacity. Finally, it is worth noting that all three experiments used the same task and the same set of stimuli, calling for conceptual replications with different tasks and stimuli.

Conclusion

The original aim of the current research was to test conflicting predictions of dual-process and single-process propositional accounts regarding the impact of cognitive load on the effects of mere CS-US co-occurrence when there is a clear assimilative vs. contrastive relation between the CS and the US. This endeavor led to the unexpected discovery that cognitive load increased effects of CS-US relations, a finding that conflicts with a shared prediction of both accounts suggesting that cognitive load should decrease effects of CS-US relations. The apparent conflict can be reconciled via theories suggesting that cognitive-load manipulations can influence behavioral outcomes via strategic shifts in the allocation of mental resources, which can lead to seemingly paradoxical outcomes like the one obtained in the current studies. These findings have important implications not only for research on EC but also for the broader field of automaticity research, demanding caution in the interpretation of cognitive-load effects.

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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 evaluative responses to stimuli (CS) that cause or prevent a positive or negative stimulus (US). Adapted from Heycke and Gawronski (2020). Reprinted with permission.

p(positive response | causes positive) = $R + [(1 - R) \times C] + [(1 - R) \times (1 - C) \times B]$

p(positive response | causes negative) = $(1 - R) \times (1 - C) \times B$

p(positive response | prevents positive) = $[(1 - R) \times C] + [(1 - R) \times (1 - C) \times B]$

p(positive response | prevents negative) = $R + [(1 - R) \times (1 - C) \times B]$

p(negative response | causes positive) = $(1 - R) \times (1 - C) \times (1 - B)$

p(negative response | causes negative) = $R + [(1 - R) \times C] + [(1 - R) \times (1 - C) \times (1 - B)]$

p(negative response | prevents positive) = $R + [(1 - R) \times (1 - C) \times (1 - B)]$

p(negative response | prevents negative) = $[(1 - R) \times C] + [(1 - R) \times (1 - C) \times (1 - B)]$

Table 1. Mean proportions and 95% confidence intervals of choice responses (yes vs. no) as a function of valence of co-occurring stimulus (positive vs. negative), relation to co-occurring stimulus (stimulus causes vs. prevents co-occurring stimulus), and cognitive load (low load vs. high load vs. no load), Experiments 1-3.

	Positive Co-Occurring Stimulus				Negative Co-Occurring Stimulus			
	Stimulus Causes Co-Occurring Stimulus		Stimulus Prevents Co-Occurring Stimulus		Stimulus Causes Co-Occurring Stimulus		Stimulus Prevents Co-Occurring Stimulus	
	M	95% CI	*M*	95% CI	*M*	95% CI	*M*	95% CI
Experiment 1								
low load	.68	[.65, .71]	.38	[.35, .41]	.29	[.26, .32]	.56	[.52, .59]
high load	.71	[.68, .74]	.36	[.33, .39]	.28	[.25, .31]	.58	[.54, .61]
Experiment 2								
low load	.69	[.66, .72]	.38	[.35, .41]	.29	[.26, .32]	.55	[.52, .59]
high load	.68	[.65, .72]	.36	[.32, .39]	.26	[.23, .29]	.56	[.52, .59]
Experiment 3								
low load	.65	[.62, .69]	.41	[.37, .44]	.30	[.27, .34]	.51	[.47, .55]
high load	.66	[.62, .69]	.36	[.32, .39]	.29	[.25, .32]	.56	[.53, .60]
no load	.65	[.62, .69]	.35	[.32, .39]	.28	[.25, .32]	.55	[.51, .59]

Table 2. Parameter estimates without model restrictions as a function of cognitive load (low load vs. high load vs. no load).

	Experiment 1		Experiment 2		Experiment 3	
Parameter	Estimate	95% CI	Estimate	95% CI	Estimate	95% CI
R						
low load	0.29	[0.27 - 0.31]	0.29	[0.27 - 0.31]	0.23	[0.21 - 0.25]
high load	0.33	[0.31 - 0.35]	0.32	[0.30 - 0.34]	0.30	[0.27 - 0.32]
no load	-	-	-	-	0.29	[0.27 - 0.31]
C						
low load	0.15	[0.12 - 0.18]	0.16	[0.13 - 0.19]	0.16	[0.13 - 0.19]
high load	0.16	[0.13 - 0.20]	0.16	[0.13 - 0.19]	0.11	[0.08 - 0.14]
no load	-	-	-	-	0.12	[0.09 - 0.15]
B						
low load	0.46	[0.45 - 0.48]	0.47	[0.45 - 0.48]	0.45	[0.44 - 0.47]
high load	0.46	[0.45 - 0.48]	0.44	[0.42 - 0.46]	0.45	[0.43 - 0.47]
no load	-	-	-	-	0.44	[0.42 - 0.45]

Note. The *R* parameter captures effects of stimulus relations; the *C* parameter captures effects of stimulus co-occurrence; the *B* parameter captures general response biases. The neutral reference point for *R* and *C* is 0; the neutral reference point for *B* is 0.5, with scores higher than 0.5 reflecting a general bias toward positive responses and scores lower than 0.5 reflecting a general bias toward negative responses.

Figure 1. Multinomial processing tree depicting effects of CS-US relations, CS-US co-occurrence, and general response biases on evaluative responses (positive vs. negative) for stimuli that cause or prevent either positive or negative stimuli. Adapted from Heycke and Gawronski (2020). Reprinted with permission.

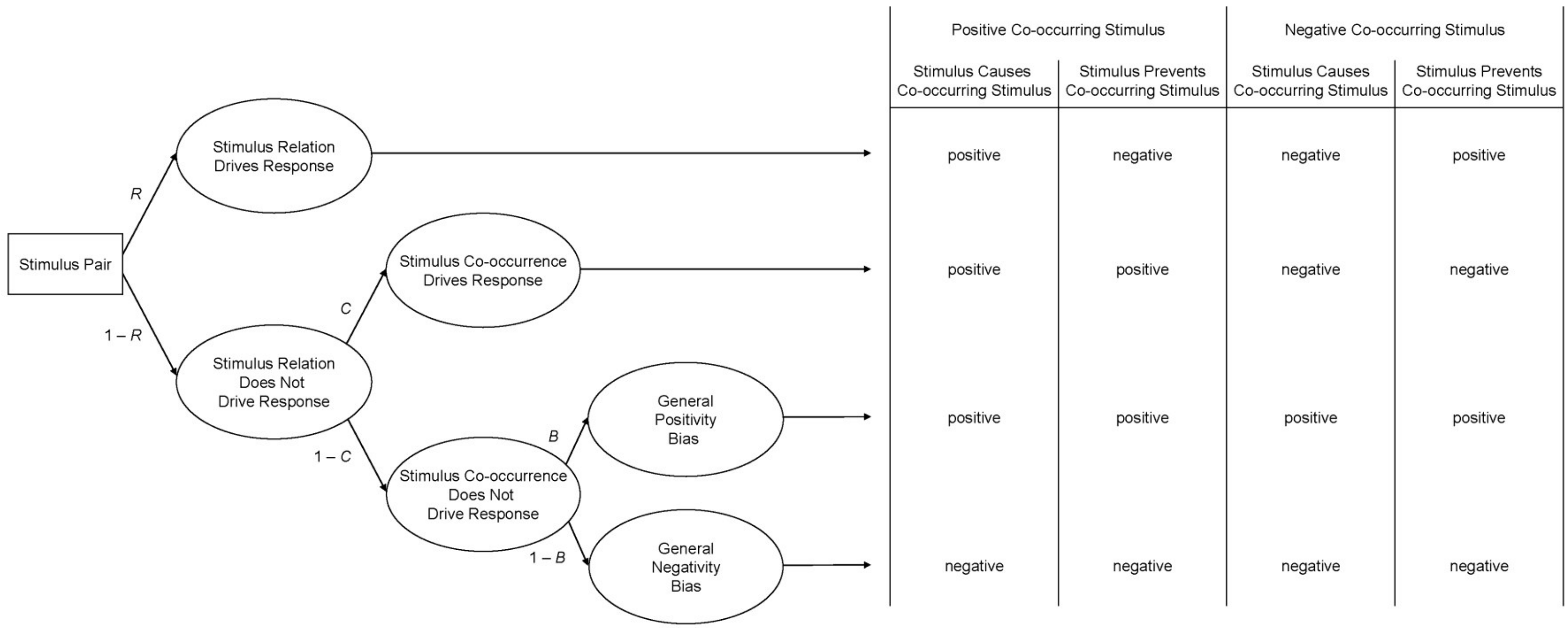
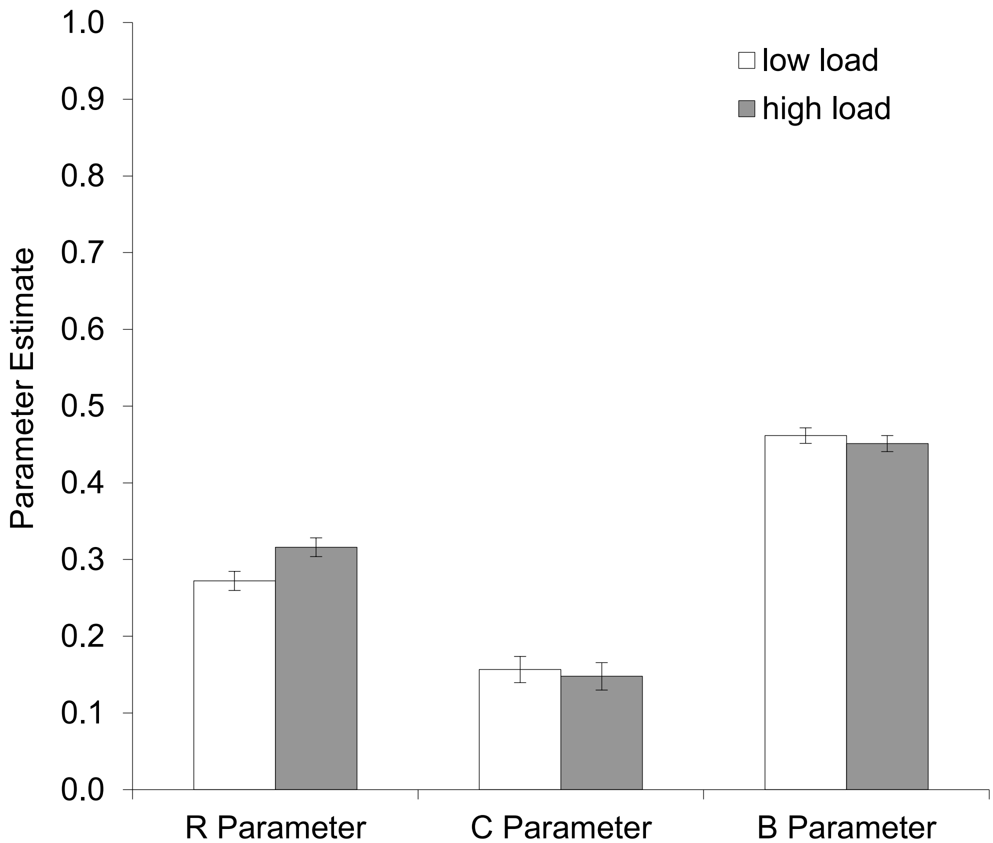


Figure 2. Parameter estimates without model restrictions as a function of cognitive load (low load vs. high load), combined data from Experiments 1-3.



Note. The *R* parameter captures effects of stimulus relations; the *C* parameter captures effects of stimulus co-occurrence; the *B* parameter captures general response biases. The neutral reference point for *R* and *C* is 0; the neutral reference point for *B* is 0.5, with scores higher than 0.5 reflecting a general bias toward positive responses and scores lower than 0.5 reflecting a general bias toward negative responses. Error bars depict 95% confidence intervals.

Figure 3. Mean proportions and 95% confidence intervals of choice responses (yes vs. no) as a function of valence of co-occurring stimulus (positive vs. negative), relation to co-occurring stimulus (stimulus causes vs. prevents co-occurring stimulus), and cognitive load (low load vs. high load), combined data from Experiments 1-3.